Why the Social Sciences Won’t Become High-Consensus, Rapid-Discovery Science

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A research front of rapid discovery, leaving a trail of cognitive consensus behind it, is characteristic of natural sciences since about the 17th century in Europe. The basis of this high-consensus, rapid-discovery science is not empiricism, since empirical research existed in the natural sciences before the 17th century. The key is appropriation of genealogies of research technologies, which are pragmatically manipulated and modified to produce new phenomena; high consensus results because there is higher social prestige in moving ahead to new research discoveries than by continuing to dispute the interpretation of older discoveries. The social sciences have not acquired this pattern of rapid discovery with high consensus behind the research front. Their fundamental disability is not lack of empirical research, nor failure to adhere to a scientific epistemology, nor the greater ideological controversy that surrounds social topics. What is fundamentally lacking in the social sciences is a genealogy of research technology, whose manipulation reliably produces new phenomena and a rapidly moving research front. Unless the social sciences invent new research hardware, they will likely never acquire much consensus or rapid discovery. Possibilities may exist for such development stemming from research technologies in microsociology and in artificial intelligence.

KEY WORDS: science; research technology; networks; methodology.

INTRODUCTION

In the postmodernist atmosphere of the late 20th century, it has become unfashionable to ask about the scientific prospects of sociology and

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the other social disciplines. The natural sciences are regarded by many intellectuals as authoritarian and destructive; hence in the social disciplines we should not try to become a science even if we could. On the other side, science was once considered a liberalizing and enlightening movement in Western culture, and some persons continue to work at exploring the social realm in the same spirit. These contentions are moot, if there is no possibility that the social disciplines can be sciences in the same sense as the natural sciences. Let us step back from the polemics and consider the issue using the resources of the sociology of science.

I stress the social organization of intellectual fields in order to avoid the ideological and philosophical terms in which debates over the scientific character of the social "sciences" have often been carried out. Philosophical efforts at demarcation between science and non-science have a long history of difficulties. The logical positivist program of reducing true scientific knowledge to observation statements plus the tautological calculus of mathematics foundered on the inability to make sense of its own metas- tatemts, as well as on paradoxes in the foundations of mathematics. Historians of science and social ethnographers of laboratory life have shown that criteria such as precision, verifiability, and falsifiability are not often used in practice, that they are ideals or after-the-fact reconstructions. Latter-day philosophers from the positivist tradition, such as Quine, hold that no theory is conclusively verified or defeated by any particular empirical observation or logical inconsistency. At best science is justified by a global pragmatism; some scientific theories work better than others, on the whole and in the long run, while others break down and undergo Kuhnian paradigm shifts. Why this is so continues to be debated by philosophers of science, thus far without definite conclusions.

Does this mean that all disciplines are the same, that physics and theology, literary criticism, and sociology are all on the same plane? Postmodernists revel in dissolving boundaries, and the failure of positivist philosophy seems to give grounds for regarding every discipline's knowledge claims as dubious and relativistic. Nevertheless, there is an appearance of sophistry in claiming that all the disciplines are the same. There are social facts to be accounted for: massive differences in their material equipment, sources of funding, social prestige, patterns of recruitment and training. Epistemology is not a good basis for understanding why such differences exist and how they have come about over the past few centuries. A more fruitful approach is to ask, What is distinctive about the social organization of the disciplines that we now take as natural science, and do the social disciplines have (or can they acquire) the conditions that make possible that kind of organization?
THE EMERGENCE OF HIGH CONSENSUS AND RAPID DISCOVERY IN INTELLECTUAL COMMUNITIES

In Europe around 1600 emerged a form of scientific organization characterized by two distinctive traits: high consensus on what counts as secure knowledge and rapid-discovery of a train of new results. These traits did not exist previously, even in the subject matter of natural sciences; it was not science in general that was created in this period, but the organization of high-consensus rapid-discovery science. (To avoid the clumsiness of this term, I will hereafter abbreviate it to rapid-discovery science.) The adaptation of this organization, usually known as the scientific revolution, was in fact not so sudden; from a few areas, notably mechanics and mathematics, it spread during the next 300 years to encompass most of the natural sciences.

High consensus contrasts not only with the pattern of the humanistic disciplines, which have remained outside the orbit of modern science, but with the typical mode of intellectual life throughout world history. In every civilization since the invention of writing, the undifferentiated intellectual role has been that of the philosopher. In this field, in China and India, in ancient Greece, and in the medieval and modern West, the pattern has been for major innovators in philosophy to appear in clumps: that is to say, rival positions are created at the same time, and opposing schools of thought maintain themselves over many generations without one of them establishing ascendancy. The basic pattern of intellectual life is not consensus, but disagreement. This does not mean that philosophy is totally unstructured; it is structured precisely by its rivalries, by its focus upon issues of contention. The rivalry is structured socially by the intergenerational networks that connect eminent individual philosophers with each other: in any period of creative life, there are typically between three and six such lineages or schools, a pattern that I have labeled the Law of Small Numbers. Times when the number of schools exceeds the upper limit of six are periods of structural instability and transition; during these times there is a sense of intellectual crisis, followed by restructuring in the following generations, which reduces the number of lineages to the normal ceiling. The dynamics of intellectual life in the philosophical mode have been driven by the struggle to appropriate a portion of this limited attention space, to

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2Detailed documentation cannot be presented in this paper. The analysis derives from a comparative study of long-term intergenerational networks of philosophers, including their overlap with European scientists and mathematicians in the period between 1500 and 1900. Sources of data are given in my manuscript in progress, The Sociology of Philosophies: A Global Theory of Intellectual Change. Portions are available in Collins (1987, 1989).
be one of the small number of contending parties. Success does not consist in being a sole hegemon, but in finding a place within the action of a multised conflict.

