The Hard Sciences and the Soft:

Some Sociological Observations*

By Norman W. Storer, Ph.D.†

Staff Associate
Social Science Research Council
New York, New York

ABSTRACT

The paper focuses on the implications of the terms "hard" and "soft" as they are used to characterize different branches of science; this is one approach to understanding some of the relations between knowledge and social organization. Given the importance to scientists of having their work evaluated accurately, it can be seen that the more rigorously a body of knowledge is organized, the more readily professional recognition can be appropriately assigned. The degree of rigor seems directly related to the extent to which mathematics is used in a science, and it is this that makes a science "hard." Data are presented in support of the hypothesis that "harder" sciences are characterized by more impersonality in their members' relationships where impersonality is indexed by the frequency that only first initials are used in footnotes. Finally, some parallels between the economic and the scientific sectors of society are suggested, viewing money and professional recognition as "generalized media" and noting certain analogies in science to inflation and deflation in the economic system. Implications for the obsolescence of parts of the literature of science are discussed, and the relevance of this analysis to Kuhn's work on scientific revolutions is briefly noted.

Introduction

At the outset, let me make clear that sociology is *not* a "hard" science. What I have to say in the following pages will be more concerned with developing a sociological perspective on science than with the demonstration of laws of social behavior. I am interested primarily in the development of a conceptual framework within which science may be fruitfully viewed as a social phenomenon; the few facts that I shall present later on will be useful only insofar as they support my contention that this perspective can, indeed, have some utility.

I have chosen to discuss "The Hard Sciences and the Soft" for two reasons. First, I think the topic affords an interesting opening into the exploration of the relationships between two aspects of science: its body of knowledge and its organization as a community of human beings. We are well aware of some of the influences each of these has upon the other, such as that obstacles to communication among scientists will hinder the extension of knowledge, or that the way scientific knowledge is organized has definite consequences for the grouping of scientists into departments, laboratories, and professional associations. My concern, however, is with a more subtle aspect of these relationships.

I am interested in the differences in the *qualities* of social relationships in the different sciences, or, perhaps, in the "atmospheres" or "moods" that characterize different fields of science, and with how such differences produce visible consequences that may be of particular interest to you as librarians. As will become perhaps clearer later on, I think that our use of the terms "hard" and "soft" is somehow related to these aspects of scientific disciplines, and that these are in turn related to the organization of knowledge within these different fields.

My second purpose, related to the first although perhaps more didactic, is simply to demonstrate that there is frequently more good sociological sense in the ways that people talk about their social world than my colleagues and I sometimes care to admit. Through this analysis of the meaning of hardness and softness, I hope to get at some of the social realities that

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† Formerly Assistant Professor of Sociology, Department of Social Relations, Harvard University, Cambridge, Massachusetts.

are represented, like icebergs, by the small parts of them that project above the surface of our awareness and into our everyday language.

The Problem

Let us begin by looking at some of the connotations of the words "hard" and "soft." These terms are obviously expressive in some way of something about different fields of science: we think of physics as hard and of political science as soft; we think of chemistry as being harder than zoology, and of sociology as being somewhat softer than economics. But what is it we sense about these different sciences that makes it appropriate for us to assign these adjectives as we do? To begin by examining connotations may not be a very "scientific" way to commence, but my experience has been that there is often some sociological fire behind such smoky, informal language, and that it is worthwhile to grope for it—even at the risk of burning our fingers.

"Hard" seems to imply tough, brittle, impenetrable, and strong, while "soft," on the other hand, calls to mind the qualities of weakness, gentleness, and malleability. In more personal terms, "hard" suggests impersonality, aggressiveness, and a sharp concern for the letter of the law, while "soft" implies sympathy, warmth, and informality. Going still further, we find that a "hard" job is one that is difficult or laborious, and that "soft" jobs are those that are easy, not demanding of great effort.

Somewhere among these various connotations, I think we will find the key to why we feel it appropriate to say that biochemistry is hard and psychology is soft, or that genetics is hard and anthropology is soft. The immediate explanation that comes to mind, of course, is that a hard science is one that requires more effort to learn. Physics presumably requires more concentration, more hours of homework and laboratory exercises, than does sociology if one is to earn an "A" for the semester. However, I am not satisfied that it is simply the relative difficulty involved in mastering different subjects that accounts for the way we employ these adjectives; for one thing, the *reasons* for the relatively greater difficulty in learning physics have not yet been explained. These reasons will be involved in the explanation that I shall propose in a few moments.

