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The Background and Present State of the Controversy on Social Control in Science

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Sociology of science is one of the most recent branches of sociology. Its beginnings go back to the 1920s and 30s, but in spite of the brilliance of some of these, they did not give rise to a continuous tradition in the field. Thus the main theme of the "Foreword" by Robert K. Merton to Bernard Barber, Science and the Social Order, 1952 is the reasons for the neglect of sociology of science. Barber's book was the first programmatic effort to present the field as fit for recognition as a regular specialty in sociology. It tried to show that there was enough substantive research and conceptual clarity in the field to justify such recognition. The book immediately became the authoritative text on the subject, and was very widely read. But it took another decade before one could speak of the emergence of the field as a sociological specialty.

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* There are a number of up to date surveys and bibliographies of the literature on the subject (Merton and Gaston, 1977; Lecuyer, 1978; Mulkay and Milic, 1980; Gaston, 1980), and there is no point adding another one to them. Therefore, this paper is not intended to convey an exhaustive and balanced picture of the field, but rather to focus on two controversial issues, namely on the debates about the norms of science and the sociology of scientific knowledge, which--in the view of this author--are of central importance to the field. I have dealt with these issues in several recent papers, and this one is partly a continuation and partly a modification of ideas expressed in those (Ben-David, 1978; Ben-David, 1981).

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The causes of this delay can be actually found in Science and the Social Order. Its wealth of empirical material was largely derived from investigations done by non-sociologists. Thus among the classics of the field only one, namely Robert Merton's Science, Technology and Society in Seventeenth Century England, 1939, was written by a sociologist.* And this was a study unlikely to find imitators among students of sociology growing up in the 1950s in the United States since it was based on deep historical scholarship, which was not sought by that generation of sociologists. There was a paradoxical situation in sociology. Sociological theory around 1950, influenced by the monumental efforts of Talcott Parsons, who had just assimilated and reconceptualised the great traditions of historical and comparative sociology embedded in the work of Durkheim, Weber and the British social and American cultural anthropologists; but (with the exception of a few individuals) this did not serve for the students of the fifties and the sixties as a stimulus to the acquisition of historical, ethnographic, and linguistic erudition. Parsonian theory, plus some of Durkheim, Weber, and other classics, revitalised through the work of Parsons, were studied in a disembodied way, in complete disjunction from the research tradition out of which they emerged, and--as a result--had relatively little influence on research. The training of students and the empirical research in the graduate departments were based on the assumption that

* By "classic" I am referring to works of recognized quality, which have exerted still lasting influence on the field. Two other works from the thirties and forties would, I think, qualify for inclusion in this category: J.D. Bernal, The Social Function of Science, 1939 and the essays written between the late thirties and the late forties and collected in M. Polanyi, The Logic of Liberty, 1951.

sociology was on its way to become "scientific" through the adoption of survey research as its principal methodology, since this approach enabled the sociologists to make quantifiable observations, structured according to their own concepts of social life, and to design research in a manner allowing the testing of hypotheses. Temporarily, at least, this eliminated history from the intellectual horizon of the large majority of sociologists. Therefore, neither the classic example of Merton, nor Barber's virtuosity in putting together a respectably organized body of knowledge from seemingly incoherent bits and pieces of studies and comments were sufficient to inspire even a small group large enough for a specialty.