Prior to the scientific revolution and the adoption of the rapid-discovery mode, the fields of natural science also generally had this structure of disagreement. Often natural science was pursued by the general-purpose intellectual, the philosopher; thus there were Aristotelian and Stoic physics, various brands of Taoist and Confucian cosmology, and so forth. Even when there were specialists in a field that would be considered a natural science today, such as medicine or astronomy, there were typically rival schools: for example, in Greece between 300 BC and 200 AD a variety of systems of astronomy were used; there were five in medieval India; throughout ancient and medieval China, there were between three and five rival astronomies (Neugebauer, 1957; Jones, 1991; Dictionary of Scientific Biography, Vol. 15:533-632; Needham, 1959:171-436; Siven, 1969). Greek medicine was divided in a fluctuating alignment among as many as half a dozen schools; Chinese medicine was divided among a variety of Taoist and non-Taoist schools (Frede, 1987:236-260; Welch and Seidel, 1979).

In this perspective, there is no abnormality in the proliferation of schools and the lack of consensus that characterizes 20th-century sociology or anthropology. These fields are much more typical of the form intellectual life has taken throughout history than are the rapid-discovery sciences of the last few centuries. Social scholars tend to regard their disagreements as a pathology of their discipline; but they are merely operating according to the dynamics of the Law of Small Numbers, dividing the attention space among factions who get their topics and their energies from mutual points of contention. Modern philosophy, too, continues to operate as it always did, through the clash of factional disagreement. That a large group of philosophers has taken as their topic the methods and epistemology of the natural sciences has not meant that philosophy itself has become a high-consensus, rapid-discovery science; the focus on science has given these philosophers a particular content but not a new social structure for their own field. One might say that the nonconsensus fields are in the normal condition of the intellectual world; only the rapid-discovery sciences have become deviant.

The modern natural sciences are unusual in their high consensus. Structurally, this means that they have found a mode of organization that evades the Law of Small Numbers, the focus of intellectual life upon rivalries and disagreements. How this has happened will become clearer when we examine the other key feature of modern science — rapid discovery.

In a field with a fast-moving research front, there is a chain of new results following one another with such regularity that innovation becomes
expectable. This phenomenon became socially recognized in several fields by the mid-1600s. The speed of this research front has apparently not been constant. Data from the 20th century based on the obsolescence of the research literature suggests a turnover of attention in some fields of as little as five years or less. But even in physics this extremely rapid turnover may have set in only in the 1920s (Griffith, 1988) and we lack good comparative studies of just how fast the foci of research changed in various fields (e.g., astronomy, biology, chemistry) during the past four centuries. Generally we can say that the rapidness of discovery became noticeably distinctive in some natural sciences in the 1600s, and that the rapid-discovery mode has speeded up since that time and spread to many scientific fields.

In contrast, the normal mode of intellectual life has not been characterized by rapid discovery. The natural sciences before the rapid discovery revolution, both in Europe and elsewhere, tended to maintain the same theories and bodies of recorded facts over long periods of time. And in modern Europe as elsewhere, philosophy and the humanistic disciplines have not had a fast-moving research forefront. This is not to say there are never any changes in topics and conceptions in these fields; also there is the propensity to return repeatedly to the same issues — as in philosophy’s classic epistemological questions, reinterpretations of the same authors by literary critics, or historians’ repeated explanations of the same historical events — whereas the rapid-discovery sciences move on to find new topics about which to make their discoveries. The nonrapid discovery disciplines alone have revivals (bringing back Aristotle or Marx or Nietzsche, experiencing neo-Kantian or neo-Hegelian movements, reviving the reputation of past literary figures). Another facet is the study of the classic text, writing commentaries on Confucius, Hobbes, or Max Weber, whereas the researcher on the forefront of the rapid discovery science does not study Galileo or Newton, but leaves this to the humanistic discipline of historians of science.

The contrasts that Thomas Kuhn and Derek Price initially drew between high- and low-consensus disciplines (paradigm and nonparadigm fields), in the one case, and between fast-moving/high-immediacy and stag-

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3Price (1986). Subsequent work (Cole, 1983; Cozzens, 1989; Leydesdorff and Amsterdamska, 1990) suggests citation analysis is not the best way to measure movement of a research front, since citations are used for a number of different purposes that vary across fields. Differences between the sciences and humanities continue to be found, especially in consensus (Cole et al., 1978; Hargens and Hagtrom, 1982). Rapidity of a research front may best be judged qualitatively, in terms of changing content of ideas and focus on new discoveries.

4We lack comparative studies of the rate of movement in particular periods when sciences were changing — e.g., in ancient Greek or Chinese astronomy — so we do not know just how the non-European sciences at their most innovative compared with the rates of change in Europe since the 1500s.
nant classics-oriented research literatures, in the other, now appear overly simple. There are aspects of dissensus in the rapid-discovery sciences, and of consensus in the humanities; and there are various forms of innovation which take place in the nonsciences intermingled with the several aspects in which their current publication forefront does not have a rapid discovery character. But the basic intuitions were on the right track; the high-consensus, rapid-discovery mode has made a crucial division in the social organization of the intellectual world.