The Context of the Problem

Having set up this puzzle, let me leave it in abeyance for a while in order to talk about some of the dynamics of the scientific community that have already been investigated by sociologists. Here we shall delve briefly into the current state of what we understand about the social structure and the moving forces within science, attempting to build a context within which our puzzle can take on broader meaning. What are some of the general statements we can make about the scientific community?

We can say, first of all, that science is primarily a *nonservice* profession. By this, I mean that the essential *raison d'être* of the scientist does not lie in his providing specific services to individual clients. If anything, his services to the lay public are indirect, for he is interested mainly in services to his colleagues: services in the form of the contributions he makes to a body of knowledge. These services are to be distinguished from those rendered by the service professions, like law and medicine, for they are not provided "on demand," nor are they important in helping individuals to resolve personal problems in the way that medical aid or legal counsel is. Rather, as Warren Hagstrom has put it in his book on *The Scientific Community* (1), these services are better seen as gifts given to the scientific community as a whole.

Just as the giver of a gift receives thanks in return, as well as probably another gift sometime later on, so the scientist receives the thanks of his colleagues in the form of professional recognition, as well as their own subsequent contributions to his field. Professional recognition, though, is not simply a type of gratitude; it is more accurately seen as a critical, public evaluation of the contribution. That this is defined as "thanks" by the scientist gives us a clue to its importance to him, and Robert K. Merton of Columbia University has spelled this out clearly through his analysis of conflicts over priority that have occurred throughout the history of science (2).

Priority in discovery is obviously necessary if one is to receive credit for it—to receive professional recognition—and Merton has proposed that professional recognition is the appropriate reward for achievement in science. To be sure, the scientist also receives a salary, but this is rarely, if ever, based entirely upon the number or the significance of his contributions. And just as man does not live by bread alone, so the scientist does not live by salary alone. In varying degrees, and often with considerable ambivalence, the scientist seeks professional recognition. The formal acknowledgment by his colleagues that he has indeed made a significant contribution to his field is the legitimate reward for successful performance as a scientist, just as profit in dollars and cents is the legitimate reward for successful performance as a businessman, or as votes are the proper reward for successful performance in politics.

So we have within science a kind of coinage that, while different from monetary units, serves much the same functions for scientists that money serves for businessmen. Recognition is a kind of abstract, scarce commodity that circulates within science and serves as an incentive for scientific effort. I cannot claim that this discovery of the motivational importance of recognition is by any means new, for scientists themselves have certainly been aware of it for centuries, and even we sociologists have been objectively aware of it since Merton brought it to our attention nearly ten years ago (2). If perhaps some of you have seen my recent work in this area (3), you will know that I differ with Professor Merton in certain respects concerning the theoretical significance and the basis of scientists' interest in professional recognition—but this need not concern us here.

My point is that professional recognition—competent, objective evaluation of a scientist's contribution that is also favorable or even laudatory—lies at the center of the "social dynamics" of science. It is the desire for it that produces occasional fads in science, that keeps a man struggling with a research problem even after repeated failures, and that may sometimes tempt him to fake his data or to plagiarize another's ideas so that he can get into print with them first. I do not mean that this desire is a simple expression of egotism or a lust for glory, but that it serves as needed confirmation that the scientist has indeed been creative, that he has been successful in what he was trying to accomplish.

It is, incidentally, the possibility that professional recognition might become mere flattery rather than objective appraisal that seems to produce such touchiness in scientists over matters of apportioning recognition fairly, or over the question of whether to fight for one's priority when one feels it is rightfully his. To make an issue of *who* receives *how much* recognition is implicitly to admit that one desires it for himself, and thus to raise the possibility that others might bestow it simply as flattery. This would undermine the trustworthiness of recognition, the objectivity that makes it meaningful, and in effect would inflate the currency until it was valueless.

Evaluating the Scientific Contribution

Given the objectivity of the appraisal, the receipt of professional recognition depends, then, upon the standards that one's colleagues use in evaluating a contribution. It is the nature of these standards that is of immediate concern to us now. What are the criteria by which one scientist's work is judged to be worthy of a Nobel Prize, that of another is accepted, but without marked enthusiasm, and a third scientist's contribution is peremptorily rejected by all journals in his field? Much as we might like to believe that there are universal, absolute standards for such judgments, we must still admit that it is *men* who apply them, and that they can only learn them from other men. In other words, these standards are products of the scientific community and are, thus, amenable to the kind of analysis in which we are presently engaged.

