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PHILOSOPHY OF SCIENCE AND SOCIOLOGY OF SCIENCE : TOWARDS A CREATIVE INTERACTION

This paper begins by looking at the recent controversy over the question whether science as an activity can be explained in purely sociological terms. After briefly elucidating the problem in section I and setting certain records straight in section II, the paper, in section III, suggests a new orientation for philosophy of science such that both philosophy of science and sociology of science can extricate themselves from the stalemate that characterises the present debate and interact meaningfully at a new level and within a new framework constituted by hitherto unasked or neglected questions. In conceiving and advocating such a framework the paper takes its main clue from a problem Kuhn raises at the end of his magnum opus *The Structure of Scientific Revolutions*. Needless to say, since the purpose of the paper is more constructive than critical the line of thinking is more rhizomatic than deductive.

I

The rival parties involved in the controversy are characterised as 'rationalists' whose views have dominated philosophy of science till recently and 'sociologists of science' (or better, 'Sociologicists') who have challenged the hold of the former. The central tenet of rationalism is that the 'essence' of science consists in its adoption of a method whose elucidation and articulation is the task of philosophy of science in performing

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which it provides an adequate account of the nature of scientific activity. Because scientists use that method disinterestedly and rigorously their community is rationality personified and institutionalized. In short, the central slogan of the rationalist camp is ' The Method is the Message. ' Hence, ' rationalism ' is also called ' methodolgism. '

As against methodolgism, sociologism maintains that since science, like any other human activity, is a social phenomenon, its social determination is constitutive of its essence and therefore, the terms of its adequate account must be primarily sociological. Mainly due to the loosening of the hold of methodolgists consequent upon the work of Kuhn and Feyerabend, sociologism has come out recently in its full force. Its most effective spokesmen, Bloor and Barnes, who lead the Edinberg school of sociology of science, have propounded what is called ' Strong Programme ' in sociology of science. One of the central tenets of the strong programme is that both true and false scientific theories, both rational and irrational scientlfic beliefs are equally amenable to sociological explanation. The explanation of the false and/or the irrational in science is symmetrical with that of the true and/or the rational. In so far as the symmetrical explanation is possible, the very distinction between true and or rational scientific beliefs on the one hand and false and/or irrational scientific beliefs on the other, becomes questionable and even spurious. And in so far as Philosophy of Science as methodolgy of science is parasitic upon such a distinction its very identity and authenticity as an autonomous discipline becomes questionable.

It is precisely this contention that sets in motion the controversy between methodologism and sociologism. The methodologists who quite rightly see a threat to the very autonomy of their discipline reject the contention that the whole of science is

amenable to sociological treatment. They leave for the sociologists of science only those cases which deviate from the path, legitimated by the canons of scientific method. They assert, as Laudan contends, what is normal (i.e., rational) needs no explanation, including sociological; only the aberrations need explanation, possibly sociological one. [1] Of course, Laudan concedes to the sociologists of science "***exploration of the social determinants of problem*** – weighting, since that phenomenon... seems intuitively to be subject to pressures of class, nationality, finance and other social institutions." [2] It should be noted that Laudan affords to concede this since for him the selection of problems is of peripheral significance to science, the locus of whose rationality lies not in the problems selected but in the way in which it successfully solves them. However, Lakatos, another prominent methodologist, goes further and claims that the rationalist will ultimately be able to reclaim the ground he has surrendered to the sociologist of science once he develops a better theory of rationality. [3]

Offended by the attempts of the methodologists to carve out for their rivals an area characterized by peripherality, delinquency and provisionality, the sociologists of science make a strong case for rejecting the picture of science drawn by the methodologists and contend that, that picture is painted with untenable dichotomies and dubious distinctions. In the process, the territorial fight between the two academic disciplines results in a clash of two points of view concerning the nature of science and what constitutes its understanding. [4]

It is not intended here to go into the details of the debate which is briefly mentioned here to be treated as a point of departure in formulating a third point of view. The third point of view is prompted by the post–positivist developments in philosophy of science. It rejects the methodologist's claim that

the only task of philosophy of science is the articulation of the method of science, the supposed locus of scientific rationality. It is opposed to sociologism to the extent that it refuses to share the latter's contention that once the slogan " The Method is the message " is shown to be hollow, philosophy of science loses its claim for independent existence. Instead of viewing the emergence of sociology of science as a threat to philosophy of science, the view maintained here construes it to be a natural sequel to the collapse of methodolgists' hold resulting in the setting at naught a picture of science that has been meticulously sustained since Renaissance. The very breakdown of rationalism placed philosophy of science within the nexus of new relations. The relation between philosophy of science and sociology of science is one such relation in explicating which philosophers of science ought to redefine their domain of activity. The emergence of a full-blooded sociology of science is thus an occasion for redefining the sphere of philosophy of science.