Rapid discovery science does not escape the Law of Small Numbers that structures intellectual life. It too continues to be driven by rivalries and the energizing focus upon disagreements. But these disagreements are confined to the research frontier. As Latour (1987) points out, science has two cognitive aspects, and these correspond to two modes of social organization. At the research forefront, what Latour calls "science-in-the-making," there are disagreements among rival positions that are similar to those found in the humanistic disciplines. Just as in the pattern of rivalries found in philosophy, there is evidence of around five competing research groups in a scientific specialty (Price, 1986:130–133), similar to what would be expected under the Law of Small Numbers.

Here, too, studies of scientists' argumentative discourse shows them using rhetorical tactics to elevate their own position and denigrate those of their opponents, not unlike clashes of humanistic ideologies (Mulkay, 1985). Once a topic is no longer on the research front, it passes into the area of consensus that Latour calls "science-already-made." Old results are taken for granted, elevated to the status of objective facts. One form of dissensus in science that has been widely researched are priority disputes. These do not reflect the rivalry and disagreement at the research frontier, but questions of assigning credit for discoveries that are regarded as having already been achieved. A priority dispute is possible because there is consensus on what counts as an item of knowledge. It is indicative of the difference in organization that disciplines that are not rapid-discovery sciences do not usually have priority disputes.

Rapid discovery science does not so much overthrow the Law of Small Numbers as dynamicize it, by abandoning old controversies in order to get on to new ones. It is the existence of the rapid discovery research front that makes consensus possible on old results. When scientists have confidence they have a reliable method of discovery, they are attracted by the greater payoff in moving to a new problem than in continuing to expound old positions. The research forefront upstages all older controversies in the struggle for attention. Because the field is moving rapidly, prestige goes to the group associated with a lineage of innovations, which carries the implicit promise of being able to produce still further discoveries in the future.
Rapid discovery and consensus are part of the same complex; what makes something regarded as a discovery rather than as a phenomenon subject to multiple interpretations is that it soon passes into the realm of consensus, and that depends upon the social motivation to move onward to fresh phenomena.

**Inadequate Explanations of Rapid Discovery Science**

What is it in the activity of scientists, emerging at a particular time in history, that gave rise to this new mode of cognitive and social organization? Let us briefly review several traditional candidate causes of the scientific revolution; these turn out to be inadequate to explain rapid discovery science.

*Empiricism*

One explanation is to attribute the scientific revolution to a turning away from philosophical speculation and textual tradition, and toward the direct study of nature. This explanation was touted by many scientists themselves at the time, in attacking the scholastic and humanistic traditions that preceded them. But it fails to stand up to historical comparisons. There is a good deal of empirical observation in the sciences prior to and outside of Europe in 1600. The careful collection of astronomical observations had gone on for many centuries in Greece, China, India, and elsewhere, without giving rise to either consensus or rapid discovery. Medicine, biology, and mineralogy had long been studied empirically, and collections of naturalistic observations were incorporated in major works of the philosophical tradition ranging from Aristotle to Albertus Magnus. By itself, empiricism has no dynamism that leads either to a consensus on conceptual schemes and theoretical explanations, or to a rapid-moving research front.

*Measurement and Mathematization*

Was the scientific revolution due to greater precision, or more particularly, to a new emphasis upon measurement, allowing the formulation of mathematical laws? Again the generalization does not hold. Some discoveries did take this form (Galileo's experiments with the inclined plane to formulate the law of acceleration of gravity); but much scientific work at the period of the scientific revolution, and even later, was not very precise (e.g., experiments with the mercury tube at various altitudes, or the
pioneering air pump experiments); and many areas of discovery did not involve measurement at all (such as Galileo's best-known discovery, of the moons of Jupiter by means of the telescope). If the scientific revolution was not necessarily measurement-oriented and mathematical, conversely there was a good deal of mathematical science in the period before rapid discovery science. A telling instance was Greek astronomy. One highly sophisticated version, Ptolemy's planetary system, was the classic exemplar of a nonmoving research front. Moreover, in comparative perspective, mathematics is not intrinsically a rapid discovery discipline or even one necessarily characterized by high consensus. The ancient Greeks had rival forms of mathematics, oriented respectively toward geometric proofs and toward number theory carrying religious significance; in China, the official mathematical textbooks often disagreed with one another, and advanced methods discovered by particular individuals were often lost in subsequent periods (Ho, 1985). The introduction of mathematics into science does not guarantee a shift to the rapid-discovery mode.

Experimental Method

Positivist philosophers have often epitomized scientific method as the design of experimental tests, either in the form of direct manipulation of conditions or comparison of naturally produced variations. But this method is not the common denominator of rapid discovery scientific research, as we see by many examples of nonexperimental and noncomparative findings, ranging from Galileo's telescopic observations to 19th-century physiology. Nor is the inductive logic of Mill's Canons unique to modern science; it has been used in the modern social sciences without making them into rapid-discovery sciences, and it was used on occasion in Greek and Chinese science. Again we find the explanation is neither necessary nor sufficient.

