Controversy in science

M G NARASIMHAN
Science and Society Unit, National Institute of Advanced Studies,
Indian Institute of Science Campus, Bangalore 560 012, India
(Fax, 91-80-3346634; Email, narasim@nias.iisc.ernet.in)

1. Introduction

Although science abounds in controversies, detailed historical and sociological studies of their structure and role in the growth of scientific knowledge are of a recent origin. Controversies are sufficiently ubiquitous for them to be considered as indicators of intellectual change. They characterize developments within and about science. As Helga Nowotny (1975, as cited in Mendelsohn 1987), a sociologist of science, observes, "controversies are an integral part of the collective production of knowledge; disagreement on concepts, methods, interpretations and applications are the very lifeblood of science and one of the most productive factors in scientific development". Affirming this observation, Mendelsohn (1987), a historian of science, points out that controversies are fundamental to the production of knowledge in the sciences and that conflict is a natural outcome of the structure of scientific enterprise.

2. Theoretical aspects

Following McMullin (1987), I define 'controversy' as a publicly conducted and persistently maintained dispute over a matter of belief considered significant by a number of practicing scientists. This definition holds three important implications. These are:

(i) A scientific controversy is an event that endures over a period of time. This temporal extension implies that a controversy is a historical event and its analysis is mainly a historian's task.
(ii) A scientific controversy signals the participants' desire to demonstrate the well foundedness of their "epistemic" claims (i.e. knowledge claims which carry certain truth values).
(iii) Finally, a scientific controversy is a public event. No disagreement, however profound, can acquire the status of controversy unless there is an active involvement of the scientific community. The protracted nature of a controversy invests it with a historical character and community participation gives it a crucial social dimension. The clash of epistemic claims makes it a cognitive event. Thus, a controversy embodies an entire range of forces that propel science forward.

2.1 *Scientific controversy: role of different factors*

As a historical event, a given controversy is influenced by a variety of factors. Historians generally divide these into internal and external factors. Arguing that this traditional distinction is inadequate, McMullin classifies the factors constituting a controversy into two broad categories, epistemic and non-epistemic. He further classifies epistemic factors into standard and non-standard epistemic factors.

Elaborating his distinction between epistemic and non-epistemic factors, McMullin observes that an epistemic factor is one which the concerned scientist would take to be a proper part of the arguments she is making. These epistemic factors may be located first in published work which includes reports of observations and experiments, hypotheses, assumptions considered in their logical interrelation and temporal sequence. In addition to such explicit considerations, there are implicit factors as principles of method whose warrant is broader than that of the observational aspect of science. These factors are "epistemic because *they* are of the knowledge structure that the protagonists are setting at risk. What counts is what they proposed, believed, assumed to be relevant to the merits of the case that they are debating" (McMullin 1987, emphasis in the original).

These points may be summarized thus: A controversy recaptures the essential spirit of science in that it is part of the dynamics of science that aims at demonstrating the surety of the foundations on which knowledge is based.

And what determines whether a particular factor that goes into the constitution of controversy is 'epistemic' or not is the judgment of its relevance by the participants (including the concerned scientific community) in the controversy.

While McMullin identifies epistemic factors (both standard and non-standard) with observation, hypothesis and internal logic of a given controversy, he points out that non-epistemic factors are found in personality traits, institutional pressures, political influences, and 'chance events'. He argues that these non-epistemic factors also affect the outcome of the controversy; any account of the historical event which did not involve the role of such factors would be incomplete. But these do not form part of the 'truth' argument; that is, they would not be cited by those influenced by them as relevant to the merits of the case.

2.2 *A taxonomy of controversies*

Based on the nature of the issues giving rise to disagreements in a given controversy, such events can be classified into (i) controversies of fact, (ii) controversies of theory, (iii) controversies of principle and (iv) mixed controversies. Whereas the first three belong to the domain of natural sciences, mixed controversies include issues related to technological applications and are concerned with the moral or political principles on which the community is divided.