The opportunity for this emerged in 1957, as a result of a combination of external and internal events. The external event was the launching of the Sputnik which gave rise to efforts at accelerating scientific and technological growth in the Western countries and created widespread interest in sociology of science as a discipline presumably capable of contributing to the understanding of the conditions and mechanisms of such growth. The internal event was a paper by Merton based on many years of research on independent multiple discoveries, which gave to this old theme a new meaning by focusing not on the fact of multiple discovery, but on the phenomenon frequently accompanying it, namely clashes about claims to priority between the different discoverers (Merton, 1957). Although this, like the earlier works of Merton, was based on historical research, it focused attention on the importance of competition, allocation of rewards, social control and stratification in science, which were issues amenable to quantitative study. This opened the way to the articulation of sociology of science with theoretical issues central to sociology, and suitable to be studied quantitatively through survey research and analysis of data on publications, awards and career mobility (Hagstrom, 1965; Zuckerman, 1967; Crane, 1967; 1969; Hargens & Hagstrom, 1967; Merton, 1968; Gaston, 1973; Cole and Cole, 1973). This was crucial for the success of sciology of science as a specialty in American sociology (which--around 1960--was tantamount as success in world sociology), since it made possible to conduct investigations with the aid of concepts and methods basic to graduate training in sociology.* The opportunities for the use of advanced methodologies increased greatly in the sixties, thanks to the rich data base provided by the Science Citation Index. This is not to say that there were no other kinds of interest in the sociology of science. There were two other lines of research which looked as good prospects from the point of view of the requirements of graduate training. One was the microsociological study of scientific organizations from the point of view of effectiveness of research management (Shepard, 1956; Kaplan, 1965; Glaser, 1964; Allen, 1966; Gordon and Marquis, 1966; Pelz and Andrews, 1966; Allen and Cohen, 1969). These studies have, indeed, had an uninterrupted development since the fifties, but in the Western countries they became separated from the mainstream of sociology of science, and have been integrated with general management studies. In the Communist countries of Europe, however, they are of central importance in the sociology of science (Mulkay 1980).

* The relationship between the two events, Sputnik and Merton's priority paper was entirely coincidental. Merton's paper did not deal with the kinds of problems relevant to the issues raised by the Russian technological success. But they interacted and contributed jointly to the emergence of sociology of science as a recognized specialty.

The second line of research suitable for graduate training has been the study of scientific communication and cooperation, which were so to speak obvious social processes and could be linked to the general study of the flow of communications (Menzel, 1966) and the sociometry of groups. These studies received great impetus from Price (1963) and the Science Citation Index. Price showed how the application of demographic methods, and analyses of authorship and co-authorship could teach a great deal about the rise and decline of scientific specialties and the Citation Index provided extremely rich source material for the performance of complex sociometric analyses of scientific networks. Indeed there has been a succession of publications in this area, but they have never had such a clear cut focus as the studies of the reward system (Hagstrom, 1965; Price and Beaver, 1966; Griffith & Miller, 1970; Griffith and Small, I-II, 1974; Small, 1973, 1977; Gilbert & Woolgar, 1974; Breiger, 1976; Sullivan, White & Barboni, 1977a, 1977b; Chubin, 1976; Friedkin, 1978). Furthermore, serious criticism was raised against the use of the Citation Index and the purely sociometric approach to the analysis of the rise and decline of specialties, on the ground that citations are neither reliable nor valid indexes of communication and cooperation, and it has been suggested that quantitative analyses should be trusted only if corroborated by direct historical evidence (Cohen-Shanin, 1975; Gilbert, 1977; Edge, 1979). Indeed, there were throughout this period sociological studies of scientific disciplines, and growth using purely qualitative data, or quantitative data in conjunction with qualitative ones (Ben-David, 1960a, 1960b; Ben-David and Zloczower, 1962; Zloczower, n.d.; Ben-David and Collins 1966; Fischer, 1966, 1967; Crane, 1967; Mullins, 1968).

Throughout the sixties work in sociology of science was centered in

American sociology departments engaged principally in studies of the scientific reward system, stratification and, to a smaller extent, specialty networks with an emphasis on their sociometric aspects. Elsewhere--with the exception of science mamagement studies in Eastern Europe--the field was still represented by individuals rather than groups, not all of whom were professional sociologists. But there was sufficient world-wide interest to launch a Research Committee on the Sociology of Science in the framework of the International Sociological Association in 1966. In 1962 Edward Shils began publishing Minerva (in London), and sessions devoted to the field at the meetings of the American Sociological Association drew large audiences. By 1970 the field seemed to be set for rapid and continuous development. One of the most promising features of this new branch of sociology was, that in contrast to many other branches, which were by that time torn by controversies, there was in sociology of science a high degree of consenses on basic intellectual matters. Some of the practitioners attributed this to the beneficial disciplining effect of the proximity of the field to the natural sciences.