Rapid Discovery Science is Produced by Genealogies of Research Technologies

Modern rapid discovery science hinges on the mode in which scientists became organized around their research technologies. Galileo's discoveries in the early 1600s were often considered the takeoff of the scientific revolution. His crucial innovation was not so much a set of new ideas as the practice of adapting or inventing technologies for purposes of research (Price, 1986:237–253). Galileo tried out various combinations of lenses—available as eyeglasses since the 1200s—to adapt them to astronomical observation; he made use of a pendulum for measuring time in calculations
of motion; he adapted inclined planes not for traditional mechanics but as a way to make observations on the effects of gravity on balls rolling along them. Galileo's techniques may have had earlier precedents, but now they spread to constitute a research forefront. His followers invented barometers and thermometers, and modified the telescope into a different configuration of lenses, yielding the microscope and opening up further areas for investigation. Other researchers, away from Galileo's personal network, took up the example; Guericke and Boyle, for instance, recognized that pumps, which are already available from the technology of mining, could be modified for scientific experiments with pressure, temperature, and the vacuum. In the late 1700s, another wave of research techniques was set off by the invention of the electric battery, giving rise to far-flung research areas including the discovery of new chemical elements by electrolysis of fluids, and by techniques of electromagnetic and subatomic particle research in the following generations. In the same way, the history of modern nuclear physics hinges on a genealogy of accelerators, each evolved from the last; and the progress of astronomical discovery has been the production of new phenomena by the successive developments involving larger optical telescopes, their use in combination with spectroscopy, and the analogous development of radio telescopes. Instead of regarding these technologies as transparent media between scientists' brains and the phenomena that are discovered, we should see the main dynamic in the research technologies.

Genealogies of research technologies can be regarded as the core of the rapid discovery revolution because they produce the fast-moving research forefront as well as the tendencies to consensus and imputed objectivity of results. What Galileo and his followers hit upon was not so much the value of any particular technique, as the practical sense that manipulating the technologies that were already available would result in previously unobserved phenomena. Such a stream of new observables in turn gave rich materials for interpretation in scientific theories. What was discovered was a method of discovery; confidence soon built up that techniques could be modified and recombined endlessly, with new discoveries guaranteed continually along the way. And the research technologies gave a strong sense of the objectivity of the phenomena, since they were physically demonstrable. The practical activity of perfecting each technique consisted in modifying it until it would reliably repeat the phenomena at will. The theory of the phenomenon, and the research technology that produces the phenomenon, became socially objectified simultaneously, when enough practical manipulation had been built into the machinery so that its effects were routinized.

In the early years of the scientific revolution, the practice of making research equipment capable of repeating results took considerable time.
Boyle's generation of air pumps produced consistent results only after about 15 years (Shapin and Shaffer, 1985:274–276). Even in the late 20th century, too quick an announcement of findings leads to controversy and embarrassment when phenomena like cold fusion cannot be routinely evoked. In general, the speeding up of the rapid-discovery front has probably been due to the growing backlog of reliable equipment available to be tinkered with.

Research technologies are not necessarily the brain-children of scientific theorists—ideas embodied in physical apparatus. The Kuhnian paradigm, insofar as it is a cognitive model of the topic under investigation, gives a misleading sense of what scientists on a research forefront are doing. The scientists may have quite irrelevant ideas of how their equipment is operating. What guides them is on a nonintellectual level, a sense of what kinds of physical manipulations have resulted in interesting phenomena in the past, and of what sorts of modifications might be tried that will produce yet further phenomena. This is not to say that scientists' ideas are superfluous; scientists can bring their equipment-generated discoveries into the social community of scientists only by interpreting them in concepts related to the going currency of intellectual discussion. Research scientists lead a double life: as intellectuals in the game of argument for theoretical positions and as possessors of a genealogy of machines.

Just as the intellectual community consists in networks that pass ideas, problems and social validation along from one person to another, research technologies comprise a parallel set of networks. One machine gives rise to another in a genealogical succession: by modifying the past machine, or by cloning it from another in the same laboratory, or by a kind of sexual reproduction recombining parts from several existing pieces of equipment. Human and machine networks develop symbiotically; a machine embodies the results of the human activity that went into making it work in a particular way, while these human skills are typically tacit (H. Collins, 1974) and cannot be conveyed to another person except by hands-on experience at the machine. There is some evidence that research technologies on the active cutting edge can only be made to operate successfully by persons trained on the immediately preceding generation of machines. For instance, Boyle's vacuum pump diffused to other researchers only through a network of persons who had used an earlier exemplar (Shapin and Shaffer, 1985:229–230, 281). Sometimes there are bigger breaks in the genealogy of research equipment, as when one scientist recognizes that something may be made analogous to the kind of equipment used in another line of research, perhaps even the word of mouth that a particular kind of equipment has yielded interesting results.

Two implications of this genealogical structure of equipment are important for the social organization of rapid discovery science. One is that
access to the forefront research technology tends to be socially concentrated; since new discoveries are produced by machines based on those that had made the previous discoveries, the new discoverers are likely to be persons who worked intimately with the previous round of equipment. It is not unusual for a person admitted to the scientific network as a laboratory technician to become a notable discoverer in his or her own right—as Hooke did by working with Boyle and his equipment, or the instrument maker Watt who developed a steam engine after working for the chemist Black. Limited access to forefront research equipment is one of the features that reduces competition among rival scientific theories and helps bring about consensus.