Herbert A. Shepard has given us a rough ordering of the relative amounts of recognition that are accorded different types of contributions. Writing in 1956, he pointed out that

> Highest honors go to those whose work involves radical reformulations or extensions of theory or conceptualization. [Copernicus, Newton, and Einstein are apt examples here.] Next come those who do the pioneer experimental work required by a theoretical reformulation. [In this category we might place, for instance, Tycho Brahe and Niels Bohr.] Next come those who carry out the work logically required to round out the conceptual structure. [Perhaps Henry Cavendish would fit here.] Next come those who carry out redundant experimental work of a confirmatory nature, or concern themselves largely with relevant data accumulation. Last are the doers of sloppy or dull work (4).

We must ask again, though, how it is that a scientist's colleagues can tell when he has been successful in his efforts to make a contribution at any of these levels. Rather than go into a long discussion of Mill's canons of proof or of the hypothesis-experiment cycle, however, let us remain on a more analytical plane and look at the general process of evaluating a contribution.

The aim of science, Merton has said, is "the extension of certified knowledge." Knowledge refers essentially to a set of symbols that are organized so that the meaning of each symbol is supported by the others. Further, the relationships among these symbols must be such that, ideally, no logical contradictions among them are produced by the rules that govern their relationships. That is, while the symbols making up scientific knowledge refer primarily to events and to their relationships with each other in the "real world," there do exist rules that enable us to relate these symbols to each other so that they constitute something more than just a congeries of separate statements. To judge the goodness of a contribution to knowledge, then, requires not only that we find it to be a valid representation of empirical phenomena, but also that we be able to relate it to the established set of symbols representing what we already know about these phenomena.

If a new contribution cannot somehow be related to what we already know, we shall certainly be unable to estimate its significance, even though we *can* determine whether or not it is empirically valid. And without being able to assess its relationship to or consequences for what is already known, we cannot be sure of how much professional recognition, if any, its contributor deserves.

It is here that the rules governing the relationships among these symbols are extremely important, for they may be more or less "rigorous." Both the precision with which a contribution fits into an existing body of knowledge, and the specific implications it has for existing knowledge, are ultimately functions of the amount of precision that characterizes these rules. For example, if our present body of knowledge contains the statements, "$2 + 2 = 4$" and "$3 + 3 = 6$," it is relatively easy for all to agree that the man who contributes the finding, "$2 + 3 = 5$," has made a valuable contribution. On the other hand, there will be virtually complete agreement among his colleagues that the man who claims to have discovered that "$2 + 3 = 6$" has made a serious error of some sort, either a technical mistake in his laboratory or else in interpreting his data. It is highly unlikely that a controversy will arise over which of the two contributions should be accepted.

In a field like sociology, however, the rules governing the relationships among concepts are by no means so precise or widely shared. We know pretty well, for instance, that working-class Catholics are quite likely to vote Democratic, and that upper-class Protestants tend ordinarily to vote Republican. But when a sociologist reports his research to show that *upper*-class Catholics tend to vote Democratic we have no logical reason to question his findings, any more than we would have grounds for questioning research reporting that upper-class Catholics tend to vote *Republican.* The first two statements give little or no guidance to help us assess the probable validity or significance of either of these latter contributions. Both, in other words, can be accepted into the body of sociological knowledge without creating internal contradictions, because the rules of organization here are relatively imprecise.

After these admittedly exaggerated examples, my conclusion should be obvious. I am proposing that the use of mathematics in a science provides a greater degree of precision in organizing its body of knowledge and, thus, a "tougher" set of criteria for the evaluation of new contributions. This is not to say that the organization of any body of knowledge is watertight, for the use of mathematics is a matter of degree rather than of black or white; I am suggesting only that there are appreciable differences among different fields of science in terms of the rigor with which their bodies of knowledge are organized.