Of this taxonomy, the commonest kind of controversy in science originates in differences over theory. Two or more theories are put forward to account for the same phenomena. When the contending theories are mutually incompatible or irreducible every effort is made to eliminate all except one. Such attempts at elimination take the form of sustained debates or controversies. The possibility of scientific controversies of this kind is itself a statement on the fluidity of the philosophical situation (broadly construed) involving theory evaluation and theory choice. Although various logical models are available, the criteria for theory assessment and choice are not unambiguous or algorithmic in their operation. There is no evaluation metric that allows automatic and infallible choice between the theories being debated. Polanyi (1958) even claims that there is no objective framework which can account for the scientists' acceptance or rejection of theories. In general, only when one of the competing theories accumulates a significantly better record over its rivals does resolution of the debate become possible.

2.3 *Scientific controversy: termination*

While examining the temporal dimension of controversies, it should be noted that every controversy has a background, a beginning, a middle stage of active exchange and an end. It should also be noted that the different ways in which controversies come to a close make for fascinating historical research. McMullin identifies three different ways in which controversies are terminated, namely, resolution, closure and abandonment. Here we will focus attention on the ways in which controversies are resolved.

According to McMullin (1987) "the controversy may be resolved, i.e. agreement may be reached on the merits of the case. The participants themselves and the scientific community of their time are the judges of whether resolutions has occurred. The factors involved in satisfactory resolution are necessarily epistemic ones. And they will be standard epistemic ones in the eyes of the participants themselves". He also points out that "the outcome of controversy resolutions is that one or the other of the contested views (or perhaps a modified 'middle' view is accepted by both sides)". In other words, there can be either total resolution with a new theory completely supplanting an extant theory as in the case of the oxygen – phlogiston dispute, or 'mixed' resolution with the contending theories co-existing with each other. Such mixed resolutions are more common in biology than in other disciplines as exemplified by the debate between preformationists and epigeneticists in the history of embryology. But not all controversies terminate in such a triumphant or equitable resolution. As Max Planck put it while discussing Boltzmann's fight against Ostwald and the adherents of Energetics, "(a) new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." (Planck 1950).

Resolution as defined here is based on arguments whose soundness enables their adherents to reach a convincing position, thereby rendering opposite views incorrect. Historically, soundness has been judged according to the rules of evidence and inference that are contextually conditioned. Nevertheless, scientific communities attempt to resolve scientific controversies by appealing to rules of evidence and inference that are, as far as possible, undistorted by the personal and ideological presumptions of the participants. Later historians and other critics of science (including philosophers and sociologists) may, however, disclose distortions and demonstrate the presence of "non-epistemic" factors in the formation of the community's judgment.

3. Illustrations

Having briefly outlined the nature and structure of scientific controversy, we will briefly consider in this part three historical case studies, which illuminate different aspects of a given controversy.

3.1 *Darwin and Glen Roy*

As an example of a typical theoretical conflict, we can consider the controversy (1838–1861) about the geological origins of the parallel markings on the sides of certain mountains in Lochaber, Scotland, also known as the parallel roads of Glen Roy (Rudwick 1974; McMullin 1987). As Rudwick (1974) observes, the controversy is of considerable historical interest as it concerns one of the most distinguished individuals of the period, namely Charles Darwin, whose explanation of the roads failed to stand the test of time.

In 1838, after his famous trip on the Beagle but before he began his work on the 'Species Problem', that is, during perhaps the most creative phase of his life, Darwin made an important excursion to the Scottish Highlands in order to study the celebrated Parallel Roads of Glen Roy. In 1839, in his first major scientific paper, Darwin argued that the Roads were ancient sea beaches, a testimony to the gradual subsidence of the Scottish landmass and an instance of a worldwide phenomenon of elevation and subsidence of land caused by movements in the fluid magma under the earth's crust (McMullin 1987). Applying the same principle, he suggested that these movements were also responsible for earthquakes, volcanoes and the gradual subsidence of coral reefs. Darwin's study was detailed and ingenious. He had argued in favour of the markings being sea beaches basing it in the idea of a retreating sea, which had paused at various levels thereby creating the beaches. Earlier to Darwin two people had worked on this problem. They had suggested a different hypothesis. McCullough (1816) and Lauder (1821) had proposed that the Roads were lake beaches. The core of the lake hypothesis was that the roads were the beaches of a former lake, which had stood at three successive levels, the impounding barrier or barriers being broken down in stages (Rudwick 1974). But Darwin was dissatisfied, as was his mentor Lyell, and he dismissed this theory on the grounds that there were "no traces of the barriers that would have been needed to create lakes at the requisite levels" (McMullin 1987).