II

There was, indeed, continued development, but controversy and conflict caught up with the field with a vengeance. Proximity to the natural sciences which was a source of consensus in the sixties, became the main source of conflict in the seventies. These unexpected developments were due to a number of circumstances which transformed the social composition of the field and its social functions.

Throughout the sixties there was much concern in Europe with the scientific and technological backwardness of Europe compared to the United States. This concern drew attention to sociology of science, as concern with Soviet superiority in space technology drew attention to the field in the late fifties. But because the gap between the United States in Europe was more real and general than that between the United States and the Soviet Union, the general interest in sociology of science was more enduring in Europe. Science policy became one of the most important concerns of the OECD, and was widely reported on in the press and debated in various houses of legislature. Civil servants, politicians and scientific statesmen were all interested in sociology of science, as a field of potential use in the reformation of scientific institutions and the acceleration of scientific and technological growth in Europe.

This practical, "lay" interest had several repercussions on the development of the field. It greatly facilitated the establishment of special institutional arrangements for research and teaching in the sociology, economics and politics of science, such as the Science Policy Research Unit at the University of Sussex, the Science Studies Unit at the University of Ediborough, and attracted a number of younger scholars trained in experimental science, philosophy, or history of science to the field. Because of the practical and extradisciplinary origins of much of the interests in, and some of the practitioners of the field, the new institutions were usually conceived as interdisciplinary ventures. They were institutions for the "social study," rather than "sociology of science." Thus, a significant part of sociology of science in Europe, especially in Britain, has been practiced in interdisciplinary units, rather than departments of sociology.

From the perspective of this--to a large extent interdisciplinary and policy oriented background--the interests of sociologists of science in the reward system, norms, and stratification in science appeared as narrow and parochial. It was disappointing that much of sociology of science was about the sociology of the scientific profession, and less about the contents of science and matters of science policy. During the seventies this dissatisfaction with the state of the field assumed a critical tone. This was probably related to the emergence of critical and hostile attitudes toward science and higher education in general, but the contents of the criticism were determined by problems immanent to sociology in general and the sociology of science in particular. The criticism focused on two issues: on the alleged deficiencies of the structural-functional approach in sociology of science, particularly on the use of "scientific norms" as a key concept in the description and interpretation of the institutional framework of science (Mulkay, 1969; Barnes & Dolby, 1970); and on the absence of a sociology of scientific knowledge (Barnes, 1974; Bloor, 1976; Mulkay, 1979).

III

Criticism of the use of scientific norms derived from the more general criticism of structural-functionalism in sociology. In debates on this subject in the fifties and sixties, the theory was attacked on the grounds of postulating common values and norms as important explanatory concepts, and stressing the theme of social homeostasis, instead of looking beyond such appearances and discovering the conflicts, negotiations, divergences of meanings and constant changes which go on behind the conventions of normative behavior and the semblance of social equilibrium. The tendency of stressing common values, norms and social equilibrium was attributed by some of the critics to political conservatism (Dahrendorf, 1958; Gouldner, 1970).