However, the elucidation of the third point of view, as said earlier, is mainly based on a clue from a question Kuhn raises. Before coming to this point it is necessary to clarify the relation between Kuhn and sociology of science, including and especially, its strong Programme.

II

There is a strong impression among many that Kuhn is mainly responsible for the emergence of a fullblooded sociology of science. This impression is unwarranted. To a great extent this is made clear by Barry Barnes in a recent article.[5] Undoubtedly Kuhn "explains stability ... in science sociologically, in terms of the existence of potent mechanisms of socialisation and social control."[6] Also according to Kuhn, the question whether a perceived anomaly is a puzzle or a symptom of a deep crisis of the reigning paradigm cannot be settled except in terms of the

preferences of the scientific community. Further, for him, the issues concerning the paradigm choice can not be thrashed out by logic and experiments alone. In other words, the criteria which are used while choosing among competing paradigms and the criteria on which the superiority of the new paradigm over the old one is 'established' are too complex and too unique to the situation at hand to be expressed in the neutral idiom of logic and laboratory operations. The criteria and their use can only be understood in terms of a-rationally held values of the scientific community as a sociological entity. The very notion of a paradigm cannot but be explicated in sociological terms. "If the term 'paradigm' is to be successfully explicated" Kuhn says, "Scientific communities must first be recognised as having an independent existence."[7] This means that the concept of scientific community is more fundamental than the concept of paradigm. In short, by repudiating the power of autonomous individual reason and rejecting the individualistic account of research and pointing to the scientific community as the pillar of scientific stability and as piloting the locomotives of scientific change, Kuhn stands directly against rationalism, the received view whose individualistic account of science is in perfect harmony with the ideology of liberalism.[8]

Yet Kuhn's departure from the received tradition is not as radical as it appears. In fact he very much accepts the status given to science in the rationalist school. He only provides an alternative account of how science attains that status.[9] Secondly, the terms 'social' and 'sociological' in Kuhn connote nothing more than 'communital.' That is to say, the 'sociological' in Kuhn is only invoked to bring to bear the needs and requirements, the choices and preferences, the values and evaluations of scientific community as a professional group or sub-culture. Nothing more is implied. In fact if by 'positivism' we mean a '

scientistic self-understanding of sciences' to use Habermas' phrase, [10] Kuhn does not go much beyond positivism.

As if not to give any impression that he means by these terms or includes in their connotation anything like broader social factors, he says :

> it (*The Structure of Scientific Revolutions*) can be understood as an attempt to explain why the evolution of the more highly developed sciences is more fully, though by no means completely, insulated from its social milieu than that of such disciplines as engineering medicine, law and the arts." [11]

Again,

> .. compared with other professional and creative pursuits, the practitioners of a *mature* science are effectively insulated from the cultural milieu in which they live their extra-professional lives [12]

Nothing more is needed to ward off the impression that anything like a substantial justification for a strong programme in sociology of science can be got from Kuhn. With this we will come to the elucidation of the third point of view.

III

After drawing an analogy between political and scientific revolutions and plotting the evolution of science on the non-teleological line of biological evolution as conceived by Darwin, Kuhn says :

> Anyone who has followed the argument this far will nevertheless feel the need to ask why the evolutionary process should work. *What must nature, including man, be like in order that science be possible at all ?* ... The world of which

> that community (the scientific community) is a part must also possess quite special characteristics, and we are no closer than we were at the start to knowing what these must be. That problem–what must world be like in order that man may know it-was not, however, created by this essay. On the contrary, it is as old as science itself, and *it remains unanswered.* [13]

The question raised in the above passage is very much like the question "How is kgowledge possible?" – the central question of Kant in his *Critique of Pure Reason.* Hence we can call this question the Kantian question.[14] Since this question concerns the very nature of Being (i.e., Man and Nature) in relation to the possibility and actuality of science, this question is an ontological question. If and in so far as philosophy of science takes this as its central question, philosophy of science becomes, primarily, ontology of seience as distinct from mere epistemology of science which it is at present as is borne out by the fact that it is sometimes called, somewhat clumsily, scientific epistemology. Assuming such a metamorphosis of philosophy of science from epistemology of science to ontology of science, let us see what will be its various dimensions. This would, perhaps, make the metamorphosis less Kafkaean than it would otherwise appear.

The above mentioned ontological question can be decomposed into two questions : one, "What should the physical nature be like in order that science be possible?" and two, "what should man be like in order that scienec be possible?" Since man is biological and a social being, the second question itself can be further decomposed into, one, "what should the biology of man be like in order that science is possible?" and two, "what should the society be like in order that science is possible?" Thus, our central ontological question is divided into three sub-question concerning the physical world, the biological world of man and

the social world of man. We can say, therefore, that philosophy of science construed as ontology of science, has three dimensions which can be characterized as Metaphysics, Metabiology and Metasociology of science.