But in 1842, Louis Agassiz, who was looking for evidence for his theory of Alpine glaciations, came up with a new lake hypothesis; he argued that the Roads were the remains of lakes, but the lakes were glacial ones. The barriers lacking in the original hypothesis were now provided by glaciers, which had melted away over a period of time and left little trace. Agassiz's new hypothesis added a fresh dimension to the controversy: "Darwin's falling sea level versus Agassiz's glacial lakes versus Lauder's ordinary lakes." At a later stage, after Agassiz's new hypothesis Darwin wrote to Lyell: "I made one great oversight, as you would perceive. I forgot the glacier theory." (Rudwick 1974; McMullin 1987). Darwin's explanation of the Roads of Glen Roy was based on his South American experiences of fossil sea beaches; whereas, in contrast, Agassiz's was based on his detailed knowledge of the glaciers of the Alps. Although Darwin confessed to Lyell that his marine theory had been knocked on the head by Agassiz's ice-work, he was not prepared to abandon his theory. He was not convinced, even though the absence of the barrier problem had been solved and his main objection to the original lake hypothesis had been removed. In addition, his marine hypothesis fitted in too well with his theory of world wide land elevation and sea subsidence. Darwin also received support from his mentor Lyell and other friends. The main problem with Agassiz's paper was that it was a brief sketch of an hypothesis (while Darwin's had been a masterpiece of an argument) and Agassiz's global "Ice Age" theory was suspect to Lyell and other uniformitarian geologists of Britain.

The controversy took a new and somewhat different turn when, in 1847, David Milne published a revised interpretation of the Roads. In his paper, Milne not only dissented from Darwin but also from Agassiz's glacial alternative and presented a modified version of the original Ordinary Lake hypothesis as suggested by McCullogh, and in particular, by Lauder. Milne's attack shook Darwin and made him "tremble", but he described the *ad hoc* hypothesis of barriers that had somehow vanished as "monstrous". The most decisive blow fell on Darwin's original hypothesis, which he had (over) confidently described as "demonstrated" when Thomson Jamieson published his detailed review of the controversy in 1863. (Rudwick 1974).

In 1861, Lyell had suggested to Jamieson that he should make a thorough study of the area. But, as Rudwick informs us, Darwin too, having "to some extent doubted his own observations" there, "not having glacier action in view" in 1838, had also encouraged Jamieson and lent him his maps and notes. Jamieson, after reviewing the varied contributions made by McCullough, Lauder and Darwin, argued that Agassiz's glacial interpretation of the area, despite its sketchiness, had the merit of suggesting that the mysterious barriers might have been composed of ice, and not rock debris. He also pointed out that the Roads were beaches "too fine and neat" to have been formed under tidal marine conditions and that their fine preservation showed that they had accumulated in sheltered water. In addition, the perfect horizontality of the Roads – their most striking feature – was evidence in favour of Agassiz's hypothesis. After carefully examining all available pro and contra evidence, Jamieson concluded that both lack of good and positive evidence in favour of the marine hypothesis and availability of such evidence in favour of Agassiz's hypothesis had rendered Darwin's theory completely untenable.

The controversy was over. Darwin's response was somewhat melodramatic. He wrote to Lyell stating that he had been "smashed to atoms about Glen Roy" by Jamieson's paper. He also felt that his own paper was "one long gigantic blunder from beginning to end" (cited in Rudwick 1974). Again, two decades later, Darwin recalled that as soon as he had read Jamieson's article on the parallel roads he "had given up the ghost with more sighs and groans than on almost any other occasion in my life." (Rudwick 1974). Rudwick remarks that Jamieson's paper was a model of accurate description and cogent reasoning. With its elegant map (reproduced in Rudwick) and clean, persuasive and virtually watertight case, the paper clinched the debate in favour of the glacial lake hypothesis.