Parts of this criticism were of particular interest to sociology of science. The concept of "norms of science," meaning a code of professional ethics obligatory for scientists, such as judging scientific work in a universalistic and disinterested manner, publishing research results in a way to make it accessible to others, or "organized skepticism," which-- in principle-- leaves open every theory to reexamination on the basis of new argument and evidence, was used a great deal in the sociology of science of the sixties (Storer, 1966), and conformity to or deviance from these norms featured heavily in all the studies of scientific rewards and social control in science. No one asserted that there was universal conformity with these norms and in fact both papers of Merton (1942, 1957 in Merton, 1973) which introduced their study in sociology were concerned with deviations from them (by Nazi scientists, and by many others in fights about priority). But all sociologists took it for granted that these norms were generally known and approved of by scientists, and that there were effective social controls in science to enforce conformity with them. In fact, they considered these controls in science as singularly effective and regarded science as a model for the study of social control and consensus formation. These strong assumptions about the norms of science made sociology of science a particularly interesting challenge for those who were skeptical about the sociological usefulness of the concept of norms altogether. If it could be shown that in science--which was considered as the social institution with the most effective controls to enforce norms--deviance from the alleged institutional norms is no less characteristic than conformity with them, then this would effectively undermine the entire structural functionalist approach based on the assumption of the existence of different norms characteristic of each social institution.

This gave rise to an extensive debate, and some empirical research on this issue. The empirical research is of three kinds: statistical investigations of the behavior of scientists in matters, such as recognition, citation, or the awarding of prizes, in order to test their consistency with the norms of science, which is, of course, a direct continuation of the tradition of studies of the scientific reward system (Blume and Sinclair, 1973; Cole and Cole, 1973; Gaston, 1978) attitude surveys of scientists with questions supposedly reflecting conformity or non-conformity with the norms (Blissett, 1972; Toren, 1980); and detailed studies of scientific controversies in order to discover the actual rules of behavior followed by those involved in them (Mulkay, 1969; Mitroff, 1974; Collins, 1974, 1975; Wynne, 1976).

The results are seemingly contradictory. Those of the statistical studies of scientific judgements and allocation of rewards support the structural functional view of the existence of considerable conformity with the professional ethos (or norms) of science. The outcomes of the attitude

surveys are not clear, since they show conformity on some items, not on others, and because the outcomes of the different studies are inconsistent with each other. Finally the case studies show little conformity with the norms, producing instead a bewildering variety of considerations and "rules" resorted to by participants in controversies, and revealing a process of reaching agreement through "negotiation", which is interpreted as inconsistent with the existence of consensual norms of conduct among scientists.

Critics of the structural-functional use of norms choose as more convincing the case studies, and deal with the problem presented by the studies of the reward system by suggesting (however not in a very detailed way) that the outcomes of those studies can also be interpreted in ways not necessarily involving conformity with the scientific norms (Mulkay and Milic, 1980).

It seems to me that this interpretation is erroneous, and that, in fact, the three sets of studies do not contradict each other, but present a coherent and consistent picture, with each kind of study illuminating different parts of it.

The studies of the rewards system, as well as the entire structural-functional theory on the importance of professional ethos, in general, are silent about the behavior and motivations of individual scientists, and about the decision making process in individual cases. They only assume that there are institutional mechanisms which in the aggregate and the long run try to enforce conformity with the moral norms of the profession. This aggregate behavior is what the studies of the reward system are concerned with and all that they claim to show is that there is an aggregate tendency to conformity with the norms.

The findings of the case studies do not contradict any of this. They throw important new light on that part of the process of scientific evaluation which consists of the classification of the factual, cognitive aspects of particular instances, and the formulation and interpretation of methodological and logical principles appropriate to them. Moral issues are rarely raised in these debates, since conformity is usually assumed. When there is an accusation that some of those involved in the process did not act according to these norms, however, this is taken seriously. Thus rejection of the very unusual cosmological ideas of Velikovsky without examination of the evidence was widely challenged (although few believed that he was right), and it was felt by representatives of the scientific community that the accusations required answer (De Grazia, 1966; Polanyi, 1967). A reverse of this case occurred in the controversy following A. Jensen's paper on the importance of heredity in the explanation of differences in the intelligence quotients of blacks and whites (Jensen, 1969). This paper was seen by some as advocacy for racial prejudice, and was also severely criticized for empirical and methodological shortcomings. Although some of the critics used scientifically acceptable arguments, it was suspected that they went in their criticism beyond accepted cognitive standards and procedures in the field. This question was considered as important enough for detailed discussion and censure (Barnes, 1974; Cronback, 1975).*

* Barnes uses this case in order to show that it can be analysed without recourse to norms of science. What he actually shows, however is that the application of methodological rules is less than equivocal. But the moral question of universalism which he implicitly raises is unequivocal.