It should be noted that metaphysics of science conceived in this fashion is a different type of activity from building metaphysical theories on the basis of some particular scientific theories as did Whitehead and some others in recent times. But in construing metaphysics of science on the above lines we are not going away from the historical tradition. Emile Meyerson's work *Identity and Reality* to which Kuhn acknowledges his indebtedness is a paradigm work in that direction.

Metabiology of science deals with the question how certain concepts germane to the very possibility of science are to be traced to the organic needs of human species as construed in our conception of biological evolution. In one important way, this has been brought out very well by the 'temperate' rationalist–Newton–Smith.

> Take for instance the question as to why we believe that collecting instances of generalization increases the probability that a generalization is true. A philosopher may explain this as a conceptual consequence of some general concept of what it is to have evidence. But that only prompts the question : why do we possess that concept rather than another ? In the end it is probably only to be accounted for within some evolutionary framework in which the development and longivity of concepts is in part explained by reference to their utility in preserving our species. [15]

Obviously this needs the enlarging of the concepts of adaptation and environment to include human cultures while firmly retaining the biological core of these concepts. [16]

But the most important dimension of philosophy of science as ontology of science concerns the question " what should society be like in order that science be possible ? " In other words, philosophy of science as metasociology attempts to construct a social ontology in terms related to the question of the possibility of science. Such an ontology provides the theoretical back–drop which facilitates the working of sociology of science in its search for explaining specific modes of scientific practice. In what follows, some of the conceivable characteristics of such an endeavour are briefly mentioned.

It needs hardly to be stressed that the nucleus of social ontology is the idea of praxis. In their social existence men perceive themselves and their world in terms of certain concepts and categories determined by their knowledge of what they are and what they ought to be and shape them accordingly. The nexus of these concepts and categories is woven into the very texture of their praxis. It is this which lends praxis specificity and plurality. The pluralistic character of the mode of social praxis should be axiomatic for any attempt to build a social ontology. In other words, the recognition of the divergent trends of scientific practice traceable to their roots in the divergent modes of social praxis must be the starting point. But at the same time a global conception of praxis cannot also be eschewed. One of the most significant challenges to metasociology of science is to show in connection with the possibility of science how the possibility of a global theory of social praxis in no way entails the monolith of socio–economic formation. In other words, it should point out how a general theory concerning what society must be like in order that science is possible, in no way logically precludes the possibility of divergent traditions of scientific practice. In doing this, philosophy of science provides sociology of science the theoretical safeguards against ethnocen-

trism hitherto characteristic of philosophical, historical and sociological studies of science as reflected in their latent or blatant presumption that science is basically a western phenomenon. [17] Thus, whereas metaphysical and metabiological aspects of ontology of science might account only for the 'universal' component of science, metasociology does justice to the 'specificity' of scientific practice.

Secondly, metasociology should provide sociology of science the normative teeth enabling it to critically evaluate scientific practice in terms that go beyond the ones accepted by science itself or its shadow–ideology. It ought to show how a degrading praxis leads to a degrading science and how a mode of scientific practice with all its conceptual sophistication and technical achievements can still be degrading. It is only when sociology of science imbibes such critical perspective provided in terms of a set of value–judgements that it can get rid of every bit of positivist hangover.

If a mode of scientific practice is to be construed in terms of a type of social praxis which in its turn has to be understood in terms of concepts and categories in and through which human beings in a certain society perceive their own existence and the reality surrounding them, the 'rationality' of their scientific practice needs to be construed in ways unacceptable or even inconceivable for positivists or methodologists. Such a conception goes beyond internalism and externalism. Under such a conception of rationality, to use Feyerabend's words,

> There is only *one* task we can legitimately demand of a theory, and it is that it should give us a correct account of the world, i.e., *of the totality of facts as constituted by its own basic concepts.* [18]

It may be noted that such a conception of rationality is neither guided by anti–sociological considerations nor does it imply

anti-sociological consequences, characteristic of rationalist attempt to develop 'richer' theories of rationality. According to the rationalists like Lakatos the richer a theory of rationality the less it would leave for sociologists. There is absolutely no reason why the richness of a theory of rationality is inversely proportional to the need for sociological explanation.