As can be seen from the observations made earlier, this controversy has all the major features of a theoretical controversy. Although McCullough and Lauder had done some initial work, the real impetus for further debate and work came from Darwin's first (and last) paper on the topic. Following this, there was vigorous debate conducted both in the formal scientific literature and in private correspondence testifying to the fact that a controversy is a communicative event par excellence. In terms of the role played by various factors, epistemic and non-epistemic, McMullin points out that the resolution of the debate was "standardly epistemic". This is not to say that non-epistemic factors such as the sketchiness of Aggasiz's paper had no role to play. But the final settlement of the problem was based on Jamieson's work for which Lyell and Darwin were responsible. Finally, as Rudwick shows, Darwin's tenacity in sticking to his position is also explicable not so much by such non-epistemic factors such as his "personal pride" as by a variety of epistemic factors; the structural parallel between the argument of the Glen Roy paper and that of his earliest work on the origin of species; the consonance of the Glen Roy hypothesis with his theory of land elevation and subsidence; and his reliance on methodological themes from Lyell, Whewell and Herschel (McMullin 1987).

3.2 *Spontaneous Generation*

While the conflict related to Darwin and Glen Roy could be construed as an instance of an instance of a controversy where in standard epistemic factors had played a decisive role, the Spontaneous Generation controversy (Farley and Geison 1974; Mendelsohn 1987) presents us with a type where in several elements (i.e. apart from epistemic factors) contributed to the development and termination of the controversy. Although the controversy stretched over a long period of time from Descartes to Oparin, i.e. the 17th to the 20th century, one of the most decisive phases of the debate occurred in the 1860s in France in the confrontation between Louis Pasteur and Felix Pouchet. When the controversy between them began, Pasteur was 37 years old and Pouchet was nearly 60. Pasteur had only just then entered the study of biological problems. Earlier to this his training, interest and expertise lay in the fields of crystallography and chemistry. Pouchet, on the other hand, entered the debate after a long career in traditional biology with his major interest being in animal generation.

In the case of Spontaneous Generation, traditional historiography informs us that Pasteur defeated Pouchet (an ardent supporter of Spontaneous Generation, also known as abiogenesis or heterogenesis) in a decisive manner on the basis of experimental evidence alone. Two historians of science, Farley and Geison, in a reassessment of the entire event have shown that this account is completely inadequate. [This, in turn, has been challenged by Nils Roll Hansen (1979).] In 1859 (the same year in which Darwin published the Origin) Pouchet published his monograph, 'Heterogenie'. Earlier he had done a good deal of work on animal generation. In this work, Pouchet argued that ". . . a plastic manifestation is produced in animals themselves. Or elsewhere, which tends to group molecules and to impose on them a special mode of vitality from which finally a new being results." (cited in Farley and Geison 1974). He further observed that this 'plastic force' could manifest itself in plant and animal debris. But the distinctive aspect of his argument was that ". . . it is not adult organisms which are thereby spontaneously generated, but rather their *eggs*." (Farley and Geison 1974; emphasis in the original).

Within several months of the publication of Pouchet's work, Pasteur performed his own experiments, which appeared to be innovative and brilliant. Therefore, it seemed as though the dispute was limited to experimental results and their interpretation. However, the situation was more complex. In 1862, the French Academie des Sciences formalized the debate by announcing a prize that would "be given to him who by well-conducted experiments throws new light on the question of so-called Spontaneous Generation" (Farley and Geison 1974). The entry and participation of the Academie, Mendelsohn argues, "seems to have been inspired by the fact it had its eyes on other elements. (i.e. other than purely scientific) than merely spontaneous generation. Indeed, the Academie acted as an arbitrator and cast the terms of the debate in the form of judicial proceeding. The Academie also appointed a commission to survey the submissions and judge a winner to whom the prize could be awarded. While this in itself was not politically suspect, the commission had to be disbanded on Pouchet's charges of unfairness – some members had announced their decision before even examining the entries, forcing Pouchet and his collaborators to withdraw, thereby enabling Pasteur to receive the prize uncontested on the strength of his 1861 memoir. A new commission was appointed in 1864, but it ran into similar problems of unfairness and favouritism. One may wonder, perhaps legitimately, as to what role Pasteur had played in these developments. While Pasteur's experiments did prove to be significant, his overall conservative political stance also played an influential role in deciding the outcome of the dispute. As observed by Richard Owen (himself an opponent of spontaneous generation), "Pasteur, like Cuvier, had the advantage of subserving the prepossessions of the 'Party of Order' and the needs of theology" (cited in Farley and Geison 1974).