These cases show that the critics of the normative approach confuse cognitive judgement with moral norms about the behavior proper for those exercising scientific judgement. What the case studies on the conduct of scientific controversies examine is the scientific parallel to the court procedure of fact finding through examination of conflicting evidence, presented, frequently, in a biased way, and the determination of the legally relevant aspects of the evidence. This is an entirely cognitive process, and like all such processes, it is subject to many doubts, inaccuracies and uncertainties. Moral norms cannot eliminate cognitive difficulties, but they can, and do, provide some kind of control on the behavior of scientists, making it difficult to discriminate openly against any person or theory on grounds external to science ("universalism"). In the Velikovsky case of rejection of a theory without proper examination, the question is contextual, namely whether this norm was transgressed by denying hearing to an outsider who uses ideas and methods completely different from those accepted at that time by scientists; in the Jensen case the question was whether the way in which apparently correct procedures were used was not actually discriminatory. The moral norm itself has not been in doubt in these cases.

This interpretation is also consistent with the ambiguity of findings of attitude surveys. This is probably the result of the conceptual difficulty of distinguishing between cognitive rules and moral norms, and, of making unequivocally clear the context within which these rules and norms apply.*

* For example, Toren finds weak support--51 percent--for the norm of "disinterestedness" on the basis of argeement or disagreement with the statement: "Scientists need not be motivated solely by the contribution of their work to scientific knowledge; they have a right to choose projects which will enhance their reputation and personal interest." But in a different context she finds that the norm of "utility"--to judge work mainly by its practical usefulness--is rejected by 68 per

Therefore, the terms in which much of this debate has been conducted throughout the seventies are beside the point. The research reviewed here--on which the debate about the norms is based-- shows that there are relatively stable moral norms (see also Ezrahi, 1980), but their application, like that of of all norms, is dependent on context, namely on the definition of the situation as falling within the accepted boundaries of scientific work, and probably also on whether a given scientist acts in the role of contestant in a scientific debate, or in that of the judge or referee (Ben-David, 1977). The main difficulty, however, arises from the fact that the moral code has to be applied to cognitive processes of evaluation of evidence and examination of arguments (Zuckerman, 1977). These processes are complicated and subject to a variety of more or less stable criteria and much less stable rules of interpretations, and considerations of particular circumstances (Kuhn, 1977; Zuckerman, 1977). Much of the argument about the non-existence or a "negotiability" of the norms of science derives from a confusion of the normative moral process with the cognitive one.

This is not to say that the debate on scientific norms which took place in the seventies was entirely futile. It has had the useful function of drawing attention to the contextual variation in normative behavior, and--more importantly--to the need for articulating the application of moral norms with the procedures of cognitive evaluation of evidence and argument. This

is a very important shift of emphasis and a definite broadening of the horizon of the field. Instead of choosing for investigation the most formally institutionalized aspects of science in which social control is most effective, sociologists of science are willing to look now at processes on the margins of institutional boundaries, and on the complex relationship between social control and cognitive evaluation in science, without which the study of social control in science was a somewhat abstract concern, far removed from the daily concerns of scientists.

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* (continued from preceding page)

cent (Toren, 1980). This latter can be regarded as a better item actually used for that purpose, since the "utility" item relates to the context of public judgement, the "disinterestedness" one to the context of individual decisions, in which one tends to be much more lenient towards considerations of personal circumstances, such as financial needs, or problems of finding employment.

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