Bernard Barber

Some Patterns and Processes in the Development of a Scientific Sociology of Science: Notes from a Sixty-year Memoir

It is *past time* for us to begin to write the history of the development of the field of social studies of science. Such a history, of course, would be a sociological history. Indeed, such a history, since it would be the history of an emerging and ongoing science, would be much like all our other studies of science. We would be studying ourselves as scientists the way we study all other scientists. We would look for the patterns and processes, and for their various social structural and cultural determinants, of how and when our specialty emerged from near non-existence into the complex and variegated maturing discipline it now is.

As a contribution to this history, I submit, in this paper, a discussion of *some* of the patterns and processes in this development that I have experienced in my own work and observed in the work of our colleagues. It hardly needs be said that caution on the part of my audience is called for. This is an *initial* effort by *one* scholar; as such, it cries out for correction and amplification (Barber, 1990). A satisfactory sociological history should be a continuing work-in-progress.

Here, then, are my provisional patterns, principles, and their determinants, for the development of the sociology of science.

The application of general social system theory

In the marvelous sociological training I had received as an undergraduate and graduate student at Harvard during the thirties and forties from Sorokin, Merton, Parsons, and L.J. Henderson, I had been most attracted, as a result of Parsons' teachings and writings by general social system theory (Barber, 1993). No wonder, then, that I decided, in my first work after leaving Harvard, to show the usefulness of that theory and, concurrently, to improve and extend it. A colleague and I hatched a grandiose plan to this, a plan which had two parts. The first part was to write a social system theory

Science Studies, Vol. 9 (1996), No. 1, 26—33.

treatise, a text *a la Samuelson* in economics, laying out a social system analysis of society filled not only with that analysis but with as much comparative empirical work from sociology, history, and anthropology as we could find. The second part, such was the grandiosity of our plan, was to write a full volume on each of the social structural and cultural components of the social system that we had dealt with in only a chapter in our treatise. Both in the treatise and the special volumes we intended to include analytic and empirical discussions of social problems and social change.

Unfortunately, the collaboration with my colleague on the treatise was aborted for personal reasons and we agreed that he should write the treatise and I would write a book on one of the many social structural and cultural components of society. I chose to do a book on science, and the result was my *Science and the Social Order* (Barber 1952). Using social system theory, the book discusses the functions of rationality and science in the social system, the development of modern science, its social organization, science in the two authoritarian countries — Germany and Russia, the social process of discovery and invention, the processes of social control in science, and (as a harbinger of my future work on the ethics of scientific research on human subjects) the social responsibilities of science. I ended with an optimistic account of the nature and prospects of the social sciences, an account which I think has been justified by developments in the social sciences generally and not least of all in our own specialty.

Why did I choose science for this first book on each of the several social structural and cultural components of the social system?[1] I did so to illustrate the usefulness of social systems theory, not because I hoped to start a new specialty in the sociology of science. I chose science because it was the component I thought I knew the most about. As an undergraduate and graduate student I studied the work on the social aspects of science by Sorokin, Merton, Parsons, and Henderson. I did an undergraduate paper in an American Intellectual History course on Jefferson as a scientist. Especially in my graduate years, I came to know and study with the great historian of science, George Sarton, and to know the younger historians of science who were to play a great role in the remarkable development in the history of science that has occurred in the last forty years: people like I. Bernard Cohen, Henry Guerlac, and Giorgio De Santillana. I had been much impressed by the work on the nature and history of science by President James Bryant Conant of Harvard.

Despite Robert Merton's especially brilliant work on Puritanism and science and his pioneering essays on the norms of science, the sociology of science did not exist as a recognized and teachable subject. In the broader field of the sociology of knowledge, which we knew well particularly from Karl Mannheim, science was omitted as a privileged and independent category. Merton and I remained pretty much isolated characters. In my book, although scores of citations were provided, those cited were also mostly isolated scholars, not part of dense inter-citation networks such as those we now know to be the clear sign of an active discipline.