What this brief analysis demonstrates is that the political structure of controversies are, on certain occasions at least, as important as the cognitive and epistemological elements. The Pasteur–Pouchet debate was carried out in a politically charged France of Louis Bonaparte (nephew of Napoleon I) emperor of Second Empire. Having come to power with the general support of the Catholic Church, the new emperor was widely recognized as a vigorous opponent of republicanism, atheism and materialism. In the context of his rule all religious and philosophical issues were simultaneously political issues as well. In the light of this, Pouchet was portrayed as a rebel and Pasteur was portrayed as a strong supporter of the orthodox church and the conservative state. Although the French debate was closed on the basis of Pasteur's so called 'Victory', it reemerged in Britain, a decade later. Here too, as elsewhere, a variety of forces came into play with T H Huxley and John Tyndall, on the one side and Charlton Bastian, the proponent of Spontaneous Generation, on the other.

3.3 *The continental drift debate*

As a third example of a controversy and its resolution, we can briefly examine a recent debate that took place in the earth sciences (Frankel 1987).

The continental drift hypothesis was first proposed by a German meteorologist, Alfred Wegener, in 1915. According to this hypothesis, the continents of the earth are thought of as 'ships' made of light sialic material 'floating upon' a heavier material that formed the sea floor. He hypothesized that the continents underwent horizontal displacement relative to one another by plowing through the denser basaltic ocean floor. The drift hypothesis as it came to be called sharply contradicted the earlier well-established 'fixist' theory. Fixism maintained that the continents having been fixed in their positions for a very long time had not changed their positions through any lateral displacement. Although initially the reaction to Wegner's drift hypothesis was not hostile, within a few years it encountered very strong opposition. For many years the adherents of the drift hypothesis were dismissed, particularly by geologists and geophysicists, as cranks.

But in the 1960s a dramatic change occurred in scientists' attitude. In 1962, Harry Hesse published a paper which provided fresh support to the drift hypothesis. Hesse argued that the floor of the seas spread outward and as it spread outward, it carried the continents along with it on the mantle. Hesse's proposal was soon picked up by Vine, who along with Mathews, confirmed the hypothesis of the spreading sea floor. This showed that the foundations of the drift hypothesis were secure. With the vindication of the Vine–Mathews–Morley hypothesis (Morley had independently arrived at the idea of sea floor spreading) the conversion of the fixists was fairly quick. Maurice Ewing, who had earlier supported the fixist position, observed that the Vine–Mathews–Morely proposal provided "strong support for the hypothesis of spreading" (Frankel 1987). The continental drift hypothesis is now subsumed under 'Plate tectonics' and constitutes a major research programme in the earth sciences. Thus, the single most important factor in bringing about the closure (i.e. resolution in McMullin's terms) of the drift-fixist debate was the vindication of the Vine–Mathews–Morely hypothesis. It is clear that this debate like the Darwin–Glen Roy debate but unlike the spontaneous generation controversy, was resolved largely on the basis of standard epistemic factors.

To sum up then, these case studies show that each controversy while being unique, also shares certain common features with others.