The other result is that research equipment, once routinized, may be exported from the laboratory, where they stand as physical reminders of the scientific theories that are associated with them. The triumphs of applied science are not necessarily the application of a scientific theory to a layperson’s problem. They often consist in a practical activity developed in the laboratory and then reproduced in the field; for instance, the pasteurization of milk to eradicate disease consisted in transforming the dairy into a technological routine resembling that of the medical laboratory (Latour, 1988). We noted above that scientific research equipment may begin by being imported into the laboratory after existing, perhaps for hundreds of years, in the lay world. After modifications at the hands of scientists, the equipment may become commercially viable when reintroduced into the lay world. Thus the research device for producing and detecting electromagnetic waves could be adopted for lay use as the radio. Once this happens, the research process is socially legitimized to a high degree: not merely on the level of ideology (which may wax and wane), but in the taken-for-granted practices of everyday life. Rapid-discovery science generates a strong sense of its objectivity and factuality because its research technology produces many allies.

In these ways, genealogies of research technologies play a key role in every aspect of modern rapid discovery science: making possible rapid movement along a research front, creating a sense that new discoveries are always possible, turning attention away from old controversies in order to get on with still newer discoveries, and promoting consensus and the sense of objectivity of past results.

DO THE SOCIAL DISCIPLINES HAVE RAPID DISCOVERY RESEARCH TECHNOLOGIES?

If this is correct, sociology and the other social disciplines have severe obstacles to being rapid-discovery sciences. The most glaring lack within social science is a self-generating lineage of research technologies. Let us
briefly follow the preceding argument with a parallel consideration of the conditions of social research. In the interests of space, I will concentrate on sociology with occasional glances at the other social disciplines.

The problem of the social disciplines is not their youth. They have not lacked sizable bodies of practitioners and institutional bases of support during the past several centuries at least. Yet they have had neither much consensus — sociology in particular is famous for its battles among opposing schools of thought — nor a pattern of rapid discovery.

As long as we believed that the problem was in the area of theoretical approaches or methodological canons, it was possible to believe sociology might be made scientific. It should be necessary only for enough sociologists to adopt the right approach and to cease using unscientific methods. In fact, the latter part of the prescription would be redundant, since in a rapid discovery science those who clung to methods that did not bring new results would soon be unstaged by the achievements of those who were moving ahead.

Comparative evidence does not support the picture that a field becomes a rapid-discovery science by adopting particular ideas or methodological canons. That is not to say that these theories and methods are worthless; they may yield some good results without bringing about consensus and rapid discovery. It should be noted that if sociology is not a rapid-discovery science, it does not follow that it is not a science of any kind; it may well resemble the natural sciences before the rapid-discovery revolution, something like Chinese astronomy or Greek mathematics.

The usual prescriptions for scientific sociology are much the same as those that are erroneously credited with causing the scientific revolution.

Empiricism is not a panacea for sociology. Although some areas of the field are not empirical (or at any rate not very systematically empirical), increasing our degree of empiricism would have little effect in bringing about consensus or speeding up the rate of discovery. In sociology, there has been a good deal of systematic collection of data since the statistical compilations of Quetelet in the 1830s and the field studies of LePlay in 1850s, accelerating since the establishment of academic sociology departments from the 1890s onward. By itself, empirical research does not lead to consensus or rapid discovery; the results are just as likely to be sets of local descriptions repeated in other times or places, epitomized by the compilations of demographic data that have been made over many decades.

Nor does quantitative precision and mathematization result in rapid-discovery science. These were not responsible for that effect in the natural sciences. In the social sciences, there have been areas of considerable statistical precision and of mathematical treatment without bringing about a social scientific revolution. Economic theory became extensively mathemati-
cized since the marginal utility paradigm was created in the 1870s; on the empirical side, econometrics dates from the 1920s. Nevertheless, it is not clear that economics has either a rapid-discovery forefront or a high degree of consensus, especially in macroeconomics. In many ways economics is a conservative, classics-oriented discipline, much like sociology, with neo-Smithian and other revivals and a continuing focus upon paradigms over 100 years old. It is true that large areas of economic theory have been mathematized to a degree that resembles the natural sciences much more closely than any other social science. But mathematical theory appears as an encapsulated specialty within economics; it generates its own lineage of technical refinements, but these are often regarded as an idealized world bearing little relation to empirical explanation (Whitley, 1984; Rosenberg, 1992). The mathematization of theory in economics has results paralleling the theory/data split in sociology, and contributing not to consensus but dissensus about the overall direction of the discipline.

In sociology, statistical and mathematical approaches have become specialties within the larger discipline, without transforming the whole into a rapid-discovery science. Mathematical modeling has taken its place as another branch of theory alongside the great variety of sociological theories. Although it contains some works of considerable creativity, it does not appear that mathematical sociology has generated any rapid-discovery lineages, nor achieved any consensus as to a dominating approach. Sociological statistics may have come closer to approximating the rapid-discovery mode, but only among its own specialists. Social statisticians have created a progression of methods, building upon past forms, without much tendency to revive older methods or to engage in prolonged rivalries between opposing forms of statistics. But this characterizes not so much the average sociological researcher, the consumer of statistical methods, as the much smaller community of social statisticians who produce these methods. The distribution and historical changes of research methods actually used by sociologists are not known with much precision; but it seems clear that the progress of statistical methodology has not brought about a high degree of uniformity in the way sociologists do their research, and more generally has not generated consensus or a rapid discovery forefront on substantive matters.