The political and ideological response

As we know from such recent cases as the so-called "war on cancer" and the practice of gene therapy, the development of scientific specialties is affected by the circumambient political and ideological environment. Among sociologists, my book was not especially welcome for at least two reasons. First, sociologists had not been trained in the natural sciences and felt uncomfortable with it. Second, social system theory was not in great favor because of the widespread hostility to Talcott Parsons' work and to functionalism more generally. The more general political and ideological environment also played its part. An American anti-Communist scientist who had written a book about what he called "the death of science in Russia" all but dismissed my book as the work of a misguided Marxist. By contrast, when the book was published in England, where there was a group of leftists much influenced by the so-called British scientific humanists like Bernal, Hogben, and Haldane, a reviewer said, Professor Barber, "though an American," seems to know the facts of reality. That was because in my analysis of Russian science I had pointed out what was favorable to science in the Russian social system as well as what was unfavorable. Incidentally, one American who admired my analysis of Russian science was an official of the C.I.A., not so identified explicitly, who invited me to come to Washington to set up a group to study Russian science. Those being McCarthy days and I having been an undergraduate member of the American Student Union, I had to tell him that I would not be a suitable Government employee under the McCarthy rules.

The whole political and ideological environment in the United States changed, of course, with the Russian success with Sputnik in 1957. The American disdain for Russian science was abandoned and large resources were poured into the universities. The study of the history and social aspects of science was only one of the established and emerging scientific specialties in the natural and social sciences that profited hugely from this new and vast Government financial support and approval. Because of the changed atmosphere, because of the establishment of large numbers of courses in "science and society," there was a new market for my book and two large paperback printings were issued that far outsold the earlier hard cover edition.

The influence of non-systematic theories

Social system theory was not the only kind that I found useful in understanding the social aspects of science.

Various less systematically integrated pieces of theory were sometimes helpful. Because of his early theoretical work on the unanticipated consequences of social action, Robert Merton was fascinated by what he called "the serendipity pattern" in research, which occurs, as he said, when "the unanticipated, anomalous, and strategic datum exerts a pressure for initiating theory". (Merton, 1936; Merton, 1949: 99–101)

I was, of course, entirely familiar with this attractive piece of theorizing about the social processes of scientific discovery and, therefore, able to take full advantage of my knowledge when the opportunity offered. Such an opportunity did offer itself to my colleague, Professor Renee Fox, and me, when we learned, through a story in *The New York Times* and through personal acquaintances, that two distinguished medical research scientists at New York University and Cornell University Medical College, had both experienced, serendipitously, the "bizarre" and anomalous pattern of floppiness in test-rabbits' ears after the injection of the enzyme papain. One had gone on to make a minor medical discovery, the other, after some failed efforts, had gone on to others of his several research projects.

This seemed like a quasi-controlled experimental situation to us and we arranged long interviews with each of the two scientists to compare why one had succeeded and the other not. Both men had started out with erroneous preconceptions about cartilage, but the successful scientist, because he wanted to demonstrate the anomalous floppiness effect to his students, persevered. Eventually, through closely examining sections of the rabbits' ears, he saw that the cartilage was not rigid, as received knowledge had it. This discovery led on to his further work on cartilage. Not a great discovery, but typical of what happens frequently in scientific research.

We titled the paper we wrote about this work "The Case of the Floppy-Eared Rabbits: An Instance of Serendipity Gained and Serendipity Lost" (Barber and Fox, 1958).

Perhaps partly because of its catchy title, this paper was widely noted and reprinted.[1]

No theory at all; the importance of a striking empirical fact

Social science isn't always theory-driven. Sometimes a massive or striking empirical fact generates research which then calls forth explanatory theory. This was the case for my work on the subject of resistance by scientists to scientific discovery.

In my wide reading in the history of science, especially in scientific biographies and memoirs, I came to see a recurrent pattern, one in scientific discoveries were rejected for reasons apart from reasons of scientific substance or method. I decided to make a systematic inventory of examples of this pattern and look for some theoretical explanation. My research did turn up a number of examples that were new to me and I saw that such non-scientific reasons as philosophical preconceptions and status differences could explain the various cases of resistance. The most famous case, of course, was that of the resistance of von Nageli to Mendel's great discovery.

At the invitation of one of the editors, I submitted my paper on this subject to *Science* magazine (Barber, 1961). Partly because of this prominent place of publication, and partly because my subject struck a nerve among many scientists, this paper was perhaps the most widely noted of all my many published papers. I received more than 500 requests for reprints, letters that often gave alleged personal examples, even personal visits, one from a critic of Einstein who claimed to have been resisted.

