Science as a Social System

Norman Kaplan

Until recent years, our understanding of the social patterns of science came mainly from the intuitive reflections of the scientists themselves. Lately, a few social scientists have taken the first steps toward a more systematic exploration, and now Norman W. Storer, in **The Social System of Science** (Holt, Rinehart, and Winston, New York, 1966. 192 pp., illus. Paper, $3.95), tries to help us on our way by providing a theoretical framework for such efforts. It is a serious, often ingenious attempt which merits our attention.

Storer develops a model of the operation of the social system of science. He defines a social system as a "stable set of patterns of interaction organized about the exchange of a qualitatively unique commodity and guided by a shared set of norms that facilitate the continuing circulation of that commodity" (p. 75). This is an "exchange" model, an example of a kind of analysis that has achieved a considerable vogue in recent years among some social scientists. The clearest analogue is the economic system, in which money is the commodity being exchanged for goods and services. Science is seen in essentially the same terms, but the commodity unique to science is "competent response" (professional recognition), which is being continually exchanged for the scientist's creative products.

Exchange relationships in all social systems are governed, according to Storer, by three basic principles: (i) The participants must *want* the commodity in question. (ii) The commodity must be obtained through *exchange* with others. (iii) The participants must not compromise the different exchange systems; that is, they must employ the behavior appropriate for obtaining *the particular system's unique commodity* (or "reward," to use a less neutral term).

Creativity plays a central role in this model. It is assumed that the urge to be creative is widespread and continuing (perhaps even innate) and that the satisfaction of this urge (drive) requires exchange with others. In fact, competent response is seen as the essential last step which completes the creativity cycle for the individual participants.

Of several norms of science that Storer describes, he links three in particular—communism or communality, organized skepticism, and disinterestedness—to his three basic principles. The norm of communism obliges the scientist to share his findings with others and enjoins him against secrecy. The publicity he accordingly gives to his creative efforts makes it possible to get competent response from others and supports the notion, expressed in the first principle, that the participants do, in fact, want such a commodity. Organized skepticism, which directs the scientist to verify and validate the work of others as well as his own, and not to accept anyone's work on the basis of faith or authority alone, promotes the exchange required by the second basic principle. Disinterestedness, defined in a rather unique fashion by Storer, makes it illicit for the scientist to profit personally from his research, and accords with the third principle by ruling out other possible commodities (rewards), such as money, fame, or status. This norm is also viewed as crucial to the scientist's choice of problems. It restricts him to choices based solely on scientific criteria and thus promotes the behavior that insures the continuing circulation of the unique commodity.

This constitutes the bare bones of the model. Science is seen as a viable social system whose members share a set of norms and values which enable them to maintain stable relationships, as well as a distinctive incentive or reward which motivates them to continue their participation in the system. Storer explicitly recognizes the inherent difficulties of proving his model true or false. His hope is that it helps us to understand the scientific community and, especially, to explain "certain of the more puzzling social practices among scientists and . . . the major 'social problems' that seem to beset science, as a whole" (p. 100). Storer discusses a number of such problems, and it is these analyses which provide the first crucial, if incomplete, test of the adequacy and utility of the model. Taking the first two "social problems" Storer has singled out, let us consider how well the model withstands a test.

Professional Recognition

Consider the problem of the scientist's ambivalence toward professional recognition. While professional recognition is satisfying, it is not something one should solicit openly. One's published work should speak for itself, but if by some misfortune it doesn't, it is not legitimate to go out and hire a public relations firm. As we have seen, professional recognition is a key element in Storer's model. He says at one point: ". . . that the scientist *should* want professional recognition is supposedly binding upon all scientists. It is an aspect of the rights and obligations that they have accepted on coming to occupy the social position of 'scientist' " (p. 21). Why, then, should scientists be ambivalent toward it? Storer's answer is that there are actually two distinct commodities, only one of which is legitimate in the context of the system. The competent response to a genuine, new contribution to the body of scientific knowledge is the legitimate commodity. But there is a tendency, he says, to substitute illegitimate criteria, such as authority and prestige, in evaluating the new work of those who have already established solid reputations. This illegitimate offshoot of professional recognition is a threat to the system and therefore encourages ambivalence.

To begin with, I question whether the fact of ambivalence, let alone the precise circumstances in which it might occur, has yet been firmly established. Merton, upon whose work Storer leans heavily, has suggested that such ambivalence is characteristic of science. But, despite the thoroughness of his analysis, Merton was dealing with the great or near-great of a population the majority of whom simply toil in the vineyards of science, far from the Olympian heights. This is a persistent and troublesome problem for all who study science and scientists. Much of our knowledge about creativity, priority fights, ambivalences, commitment to values and norms, the choice of research problems, and so forth is based on historical information about the comparatively small number of men

The author is a professor of sociology at the George Washington University, Washington, D.C.

who have achieved greatness. The recognition they have hoped for or been accorded is surely qualitatively different from that which can be attained, or even aspired to realistically, by the vast majority of scientists in any age.