If there is something like a rapid discovery science within the community of social statisticians, this may result from a process similar to the genealogy of research technologies described above. A given statistical method may be regarded as a kind of machinery, much in the same way that the algorithmic techniques of pure mathematics have provided sets of procedures that can be manipulated to produce results in a new form. A takeoff of rapid discovery occurred within European mathematics in the period between Cardan and Tartaglia in the 1530s and Descartes in the 1630s. This development hinged
upon the shift from verbal arguments and abbreviations to the invention of standardized symbols and the formulation of rules for manipulating systems of equations. The invention of this mathematical "machinery" set in motion a series of investigations that paralleled the takeoff in physical science based on tinkering with research equipment. Successive generations of symbolism were created and recombined in various ways, with new mathematical results produced at each step. Mathematics became a rapid discovery science in its own realm, not as an adjunct to research in natural science; its chain of techniques and discoveries has proceeded by its own dynamics; conversely, the development of pure mathematics has not centrally determined the process of empirical discovery in natural science.

Much the same process of development may be seen in the history of statistical techniques. A genealogy of statistical methods is created by modifying earlier methods and trying out analogous procedures (as in the invention of ordinal statistics paralleling those based on interval measurement). There is progress within statistics, taking the form of a succession of methodological discoveries. The relationship between statistics and its application in social research, however, remains like that between pure mathematics and the empirical sciences. By itself, the pure statistical (or mathematical) side can far outrun what is happening in empirical research, and the application of the pure technique to the empirical enterprise does not make the latter a rapid-discovery science. In sociology, it may even lead to a growing sense of divergence, between the refinement of the technique and the noncumulative quality of the substantive results.

The experimental method was not responsible for rapid-discovery science in the natural sciences, nor has it had that effect in social science. It is true that the experimental method lends itself to programs of research exploring a clearly delineated topic, and several such programs in sociology have accumulated series of results (e.g., Willer's Elementary Theory, 1987, and the Expectation States program of Wagner and Berger, 1984). Nevertheless, these experimental programs have not generated widespread consensus, but have become several more strands among the many rival positions within sociology. A broader analogue to the logic of experimental design has been used elsewhere in sociology, in the form of systematic comparison. Historical sociologists have often explicitly used the comparative method, with cumulative results on macroissues such as the theory of revolution (Moore, 1966; Paige, 1975; Skocpol, 1979; Goldstone, 1991; see Collins, 1993, for an overview of continuity in this area). Other comparative work has been done by accumulating available studies and synthesizing their findings into a larger system of generalizations. Such projects have gone on in sociology since the beginning of the 20th century. This method was the basis of Durkheim's project in the Année Sociologique and played a
large part in Weber's encyclopedic treatment of economy and society; subsequent instances include Lipset (1960) and Etzioni (1975).

The comparative method is particularly useful for producing explanatory theory, since it focuses our attention upon generalizable conditions and on bringing to light the most central causal processes. These virtues are also shared by experimental research programs in the narrower sense. Why then has this work not transformed sociology into a rapid-discovery science? The paradox, I suggest, is that what can be found in the intellectual content of sociology clashes with the way the field is socially organized. Substantively, sociology has accumulated a good deal of knowledge on the causal and structural conditions explaining the major variations in social behavior and organization; in this sense sociology possesses a moderate amount of scientific knowledge. I have argued elsewhere that within sociology can be found fair approximations of the basic principles determining microinteraction, the major forms of formal organization, the dynamics of conflicts and social movements, some aspects of stratification, and some macrosociological patterns, especially in the realm of politics. In Collins (1975) I held, more optimistically than now, that sociology already had the basis of a science in the coherent patterns demonstrable by synthesis of previous researches. But this was an argument based on the cognitive content of sociology, ignoring its social organization as a discipline.

At the same time, the social organization of the discipline disperses attention onto a wide variety of theories and researches. Some of these have no social motivation for scientific consensus and cumulation, while others, which are consciously oriented toward scientific knowledge, do not generate a strong enough attraction to sustain a rapid-discovery forefront. The method of comparative research produces impressive results, but it does not give off a chain of such researches that cumulate in the way that research in the physical sciences gives off a research front.

The primary social tendency of intellectuals is to split into contending positions to fill the attention space, to follow the Law of Small Numbers. As we have seen, this tendency is overcome only by special circumstances in the rapid-discovery sciences; it is only because there are greater payoffs in plunging forward into new discoveries that these scientists forego the traditional intellectual mode of maximizing attention by making their positions distinctive from each other. Lacking genealogies of research technologies, sociology and the other social sciences are condemned to the endless contention of positions. Along with this goes the ideology that no scientific knowledge exists in these fields. The reality that is being reflected upon here is the social structure of the field, rather than a description of its cognitive contents. It is feasible, on the epistemological level, to apply canons of generalizability, explanatory power, and coherence, and to extract
from the myriad sociological publications some materials that have a higher scientific quality than others. On the substantive level, there is no need to be a relativist. Some of the contents of sociology are scientific. But they do not become the focus of a social consensus, because they are not linked into rapid-discovery chains. Lacking that social support, they are merely one strand among many others.

Without this prize possession of the rapid-discovery sciences, sociology is unable to produce a stream of discoveries, or the forward-looking structure of intellectual competition that goes along with them. Social science thereby also lacks another major support enjoyed by the natural sciences. The practical applications of the natural sciences typically take the form of exporting laboratory equipment or its physical products into the lay world. Since social science does not invent a stream of new equipment, it is cut off from this form of applicability. When the social sciences offer practical application, it is in the form of information or advice: we give policymakers descriptions of a social problem, and sometimes (more common in economics than in most other social sciences) projections of what should happen if a given policy is followed. Beyond this, applied social science consists to a large extent of exhortation — e.g., the study of stratification is ammunition for social reform movements, and the study of revolution has been an encouragement to make one. Applied sociology frequently takes the form of intervening on a particular side in social conflicts, giving an ideological appeal for one interest and thereby alienating the other.