Of course, for many of the tales I heard, I could give no judgment about the alleged resistance since I did not have the necessary details. But it was also clear that many of my readers did not get my point. I made it clear, I thought, in the article, that I was discussing only cases where initial rejection could be definitely attributed to specific sociological factors apart from legitimate substantive or methodological criticism. But in many cases, it seemed, the nerve I had struck was that connected with the difficulty many scientists have even with legitimate scientific criticism. What my experience in this cases showed was that, for all the power of the norms of objectivity and neutrality in science, working scientists put a great deal of passion into their work and resent even legitimate criticism.

The importance of new research methods

In the 1970's some valuable research methods were newly introduced into the sociology of science, where they have flourished ever since. Using the focused interview method and the survey research method that had been strongly developed and emphasized at Columbia University's Bureau of Applied Social Research by Professors Paul Lazarsfeld and Robert Merton, my younger colleagues at Columbia, Jonathan and Stephen Cole and Harriet Zuckerman, brought new data and new analysis into the study of social stratification in science (Cole and Cole, 1973) and the origins, patterns of work, and problems of the scientific elite represented by the Nobel laureates (Zuckerman, 1975). The Coles also pioneered in the sophisticated use of citation analysis, a method of seeing various social patterns in science that, despite recognized weaknesses, has become a valuable research tool for the sociology of science. The sociology of science has a large debt to Eugene Garfield's Institute of Scientific Information which provided the data for citation analysis and for much else besides in the processes of effective communication among scientists.

I was sufficiently attracted by these new methods that I resolved to use them myself in some new research on the ethics of medical research on human subjects. With a group of able younger colleagues, John J. Lally, Julia Loughlin Makarushka, and Daniel Sullivan, all well trained in the new methods, I published *Research on Human Subjects: Problems of Social Control in Medical Experimentation* (Barber, 1973; Barber, 1990: Part III), which reported the results of several large studies of these matters. I will discuss this and related work further, below.

In this section I must also pay tribute to pioneering, innovative quantitative work on the social processes of science by Professor Derek J. deSolla Price. Derek Price taught us all how indispensable quantitative data could be in our field.

The weakness of "the strong program"

Another major and exciting movement that occurred in the sociology of science beginning in the 1970's and that has lasted to the present is the movement that labelled itself "the strong program". This was a movement that came out of Great Britain, particularly out of Edinburgh, but that has spread to Germany, Holland, France, and also the United States.

It has had several features that attracted my admiration. First, in the writings of David Bloor, a professional philosopher who took up a Wittgensteinian position, it has had a strong philosophical base, albeit one that was ultimately relativist. Second, it has been carried forward by the research and writing of an exceptionally able and energetic group of social scientists: Barry Barnes, David Edge, Michael Mulkay, Harry Collins, Trevor Pinch, Karen Knorr-Cetina, Wiebe Bijker, G.N. Gilbert, Steven Shapin, Donald MacKenzie, Bruno Latour, Steven Woolgar, and others. Third, these scholars made intensive studies not only of the social organization of science but of the substantive ideas of science. This overcame a shortcoming I had felt in my own work. Not trained in science, I had to limit myself, with a few exceptions like the resistance paper, to studies primarily of its internal social organization and its place in the larger social system of society. These new people had, in some cases, like David Edge, formerly been practicing scientists. The others worked seriously and intensively at understanding the substance of scientific ideas. Finally, I admired the energy of this group: they organized programs to train students, they started and have edited successfully what is the premier sociology of science journal (*Social Studies of Science)*, they called conferences and were diligent attenders and presenters of papers about their work at all relevant meetings, and they organized new groups to discuss their work. Every scientific specialty eventually needs some committed members to do all these tasks, and "the strong program" group took them up with energy and resulting success.

For various reasons, however, I early came to feel that these strengths of "the strong program" went along with over-riding weaknesses. First, it had a strong strain of ontological relativism about science; in many cases, their work seemed to deny the possibility of an objective, cumulative science. And presumably, since they stressed the principle of reflexivity, this meant that their own work had no objective scientific standing. Second, as a sociologist, I found unsatisfactory their usual attribution of the determinants of scientific work to the broad and vague concept of "interests". Insofar as "interests" were specified, they seemed to be only political and economic ones, not interests stemming from strongly held values or from the cognitive concerns of working scientists. Implicit in this exclusive emphasis on political and economic interests, I came to feel, were definite anti-authoritarian values and ideologies. Paradoxically, social scientists with strong anti-establishment values were denying the importance of values and norms.