Parenthetically, I might add that a second aspect of organization is relevant here as well. This is the degree of complexity of the materials contained in the body of knowledge, which in human terms means the amount of "real-time" required to follow out and to grasp the logical relationships among several different facts. We tend to think of symbol systems as timeless, so that logical relationships exist instantaneously. But it does take time to work them out, and when it takes too long the sense of overall integration may be lost simply out of exhaustion. An illustration of how long it can take for men to become aware of logical inconsistencies is to be found in the reception of Copernicus' heliocentric theory. This was first announced in 1543, but the fact that it was logically inconsistent with certain aspects of Thomist cosmology did not become apparent until much later; in fact, it was not until 1616, some seventy-three years later, that *De Revolutionibus* was actually placed on the Index (5).

A less well-known but perhaps equally relevant example is the extent of disorganization present today in the field of mathematics, a situation apparently brought about by increased specialization and the extraordinary length of the "chains of logic" necessary to link the different specialties (6).

"Hardness" as Rigor and Impersonality

At this point we may return to our interest in the hardness and softness of different fields of science. At an intuitive level, we can begin to see something of why it is so fitting that physics is termed a hard science and sociology a much softer one. I am suggesting that, through some faculty of folk-wisdom, we have hit upon a way to characterize different branches of science in terms of a continuum that measures essentially the tightness of integration of their various bodies of knowledge. "Hardness" in this sense implies much more than relative difficulty in mastering a subject; it suggests also the degree of difficulty involved in making a contribution to the subject and, thus, the degree of risk a scientist takes when he offers a contribution. If a hard science is one in which error, irrelevance, or sloppy thinking is relatively easy to detect, then the scientist must take greater pains in his research if he does not wish to be exposed as incompetent.

In the softer sciences, on the other hand, where such a high level of rigor is lacking, it is likely that such nonscientific criteria as relevance to common values or to practical problems, elegance of style, or even the unexpectedness of one's findings vis-à-vis common sense, will play a larger part in determining the acceptance and success of a contribution.

How does the relative hardness of a science affect the social relationships among its scientists? As a sociologist, I am particularly interested in this sort of question, and on the basis of the foregoing reasoning I undertook to develop an hypothesis and to collect some data to test it. Thus, if we reason that there is more risk involved in contributing to a hard science than to a soft science because one's colleagues can more easily identify any weaknesses in one's work, it might be that one would feel less "close" to these colleagues in the sense of warmth and trust. It is relatively easy for them to "hurt" you. We would reason, in other words, that because of the conditions under which men must work in the hard sciences, we will find a greater degree of impersonality there than in the soft sciences.

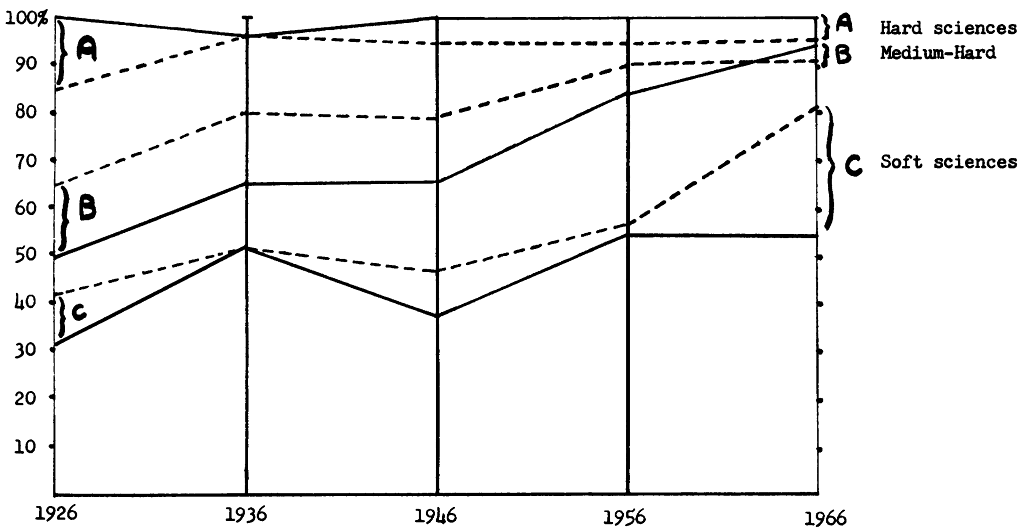
I decided to measure impersonality in a crude way by finding whether, in citing other scientists' work, the author of a research report has used their first names or only their initials. Let me say a word about the implications of this distinction. On an intuitive basis, "M. Brown" seems less personal, less "close," than does "Morris Brown." The use of only a man's initials seems to suggest that they are used merely to distinguish M. Brown from T. Brown and carry no implication that the author is personally acquainted with M. Brown. Only in rare instances are a man's initials used as a personal nickname; more often, I think, we view the use of initials as a means to help us determine who someone is *not*, rather than to tell us who he *is*. The fact that some journals exempt women from their policy of using only initials in footnotes seems to give further support to the idea that using only initials is indicative of a certain degree of "social distance."