The natural sciences have acquired a deeper basis of legitimacy. Their ability to spin off physical products from their research equipment has made the exchange between scientific intellectuals and the lay community appear more even-sided. In return for support, laypersons receive material benefits, and often these take the form of equipment that can be individually owned and used, giving huge numbers of persons a visible stake in the scientific research enterprise. Television sets and electric lights are personal reminders of the payoffs of natural science, whereas the benefits of social science are typically collective, as well as remote and hypothetical. The concrete practicality of the natural science can be appreciated by laypersons without understanding the intellectual processes that produced it; in contrast, social science applications, even relatively popular ones such as psychotherapy, are embraced largely by persons who are committed to the underlying intellectual doctrines. Without the results that flow from a genealogy of rapid-discovery technologies, the social sciences are left with a weaker base of societal support.  

5Turner and Turner (1990) have called sociology an "impossible science" because the fragmentation of research specialties and theoretical positions, in America during the late
PROSPECTS FOR INVENTING RAPID-DISCOVERY RESEARCH TECHNOLOGIES IN THE SOCIAL SCIENCES

Are there any signs that rapid-discovery research techniques for social science are on the horizon? Let us review how this has occurred in natural science. At the outset is a technique like Galileo’s lenses. Assembled in one way they produce a telescope, in another way, a microscope; combined with mirrors and prisms, they give rise to the analysis of light spectra. This, in turn, combined with techniques of the chemical laboratory for heating and separating materials, results in spectral analysis of the elements. Recombined with the telescope, the result is the study of stellar spectra, and so on, apparently endlessly. The key characteristics are a machinery that produces some new observable phenomenon (thus giving material for theorizing), plus the capacity that by physically tinkering with the machinery, modifying or recombining its parts or applying it to new uses, it produces still further phenomena for study. It is a tinkerable research technology that gives rise to a stream of discoveries.

Do the social sciences have anything like this? We are looking for something quite specific, not merely general resemblance to the forms of presentation found in scientific papers. The use of statistics in social research, as I have argued above, is not a tinkerable research technology in this sense. It is part of the theoretical manipulation of the data, not a method of producing new data. In general, sociology’s repertoire of techniques for data gathering has been quite slow moving. The basic empirical methods—field observation, questionnaires, experiments, and historical analysis—have not changed much over the past century. What has changed, in some cases, has been the kind of data that has been collected by such methods (e.g., questions about network ties) and the methods by which data are analyzed (e.g., the changing fashions for statistical analysis of questionnaire data). Occasionally we have turned up new phenomena (e.g., the microdetail pioneered by Goffman and others), but there has been nothing like a stream of rapid discoveries driven by innovations in research methods. Our research techniques are not tinkerable and recombinable in the way that those of the natural sciences are. Ultimately we rest upon native human skills of making observations, and our discoveries are laboriously driven by theoretical acumen in where to look for data and in how to package it conceptually. What we lack is the key possession of the rapid-

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twentieth century, is reinforced by decentralization of organizational resource bases. But this is an inalterable situation only to the extent that sociology does not develop streams of rapid discovery technologies.
discovery sciences, their nonhuman machinery for generating previously unobservable phenomena. They have acquired a mechanical device that produces a rich stream of novel data as rapidly as the machinery can be manipulated.

The outstanding example of new hardware in social research was the introduction of the computer. But this has been used since the 1950s, without major effects on the rapidity of the research front. The computer does not produce data, but assists in analyzing it. Developments of computer hardware are extrinsic to social research, and for the most part have little bearing on the phenomena we study or our theoretical consensus in understanding them. On the other hand, social scientists have made some contributions in creating software — for instance, algorithms for social networks or simulation models for sociological theories. But such activity is essentially theory driven; not unexpectedly, it has thus far not given rise to rapid discovery.

There are some possibilities that social research hardware may move in this direction. In microsociology during the past 30 years, the introduction of audio and video recording has opened up the field of conversation analysis; new phenomena have become visible, such as the fine-grained coordination of persons in social interaction (Sacks et al., 1974; Grimshaw, 1990; Clayman, 1993). Is it possible that such research technologies can turn into a self-sustaining genealogy of new technologies producing a stream of discoveries? There are some signs that a stream may be emerging. For example, by combining voice recording with a Fast Fourier Transform analyzer, a device that distinguishes the component frequencies of a sound, new social phenomena have been produced (Gregory, 1983; Gregory et al., 1993). This equipment has brought the discovery of a low-frequency acoustic region where conversationalists' voices converge during an interaction, thus giving a nonverbal measurement of social solidarity. An offshoot of this technique gives an instrumentation for who is deferring to whom in an interaction. So far the pace of innovation from one technology of microconversational research to another has not been particularly rapid, but perhaps the field is in the early phases of an acceleration that will become more noticeable in the future. We lack comparison studies on the pace of technological innovation in research equipment in the early phases of the natural science revolution; for instance, how long did it take before Galileo's techniques, or those of the early air-pump experiments, were extended to new discovery-making techniques? It is possible that in sociology such technologies as those involved in microconversational research will encourage analogous developments outside the realm of conversation and microinteraction that will lead to a wider stream of discoveries.
Another possibility exists in the field of artificial intelligence (AI). The software of AI is an application of theory, and hence would not be expected to be the source of rapid discovery. What raises a stronger possibility is that a series of hardware innovations may emerge that is directly connected to sociological research. Elsewhere (Collins, 1992) I have suggested that an AI that has human-like capacities can be built if it has the basic competencies of social interaction. The fundamental argument combines theoretical points from Mead, Durkheim, and conversation analysis. Human thinking is interiorized conversation; and verbal conversation operates on two levels: a cognitive level of symbols which are acquired in interactions and represent group membership; and a behavioral level of emotion-laden rituals, which establish membership. A sociological AI can be built by modeling these processes. An early prototype could be an “infant AI,” which gradually builds up symbols through its history of social interactions. An important requirement is the capacity for emotional attunement with other persons. Along with software design, this requires building an AI robot with voice sensors (or sensors for other emotional signals) and also equipment that can communicate emotional gestures to others. Such a piece of sociological research hardware would be subject to tinkering, and thus to the production of new phenomena. These would not be limited to the nonverbal properties of microinteraction; for instance, it could become possible to simulate and experiment with various interactional situations and forms of social organization. In this scenario, a sociology-built robot could give rise to a stream of research techniques and to a rapid discovery forefront.