In sum, I believe that "the strong program" group has strengthened the conviction among us sociologists of science that not only the organization but also the substantive ideas of science are *in part* determined by a whole variety of social structural, cultural, and personality factors. But it is clear that their tendency to relativism and their inadequate sociology will not do to carry this important premise of all our work forward.[3]

The cross-fertilization of sociological specialties

The sociology of science has been enriched by its cross-fertilization with other sociological specialties. As a result of my early and continued experience with the ground-breaking teaching and writing of Talcott Parsons in the sociologies of medicine and the professions, I had myself made some contributions in those fields. But I was struck, especially in the sociology of medicine as much as in the sociology of science itself, with the lack of attention to the substantive scientific data of the field. As a result of some work I did in collaboration with Professor Renee Fox at a pharmaceutical firm, I became interested in the sociology of drugs and went so far as to make up a tentative table of contents for a book on that subject. In one of my many visits to the stimulating informal lunch-seminars held regularly by Bert Brim, then the President of the Russell Sage Foundation, he said, most presciently, that the subject of drugs was going to be one of the most important in the future. When I told him about my book outline, he offered to support my writing it. I eventually accepted his offer, and the final product brought in the substantive science as well as the problems of organization in the drug field (Barber, 1967).

In addition to such topics as discovery and testing processes and education and communications processes, I discussed the functions and problems of the professional specialists who did research on new drugs. One of the problems I discussed was the questionable ethics of using human subjects in some of that research. Mine was a pioneering treatment of that problem, but because of a couple of national scandals, the use of human subjects without their informed consent and without due consideration of a satisfactory risk-benefit became a hot political and social topic. In my attempt to analyze the problem from a social science point of view and collect sound data on the extent and sources of the problem, I was given further support by the Russell Sage Foundation. One result, mentioned above, was what we called the "Two Institution Study" published in the book *Research on Human Subjects* (Barber, 1977). Our key explanatory hypothesis was what I called "the dilemma of science and therapy," and the findings from our survey and network data analyses, showed the strength of that hypothesis.

Another result of this work was that, for some years, I became active in the public and political events aroused by the concern over the use of human subjects. I testified in U.S. Senate hearings, I became a member of committees drafting new rules for professionals, I addressed many professional groups, I was asked to check some foreign research projects on reproductive biology that had been subsidized by the Ford Foundation, and I became a member of an Institutional Review Board at a medical research institution, the kind of review board that is now mandated for all research institutions using human subjects. In all of this activity, thus, I was playing my small part in the development of what I consider a valuable moral improvement in the conduct of medical scientific research.

The cross-fertilization of disciplines

Another valuable enrichment of the sociology of science has come from its cross-fertilization with the history of science and the philosophy of science. All three of these disciplines were very small and mostly separate when my *Science and the Social Order* was published forty-odd years ago. All three have not only grown considerably since then but have become increasingly intertwined. I have already mentioned the importance of David Bloor's philosophical writings for "the strong program". Perhaps the most influential book for our field that brought together the history, philosophy, and sociology of science was Thomas Kuhn's *The Structure of Scientific Revolutions* (Kuhn, 1962)

The cross-fertilization of these three disciplines has not only served to show the special and different contribution that each has to make to our understanding of science but has improved each of them by showing both their limitations as against the others but also their necessary interactions (see Barber, 1990). For example, much of the history of science now uses sociological theory to explain what it formerly only described, and sociologists have learned to avoid imperfect popular history and to use and even occasionally create accounts that meet professional historical standards. On their part, philosophers of science have profited from knowing the best of the new history and sociology of science.

The importance of values and ideologies

Perhaps the most striking example in the recent sociology of science of the importance of values and their associated ideologies, perhaps the most direct critique of "the strong program's" assertion that only political and economic interests affect the development of scientific ideas, is the vitality and burgeoning the feminist movement in our field. This movement has raised questions about such important matters as whether women scientists create different kinds of scientific ideas than men do, about their careers, and about their productivity. The sociology of science has been enlarged and stimulated by this movement (Zuckerman *et.al.*, 1991).