It was on the basis of this sort of reasoning that I hypothesized—with, I admit, some foreknowledge—that the use of "initials-only" footnotes would be more prevalent in the hard sciences than in the soft. Here, then, was one variable to be measured. The other came distinctly closer to my characterization of "hardness," namely, a measure of the frequency with which mathematics is used in the different sciences. Simply stated, the hypothesis was that the frequency of initials-only footnoting practices would be closely related to the extent to which the relations among the component parts of a body of knowledge are expressed mathematically.

The actual data used to test this hypothesis were collected for me by a student, Mr. Edgar Engleman, through a simple but laborious process. We selected two journals for each of ten fields of science, ranging from history to physics, took one issue of each for the years 1926, 1936, 1946, 1956, and 1966, and counted things. Mr. Engleman counted the number of research articles in each issue of each journal, the number of articles that employed initials-only footnotes, the number that employed equations, the number that used tables, and so on.

Transforming these numbers into percentages, we were able to produce a graph that gives at least tentative support to my hypothesis. Taking the frequency that tables are used in articles—on the assumption that a table involves at least some mathematics—and charting this over the forty-year period, together with the frequency that initials-only footnotes were used, we found that they did, indeed, go pretty closely together for three *hard* sciences (physics, chemistry, and biochemistry), three *medium-hard* sciences (botany, zoology, and economics), and three *soft* sciences (psychology, sociology, and political science). It turned out that the field of history used only a few initials-only footnotes and *no* tables, so we excluded it from our calculations altogether.

Initials-only footnotes ranged between 97 and 100 percent for the hard sciences, while the frequency of tables ranged between 85 and



—— Articles using initials-only footnotes.
– – – Articles using tables.

A. *Hard sciences:*
Physics (*Physical Review, Proceedings of the Royal Society*).
Chemistry (*Journal of the Chemical Society, Journal of the American Chemical Society*).
Biochemistry (*Journal of Biological Chemistry, Biochemical Journal*).
Total articles (one issue of each journal for each year): 1926–86, 1936–89, 1946–84, 1956–86, 1966–83.

B. *Medium-Hard:*
Botany (*American Journal of Botany, Annals of Botany*).
Zoology (*American Journal of Anatomy, Annals and Magazine of Natural History*).
Economics (*The Economic Journal, American Economic Review*).
Total articles: 1926–43, 1936–54, 1946–38, 1956–47, 1966–54.

C. *Soft sciences:*
Psychology (*British Journal of Psychology, American Journal of Psychology*).
Sociology (*American Journal of Sociology, American Sociological Review*).
Political Science (*American Political Science Review, International Affairs*).
Total articles: 1926–48, 1936–50, 1946–47, 1956–60, 1966–58.

FIG. 1.—Frequency of initials-only footnotes and frequency of use of tables for sciences at three levels of hardness.

96 percent. The medium-hard group showed a fairly steady increase on both measures, with footnotes increasing from 49 to 95 percent and tables increasing from 65 to 91 percent. For the soft sciences, initials-only footnotes went from 31 to 52 percent, while the use of tables increased from 42 to 81 percent. At no place on the graph do the lines representing each of the three groups overlap, and I think this is about as much as anyone could ask from such crude data in support of such a crude hypothesis.

Now, having established, to my own satisfaction at least, that "hardness" can be indexed and that it does bear some relationship to the degree of "impersonality" in a field as I have indexed it, it may be profitable to go on to speculate about the broader implications of the relationship between rigor and social organization in different fields of science.

Further Consequences of the "Hard-Soft" Continuum

Toward the beginning of my remarks, I made reference to certain parallels between the coinages that circulate within the scientific and the economic sectors of society. These are, respectively, professional recognition and money. Among some of the more obvious parallels between them are the following: both are abstract, both are earned through appropriate response to the demands of the "market," and both may be reinvested so as to yield more of the same (7).