In order to be receptive to and make an effective use of the the theoretical accomplishments of philosophy of science as construed above, sociology of science also should change some aspects of its strategy if not of programme. It must first eschew explicitly its construal of theories as ultimate units of scientific practice. Sociologists of science should conceive as units of scientific practice something which can be unambiguously specified in sociological terms Theories are not so easily amenable to sociological specification. Our answer to the question "what are the units of scientific practice?" is determined by and in turn determines our whole approach to science as an epistemic activity. Thus, both inductivists and Popperians found theories as units of scientific practice to be convenient to their programme and procedures. Lakatos who wanted to make Popperian frame work liberal enough to withstand Kuhnian structures broadened the unit of scientific practice and placed research programmes in the place of theories. Similarly Kuhn adopted paradigms as units of scientific practice in accordance with his general conception of scientific knowledge. As Newton-Smith points out "The positive and salutory virtue of Kuhn's use of his notion of paradigm is to remind us that in looking at the scientific enterprise it is important to focus on more than the theories (in the narrow sense of the term) advocated within a given community".[19] Eschewing theories as units the sociologist of science must adopt scientific traditions or something similar to it such that it is unequivocally specifiable in sociological terms. Of course, theories

continue to figure in his discourse, but not in the way they figure in the rationalist account. Such an approach gives the sociologists' programme a wholesomeness and free flow and facilitates total break with methodologism for which theories constitute the nucleus of scientific activity.

From the above discussion it is clear that sociology of science is far from threatening the autonomy of philosophy of science. If philosophy of science as metasociology of science provides sociology of science the much needed theoretical richness, analytical rigour and conceptual sharpness, it can in turn be provided by sociology of science the flesh and blood of empirical content. Even if metasociology is absorbed by sociology of science, the autonomy of philosophy of science is still not threatened since metasociology is only one of its three components. Thus, philosophy of science by redefining its domain as ontology of science and getting rid of methodo-mania can shed its socio-phobia.

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NOTES

1. *Progress and Its Problems*, University of California Press, Berkeley, 1977, pp. 188-9.
2. *Ibid.*, p. 222.
3. *The Methodology of Scientific Research Programmes*, Cambridge University Press, 1978, p. 134.
4. See, *Scientific Rationality : The Sociological Turn*, ed. Brown J. R.; D. Reidel, 1984.
5. ' Thomas Kuhn ' *The Return of Grand Theory in the Human Sciences*, ed : Skinner, Quentin, Cambridge University Press, 1985.

6. *Ibid.*, p. 90.
7. *The Essential Tension*, Chicago University Press, 1977, p. 295.
8. Barnes, Barry *op. cit.* p. 86.
9. *Ibid.*, p. 95.
10. *Knowledge and Human Interests*, Beacon Press, 1968, p. 4.
11. *The Essential Tension*, p. XV.
12. *Ibid.*, pp. 118–119 (emphasis added).
13. *The Structure of Scientific Revolutions*, second edition, University of Chicago Press, 1970, p. 173 (emphasis added).
14. This is definitely more Kantian than the problem of demarcation between science and non-science which Popper fondly calls Kantian.
15. *The Rationality of Science*, Routledge and Kegan Paul, 1981, p. 265.
16. For a significant attempt in this direction see, Goudge, Thomas A 'Neo-Darwinism, Mental Evolution, and the Mind-Body Problem' *Basic Issues in Philosophy of Science*, ed. Shea, William R., Science History Publications, New York.
17. For example, Karl Popper says "Polyphony, *like science* is peculiar to our western culture" (Emphasis added). *The Philosophy of Karl Popper*, vol. I, ed. Schlipp, P. A, Open Court Publications Co, 1974, p. 43.
18. *Against Method*, New Leaf Books, 1975, p. 284.
19. *The Rationality of Science*, p. 106.

PROF. G. R. MALKANI ESSAY COMPETITION

Essays are invited for the Prof. G R. Malkani Essay Competition **either in English or Hindi** from undergraduate or post-graduate students below the age of 25 years studying in any Indian educational institution on the theme "Śaṁkara's Concept of Prapañca" for the First and Second prizes to be awarded respectively of Rs. 200/– and Rs. 100/– to the essays adjudicated to be so by a panel of referees appointed for the purpose. The prize-winning essays would be published in course of time either in Students' Supplement of the **Indian Philosophical Quarterly** or **Paramasa (Hindi)**, quarterly journals published by the Department. The conditions governing submission of essays for the competition are as follows :

1. The essay typed in double space on one side of the paper must be submitted in duplicate.
2. The essay must not be longer than 2500 words.
3. The essay must be accompanied by a certificate signed by the Head of the Institution/Department where the student is studying to the effect that
   (a) the student is studying in that institution and is below the age of 25 years, and
   (b) the essay is written by him / her.
4. The essays should reach Dr. P. P. Gokhale, Philosophy Department, Poona University, Ganeshkhind, Pune 411 007 not later than 31–10–1988.
5. The decision of the panel of referees shall be binding on all the competitors and that no correspondence of any kind would be entertained on that count.

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