4. Conclusion

While it is not possible to offer generalizations based on two or three case studies, it is necessary to observe that a study of different controversies as documented by historians and sociologists demonstrates the sheer diversity of the ways in which theories change in scientific growth. It also shows how important and relevant it is to focus on the varied processes of scientific change rather than limit oneself to a "falsificationist" (Popper 1959) analysis of the way in which the final product of such a change gets established as a part of the general fund of knowledge. While explicating his theory of the logic of scientific discovery Popper drew a distinction between the context of scientific discovery and the context of justification. Since he focussed his attention on the ways and means by which a given theory stood the test of justifiability or falsifiability, Popper excluded the process of discovery from philosophical analysis. This dichotomy resulted in relegating the study of discovery processes to the sociology and psychology of knowledge. But with the emergence of controversy as a philosophical and historical category, the gap between the study of processes of discovery and the processes of justification has been bridged.

Finally, a study of scientific controversy is highly relevant to an explication of scientific rationality, particularly in view of current developments in the philosophy of science. Prior to these developments philosophers equated logicality with rationality and argued that logical processes such as induction and deduction were the only means through which the rationality of scientific change could be ascertained. But later, philosophers like Toulmin (1974) pointed out that the scientific enterprise is an intellectual endeavour whose rationality lies in the procedures governing its historical developments. Scientific rationality also involves the role of theorizing as it occurs through the indispensable role of the human imagination. In this context, it must be observed that extreme positions have been articulated – that science is totally rational and that it is totally irrational. McMullin (1987) points out that the term 'rational' is extraordinarily ambiguous and that "scientific rationality has taken a long while to form and is undoubtedly not yet fully realized. It requires complex educational and communicative procedures, ones that are of considerable interest to the social historian. It is influenced by the prevailing epistemic assumptions of the society; the mode of transmission of this influence and the manner in which it makes itself felt, are worth investigation." In other words, McMullin proposes that no one approach in isolation from others is capable of providing us with a proper understanding of scientific controversies. He sees a creative and constructive role for all the different approaches that could be used in comprehending the complexity of scientific change.

In sum then, we concur with McMullin's (1984) observation that science is a learning process, a search for understanding, and in human terms, this feature is more important than other features such as science's power for prediction and control. The learning process is a gradual historical development; a process aiming at both the fulfillment of a fundamental desire to acquire a reliable knowledge of the environment and to understand the complex panoply of puzzling events and phenomena – both mental and physical – that nature in her unbridled freedom chooses to display. Understood in these terms "scientific rationality" represents whole array of processes and procedures employed in the pursuit of this goal.

Acknowledgements

I thank Drs A R Vasavi and Renee Borges for their critical reading of an earlier version of this paper.

References

Farley J and Geison G 1974 Science, politics and Spontaneous Generation in 19th century France: The Pasteur–Pouchet debate; *Bull. Hist. Med.* **48** 161–198

Frankel H 1987 The Continental Drift Debate; in *Scientific controversies* (eds) T H Engelhardt Jr and A L Caplan (Cambridge: CUP) pp 203–248

McMullin E 1984 The rational and the social in the history of science; in *Scientific rationality: The sociological turn* (ed.) J R Brown (Holland: D Reidel) pp 127–163

McMullin E 1987 Scientific controversy and its termination; in *Scientific controversies* (eds) T H Engelhardt Jr and A L Caplan (Cambridge: CUP) pp 49–91

Mendelsohn E 1987 The political anatomy of controversy in the sciences; in *Scientific controversies* (eds) T H Engelhardt Jr and A L Caplan (Cambridge: CUP) pp 93–124

Nils Roll-Hansen 1979 Experimental method and Spontaneous Generation: the controversy between Pasteur and Pouchet, 1859–1864; *J. Hist. Med.* **34** 273–292

Planck M 1950 *Scientific autobiography and other essays* (London: Williams and Norgate)

Polanyi M 1958 *Personal knowledge: Towards a post-critical philosophy* (London: Routledge and Kegan Paul)

Popper K R 1959 *The logic of scientific discovery* (London: Hutchinson)

Rudwick M 1974 Darwin and Glen Roy: A 'Great Failure' in Scientific Method?; *Stud. Hist. Philos. Sci.* **5** 97–185

Toulmin S 1974 Scientific strategies and historical change; in *Philosophical foundations of science* (eds) R J Seeger and R S Cohen (Holland: D Reidel) pp 401–414