Both coins are abstract in the sense that neither is a concrete "good" but can serve to establish a fund of general influence—credit, in the monetary sense, and scientific reputation, in the scientific sense. In other words, neither the amount of money nor of professional recognition that one has is related specifically to what one did to earn it. Further, it is not the absolute amount but the relative amount one has that determines the extent of one's influence upon others' economic or scientific activities. This point will take on more significance in a moment.

Both coins must come from others who want what one has to offer. With no market for buggy whips today, no one is going to make a fortune by manufacturing buggy whips. Similarly, a scientist doing research on a topic that no one else is interested in is unlikely to receive much recognition from his colleagues, since almost by definition they will be unable to see its significance or to evaluate it critically.

Finally, the rich do seem to get richer in both cases. A sizable amount of money allows one to invest it and to earn more money. A substantial scientific reputation will open doors to research funds and to communication with others, even when one's work is in a relatively high-risk area.

Beyond these parallels, I think there is another that will bear investigation and will bring us back once more to the distinction between hard and soft sciences. This is the fact that both the scientific and the economic systems employ *generalized media* in coordinating the activities of their participants. By generalized media, I mean abstract units of value, represented here by money and by recognition; neither is defined strictly by the particular activities through which they were earned, and it is the search for one or the other that keeps people sensitive to the interests of others. And it is this mutual sensitivity, in turn, that keeps these two social systems—the economic and the scientific—reasonably well coordinated most of the time.

There is one difference between the economic and the scientific communities that we must be aware of. In the economic system, one receives money for the goods or services that he offers, and the total amount of money in circulation is supposed to be somehow representative of the total value of such "real goods" within the system. In the scientific system, on the other hand, while one's work is rewarded with professional recognition, this must be determined with respect to the total amount, or the "goodness" of current theory—which in turn represents the "total value" of the data presently available. So, "real goods" are the equivalent of data, and the total amount of money circulating may be seen as the equivalent of currently accepted theory. Since the elements that make up theory can be traced back to their contributors, we might say that the goodness of current theory is generally equivalent to the total amount of professional recognition in circulation, but this is not of basic importance here.

We may still say, I think, that the relationship between activity and reward in each system—that is, between labor and wages, or between contribution and recognition—will be subject to excessive fluctuation to the extent that the total amount of generalized media in circulation is imperfectly related to the amount of "real goods" in the system. During an inflationary period, the amount of money relative to the amount of "real goods" has increased, forcing a rise in prices and in wages; during deflation, money is scarce and wages fall. Within science, similarly, when theory has begun to expand relative to the amount of data available, reputations may rise; contributions will be relatively easy to make, and the amount of recognition they earn may turn out in the long run to have been quite excessive. Given the reverse situation, when theory lags behind the accumulation of data, awards of recognition may be quite stingy, because criteria for assessing significance are lacking. In this sense, it might be said that those who spend most of their energies in collecting data and look with suspicion upon theory have a "deflationary" or bearish attitude, while those who would rather build grandiose theories upon flimsy data are "inflationary" or bullish.

In both cases, it seems to me that the crucial factor is the extent to which there is a rigorous set of rules governing the amount of generalized media in circulation, rules that establish a fairly tight relationship between money and "real values," or between theory and data. When there is a well-defined relationship between paper money and gold, for instance, so that a paper dollar can be printed only when it represents a given amount of gold stored at Fort Knox, the chances of galloping inflation or deflation are minimized. Similarly, when the techniques by which generalizations are tested against data are rigorous, there would seem to be relatively little likelihood that theory—and, thus, scientific reputations—will undergo rapid inflation or deflation.

And so we have returned again to the distinction between hard sciences and soft sciences, this time having developed the hypothesis that the hard sciences will be less characterized by rampant faddism, by reputations quickly made and quickly forgotten, or by protracted internecine warfare over competing interpretations of data, than will the soft sciences. All of these things, we can suppose, are potential consequences of a lack of rigor in relating theory to data, so that susceptibility to them will be more characteristic of the soft sciences.

Obsolescence and Inflation

I do not, unfortunately, have the data at hand to test this hypothesis. I am not even certain of the best form that such data should take. It occurs to me, however, that librarians might have a special interest in one form that they might take. By this, I mean that one index of "inflation" within a field of science might be the amount of material in the literature that has become entirely obsolete and is never referred to anymore at all except by an occasional historian of science. Actually, there are two types of obsolescence that should be noted. The first is the obsolescence of particular articles because the interpretations of data contained in them have been carried forward by others and are kept current, so that even while the original article is no longer referred to, its content continues to be important in present-day work. The second type of obsolescence occurs when an entire subfield of knowledge is no longer referred to at all, even implicitly. It was, to be blunt, wrong, or at least misguided, and has been effectively excluded from the corpus of literature relevant to today's research.