In brief conclusion

How do I sum up? How do I account briefly for my long journey in the sociology of science and for its present state? In a phrase, *This is not where I came in.* The sociology of science has made enormous progress in the last forty-odd years and is now a relatively mature and flourishing specialty. It has achieved this state as the result of a set of diverse, multiple, and interactive theoretical and other social structural, cultural, and personality factors. Political and economic interests are only a few of these important determinants. We do not need to be ontological relativists about science and its development. Science is an essential functional component of the culture of all societies and has its own degree of autonomy as well as its dependence on all the other functional components of the social system. On these philosophical and sociological premises, we can go forward to a continuing theoretical, moral, and practical success.

NOTES

[1] As a further step in the plan for social system theory, in 1957, I published *Social Stratification: A Comparative Analysis of Structure and Process.*

[2] For some other examples of the valuable results of pieces of theory, see (Merton, 1961) and (Merton, 1968).

[3] For a powerful critique of the assumptions and substance of the work of "the strong program" see (Schmaus *et. al.*, 1992). A very important additional reference in connection with "the strong program's" weaknesses is (Cole, 1992) Cole's book, I feel, offers the best way forward for the sociology of science. Finally, an absolutely indispensable discussion on the subject of "truth and objectivity" is the two-part review by Paul Forman in *Science* (Forman, 1995a; Forman, 1995b).

REFERENCES

Barber, Bernard
1952 Science and the Social Order. Glencoe, Ill.: The Free Press.

Barber, Bernard
1957 Social Stratification: A Comparative Analysis of Structure and Process. New York: Harcourt Brace.

Barber, Bernard and Fox, Renée
1958 "The Case of the Floppy-Eared Rabbits: An Instance of Serendipity Gained and Serendipity Lost." American Journal of Sociology LXIV: 126–136.

Barber, Bernard
1961 "Resistance by scientists to scientific discovery" Science 134: 596–602.

Barber, Bernard
1967 Drugs and Society. New York: Russell Sage Foundation.

Barber, Bernard
1975 Research on Human Subjects: Problems of Social Control in Medical Experimentation. New York: Russell Sage Foundation.

Barber, Bernard
1987 "Big Science." Isis 78 (no. 294): 589–591.

Barber, Bernard
1990 Social Studies of Science, New Brunswick, N.J.: Transaction Books.

Barber, Bernard
1993 Constructing the Social System. New Brunswick, N.J.: Transaction Books.

Cole, Jonathan R. and Cole, Stephen
1973 Social Stratification in Science. Chicago: University of Chicago Press.

Cole, Stephen
1992 Making Science: Between Nature and Society, Cambridge, MA.: Harvard University Press.

Forman, Paul
1995a "Part I: Irony." Science 269: 565–567.

Forman, Paul
1995b "Part II: Trust." Science 269: 707–710.

Kuhn, Thomas
1962 The Structure of Scientific Revolutions. Chicago: The University of Chicago Press.

Merton, Robert K.
1949 Social Theory and Social Structure. Glencoe, Ill.: Free Press.

Merton, Robert K.
1961 "Singletons and Multiples in Scientific Discovery," American Philosophical Society, Proceedings 105: 470–486.

Merton, Robert K.
1968 "The Matthew Effect in Science: The Reward and Communications Systems of Science," Science 199: 55–63.

Merton, Robert K.
1986 "The Unanticipated Consequences of Purposive Social Action," American Sociological Review 1: 894–904.

Schmaus, Warren, Segerstråle, Ullica and Jesseph, Douglas
1992 "A Manifesto on 'The Hard Program' in the sociology of scientific knowledge." Social Epistemology, 6(no.3): 243–265

Zuckerman, Harriet
1975 Scientific Elite: Nobel Laureates in the United States. New York: Free Press.

Zuckerman, Harriet, Cole, Jonathan R. and Bruer, John T. (eds.)
1991 The Other Circle: Women in the Scientific Community. New York: W.W. Norton.

Bernard Barber
145 Central Park West, 15E
New York, New York 10023
USA