It is beyond the scope of this paper to survey the research technologies of all the social sciences. In general, sociology, political science, and anthropology are methodologically in the same boat. Economics has been discussed above in terms that suggest it too lacks the research technology of rapid-discovery science. An exception is the research technologies of archeology and physical anthropology, which draw on those of the natural sciences in dating artefacts or establishing genetic links. There are indications that the use of these techniques is splitting anthropology into non-communicating factions (Morell, 1993). Whether they are giving rise to rapid discovery science in the hardware-using branches is not clear.

A brief comment on psychology is in order. Psychology became a laboratory science in the 1870s. But it has remained a typical social science in key respects: long-standing dissensus among many competing theoretical programs (both in the period of introspectionism and during the heyday of behaviorism) and the lack of a rapid discovery front. In recent decades, part of psychology has closely allied with methods of biological research, especially genetics and neurophysiology. The result has been a rift with the
cognitive, behavioral, and social-motivational sides of psychology. It remains for the sociology of science to investigate whether any of these developments shows signs of bringing a shift to the high-consensus, rapid-discovery mode. During the time when the battle goes on among biologically oriented, computational, and more traditional methods, the result is lower rather than higher consensus. Whether one of these areas is becoming a rapid-discovery science, and what effect that will have on other parts of the field, needs investigation. One possibility is that the rapid-discovery sector will drive the others out of existence through the competition for attention and funding. Another is that the discipline may permanently split into separate enterprises.

Scenarios of Future Social Science

None of the above scenarios for sociology is certain, or perhaps even likely. The possibility cannot be ruled out that sociology will acquire a self-sustaining stream of research technologies in the future and thereby move into the rapid-discovery mode. If so, this will happen in the midst of many other kinds of sociology that continue to be pursued by traditional methods. What then will happen to all these other data-gathering techniques—historical comparisons, field observations, interviews—and to other modes of sociological discourse? Many of these would be unaffected by a research technology, say, for measuring nonverbal interaction. Nevertheless, we should bear in mind that the rapid-discovery sciences have developed not as individual technologies but as streams of research technologies. Earlier techniques give rise to new ones, sometimes by application to remote topics, sometimes by the creation of new research technologies by analogy or recombination from those existing in another area. In this case, one can imagine that sociology, with a first few pieces of research hardware, would resemble natural science in the early 1600s. There would be hundreds of years ahead of us during which research technologies would penetrate and transform the older branches of social research; and since a genealogy of research technologies produces new phenomena to be studied, we could expect that many new branches of social science would be created.

It may be that some areas of social science simply cannot be technologized, and hence cannot be turned into the rapid-discovery mode. It is hard to envision, for instance, how historical sociology would be turned into a hardware-driven research field. And doubtless huge areas of metatheory and ideological controversy will remain. Social scientists, dealing intimately with human interests and conflicts, are intrinsically more active in promoting ideological disputes than are natural scientists, and this
difference will remain no matter what happens to the conditions determin-
ing the rapidity of the research front. At most, one can expect that social
sciences could become hybrids: part in the rapid-discovery, high-consensus
mode, other parts much as they are today.

Perhaps the more likely scenario is that the social sciences, except
perhaps in isolated pockets, will not develop streams of research technolo-
gies, and will never acquire the rapid-discovery mode. On the balance sheet
of pluses and minuses, this may not be altogether a bad thing. What we
forego is social prestige and material support, collective pride in what our
field has accomplished, and the excitement of intellectual adventure that
goes along with rapid discovery. What we preserve is the independence
that goes with having a great variety of methods; for some scholars, the
pleasures of pursuing humanistic scholarship, and the freedom to teach and
theorize over the whole corpus of our disciplinary history rather than ob-
serving a consensus that tracks past movement of the research forefront.
There are social advantages of living in an intellectual community with low
consensus and slow movement. Oddly, despite the self-images of many so-
ciologists today, this makes our field a conservative and traditionalistic one
—in terms of its own history — whatever political position sociologists may
hold in regard to the larger society. Precisely what we do not like about
the rapid-discovery sciences is their incessant progressivism, their trampling
on all other values in their march to new discoveries.

Ultimately, these value questions are moot. Whether the social sci-
ces become rapid-discovery sciences or not depends upon whether they
acquire streams of research technologies. And that contingency does not
depend primarily upon whether we believe it ought to happen.

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