It is this second type of obsolescence that you may be interested in. I would guess that when such bodies of forgotten literature exist, they stem from the "softer" periods of a discipline's history, and it is in the softer sciences of today that such a form of collective obsolescence is likely still to occur. One example from a softer period of nuclear physics concerns "*n*-rays," a phenomenon "discovered" in 1902 by a French physicist, Professor Blondlot of Nancy, six years after the discovery of x-rays. His discovery led to a flurry of scientific activity in the next five years, yielding close to 100 papers in the literature of science and, then, was entirely forgotten after it was shown to have been based on an overly optimistic faith in human sensory mechanisms (8).

To the extent that a field of science is soft, we would expect it to continue producing sizable bodies of literature that become obsolete in this second sense with the passage of time, because the criteria by which they were originally judged were not rigorous enough to justify their rejection. Similarly, we would expect a larger amount of fluctuation in the reputations of scientists in the softer sciences. In a hard science, contributions can be more realistically evaluated in the first place, and the recognition received for them should be a more dependable or stable sort of currency. As a science is softer, however, reputations will be more likely to inflate grandly for a short time and then diminish again, in just the way that paper profits did during the Florida land boom of the early 1920's, or before and after the Crash of 1929.

An interesting example of this sort of inflation within a science was to be seen in the field of botany during the latter part of the nineteenth century. As the naturalists moved west with the explorers and settlers, there was a real boom in the identification of new species of plants, and recognition was to be gained through having one's name attached to the new discovery. There was intense competition to find more and more new species, and irrelevant differences were sometimes seized upon in order to justify the naming of yet another species after its discoverer. This sudden increase in the amount of recognition in circulation without an equivalent increase in the amount of useful or meaningful data, as I interpret the story, became cause for some alarm among the academic botanists whose reputations had been gained at an earlier time when, as it were, the coinage was more valuable. In the end, I believe that a stricter set of rules governing taxonomic procedures and spelling out more clearly the process through which such discoveries were to be registered, was established in order to quell the inflation (9).

A soft science, then, is on shaky grounds in terms not only of understanding and organizing the materials with which it is concerned, but also of its stability as a social group. If the relationships among the members of a discipline are influenced by the nature of its body of knowledge, these relationships will be unstable to roughly the same extent that the relationships among its concepts are imprecise.

It may be useful at this point for me to discuss briefly the relationship between what I have been talking about and the work of Thomas S. Kuhn on scientific revolutions (10), for it might seem that there is a relationship between what Kuhn calls "'revolutionary science" and some of the things I have suggested are characteristic of soft sciences. I am not certain, however, that this is the case. For instance, even when a field like physics is undergoing radical change in the conceptualization of its basic materials—as in the change from Newtonian to Einsteinian paradigms—the rules governing the ways that concepts within the field are related to one another and to the data do not necessarily change. They continue to involve a great deal of mathematics. It might even be suggested that such revolutions will be more clearly visible and perhaps of shorter duration in the hard sciences than in the softer sciences because the criteria by which a crisis is identified and by which its subsequent resolution is legitimated are more rigorous. Beyond this, I do not know that hardness and softness are necessarily related to, as Kuhn terms them, revolutionary and normal science.

Conclusions

In conclusion, let me suggest that if there is anything useful in what I have had to say here, it probably takes two forms. First, I hope that my remarks have given the reader a deeper sense that science is, indeed, a social activity and, as such, is governed by the same sort of forces that govern social behavior generally. Second, I hope that he may have gained a better understanding of the role that rigor plays in science, and perhaps some appreciation of the drive in the softer sciences to become more rigorous through the use of mathematics. This is not simply a desire to emulate the more successful sciences, but rather a desire for more effective grounds on which to organize the collective efforts of many scientists.

As the years pass, we shall certainly see many false starts in this direction within the softer sciences, but I think the long-run trend is obviously toward more hardness throughout science generally—even though this may have unfortunate consequences for some of the more subtle characteristics of particular fields.

I might say, though, that I am content for the moment to be working in a soft science; in a harder science I might not have had the courage to bring these rather fuzzy speculations before you.

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