The problem is, in part, one of defining the "proper" audience for one's work. There is a proper, in the sense of legitimate, audience which is institutionally defined. As a first approximation, this audience consists of one's fellow scientists working in the same or quite closely related areas of specialization. Within these groups, the responses of some will be institutionally defined as more relevant than others.

There is a certain asymmetry involved which appears to be difficult to fit into Storer's notion of exchange. One man's papers may be such that virtually everyone else working in the same area can offer competent responses. The contribution of another man might be acknowledged by only one or two others whose own work is most closely related and affected. Moreover, how does one compare a pat on the back from one very able scientist to the plaudits of ten lesser men? What is the "fair share" of response Storer talks about?

Consider the difference between the theoretician's work and that of the experimentalist or empirical researcher. For a new theoretical scheme in a comparatively new field (not unlike Storer's own work), in which there is as yet no empirical basis for the acceptance or rejection of that scheme, accrued prestige and a host of other factors may help to determine the initial reaction. For a piece of empirical work which stems from, and contributes to, a generally accepted theoretical framework, the response can more readily be made to the work itself. The experiment can always be replicated. Only in the very short run, if at all, is anyone's empirical contribution accepted wholly on the basis of accrued prestige. The great men, as well as the unknown, can be, and presumably are, called to task for poor research. A new evaluation of a new piece of work is one thing; the effect of each new evaluation on the evaluation of a man's total efforts is another.

Basic and Applied Research

Storer rightly points out that the distinction between basic and applied research is invidious. He sees the antagonism as arising from the dynamics of the social system and, in particular, from the norm of disinterestedness. Storer assumes that the only relevant audience for the applied scientist is made up of nonscientists—in the industry that employs him—who, by definition, are not competent to provide the institutionally appropriate response and, instead, offer money and other nonsystem rewards. He neglects to consider that the applied scientist may simultaneously have a scientific audience whose response he seeks and receives.

But there is a more basic difficulty, which stems from Storer's very restrictive view of the nature of science. "Ideally," he says, "the scientist is an individual who spends his working day in basic research and related activities: gathering information, reporting his findings, evaluating the work of other scientists, and so forth. He is engaged directly in the attempt to extend our generalized knowledge of some aspect of the empirical universe" (p. 14). He admits there are very few such people in the real world, but by stretching a point here and there he estimates that there are around 50,000 such scientists in all fields in the U.S. On the other hand, he feels that, with some exceptions, it is the scientists in the universities who are the "purest," and he notes that only about half the 33,000 U.S. scientists engaged primarily in basic research are in universities.

This restrictive view places Storer in what I find to be a quite untenable position despite his nimble defenses. Disinterestedness is explicitly defined as a norm supporting the "purity" of basic research. By definition, only scientists in universities support this norm. But, as Storer notes, "inasmuch as less than one-third of all those in America who can be classified as scientists are in the universities, their only defense against this *apparently massive violation* of the norm of disinterestedness is to point to the moral failure involved . . ." (p. 112, my emphasis). It is of the essence to any consideration of social norms that violations will be viewed as morally wrong. But at what point do we admit that the norm in question is probably no longer a viable one? Are the two-thirds who apparently do not subscribe the deviants? Should we not begin to distinguish between the perceptions of the participants and an objective analysis of the actual consequences which flow from the distinction?

The fact is that we know very little about how and why any scientist, basic or applied, chooses the problems he does. Whether he hopes to make a contribution to the whole of science or hopes to profit personally may be of some significance in many contexts but presumably not to his behavior as a scientist. Having made a choice, he is constrained to conform to the technical canons of science; he cannot, for example, fudge his results whether he aspires to be another Newton or another Croesus.

A competent response is tied to "widespread agreement among scientists as to what constitutes valuable work . . ." and Storer frequently implies that only generalized contributions to the whole of science are *really* valuable. But the kind of contribution which is possible is often a function of the state of the field, the instruments and other empirical techniques available, developments in neighboring fields, and a host of other still dimly perceived factors. It would be nice if we were all Newtons, but we are not; and it is not even clear that the Newtons could emerge from time to time without the massive and yet minute efforts of the rank and file.

While Storer does manage to incorporate some aspects of the "basic-applied" problem within his scheme, it is not without considerable toil and trouble. More important, our understanding of this problem seems not to have been advanced substantially. One can't help wondering how we managed, in the space of less than three centuries, to move from the pursuit of science for the glory of God and the benefit of man to the pursuit of "pure" science for its own sake. The search for rationales satisfactory to scientists and to the society at large may shift over time and may well be an important ingredient in determining the health of the enterprise, but this is surely not the only factor. It is necessary to distinguish the motivations which have social approval at any given time, but which are essentially *external* to the system, from motivations which are internal.

Despite these shortcomings in his model, the problems Storer has chosen to explore are of enormous interest in their own right. In directing our attention to them in such a thought-provoking fashion, he should stimulate some much-needed empirical work as well as further attempts at theory.