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THREE FRAGMENTS FROM A SOCIOLOGIST'S NOTEBOOKS: Establishing the Phenomenon, Specified Ignorance, and Strategic Research Materials

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Abstract

This occasionally biographical paper deals with three cognitive and social patterns in the practice of science (not '*the* scientific method'). The first, "establishing the phenomenon," involves the doctrine (universally accepted in the abstract) that phenomena should of course be shown to exist or to occur before one explains why they exist or how they come to be; sources of departure in practice from this seemingly self-evident principle are examined. One parochial case of such a departure is considered in detail. The second pattern is the particular form of ignorance described as "specified ignorance": the express recognition of what is not yet known but needs to be known in order to lay the foundation for still more knowledge. The substantial role of this practice in the sciences is identified and the case of successive specification of ignorance in the evolving sociological theory of deviant behavior by four thought-collectives is sketched out. Reference is made to the virtual institutionalization of specified ignorance in some sciences and the question is raised whether scientific disciplines differ in the extent of routinely specifying ignorance and how this affects the growth of knowledge. The two patterns of scientific practice are linked to a third: the use of "strategic research materials (SRMs)" i.e. strategic research sites, objects, or events that exhibit the phenomena to be explained or interpreted to such advantage and in such accessible form that they enable the fruitful investigation of previously stub-

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born problems and the discovery of new problems for further inquiry. The development of biology is taken as a self-exemplifying case since it provides innumerable SRMs for the sociological study of the selection and consequences of SRMs in science. The differing role of SRMs in the natural sciences and in the *Geisteswissenschaften* is identified and several cases of strategic research sites and events in sociology, explored.

INTRODUCTION

In his youthful journal, the exacting and agonistic literary scholar, C. S. Lewis (1975:76), makes benign reference to "the inexhaustible loquacity of educated age." Plainly alert to that capability, the Editors of *Annual Review of Sociology* wisely limit the space allotted prefatory chapters. In my own case, it was understood further that, unlike the prototypes that have long appeared in *Annual Reviews* of many other disciplines, this chapter would be neither a capsule intellectual autobiography nor an overview of the field. Instead, I tell only sporadically of biographical moments; for the rest, the asked-for personal aspect comes from my drawing upon fragments from notebooks assembled over the years and upon pieces published in obscure or improbable places. It was soon obvious that space would allow only for limited reflections on just 3 of the menu of 45 subjects I had itemized for the Editors—see Appendix, "The Menu"—the three being cognitive and social patterns in the practice of science that have long interested me. The patterns—"establishing the phenomenon," "specified ignorance" and "strategic research material"—have to do, not with scientific methods, let alone with "*the* scientific method," but with scientific practices (although there is, of course, much method in those practices).

ESTABLISHING THE PHENOMENON

In the abstract, it need hardly be said that before one proceeds to explain or to interpret a phenomenon, it is advisable to establish that the phenomenon actually exists, that it is enough of a regularity to require and to allow explanation. Yet, sometimes in science as often in everyday life, explanations are provided of matters that are not and never were. We need not reach back only to ancient days for such episodes as the younger Seneca explaining why some waters are so dense that no object, however heavy, will sink in them or explaining why lightning freezes wine. Our own century provides ample instances. There is René Blondlot's report of having discovered a "new species of invisible radiation," dubbed N rays. These were later "observed" by a dozen or so other investigators in France, but there were no comparable replications in England, Germany, or the United States. Intensive further inquiry, by French scientists as well as others, established the fact that the phenomenon was not one of N rays but rather of wishful perception and self-fulfilling prophecy. After that, N rays were no longer observed (Rostand 1960:12–29, Price 1961:85–90). Or again, there is Boris Deryagin's "discovery" of polywater in the 1960s, later found to be wholly artifactual (Franks 1981). Such episodes return us to Claude Bernard's observation that "if the facts used as a basis for reasoning are ill-established or erroneous, everything will crumble or be falsified; and it is thus that errors in scientific theories most often originate in errors of fact" (Bernard [1865] 1949:13).

No small part of sociological inquiry is given over to the establishing of social facts before proceeding to explain how they come to be. Often enough, the empirical data run contrary to widespread beliefs. Thus, it would seem premature to ask why "urbanization is accompanied by destruction of the social and moral order" inasmuch as evidence accumulates (Fischer 1977) to suggest that the connection is rather more an assumption than a repeatedly demonstrated fact.

In sociology as in other disciplines [see Leontief (1971) on economics], efforts to establish recurring social patterns are often described—sometimes of course with justice—as simply "fact-finding" or "fact-mongering" by those preferring swift explanation. Yet years ago, at the turn of the century, the exemplary scientist-philosopher C. S. Peirce was reminding us of the analytic function of fact-finding in what he described as the salient process of abduction (in turn related to processes of deduction and induction):

> Accepting the conclusion that an explanation is needed when facts contrary to what we should expect emerge, it follows that the explanation must be such a proposition as would lead to prediction of the observed facts, either as necessary consequences or at least as very probable under the circumstances. A hypothesis, then, has to be adopted, which is likely in itself, and renders the facts likely. This step of adopting a hypothesis *as being suggested by the facts,* is what I call *abduction* (Peirce 1958:VII 121–22; emphasis supplied).

The ex post facto phase of an empirical inquiry—or as some prefer; the post factum or post festem phase—has us introduce a hypothesis adopted, of course, only "on probation," while the ex ante phase draws out necessary and probable experiential consequences which can be put to falsifying or confirming test. Practiced investigators take it as a matter of course that, along with the free play of imagination drawing upon explicit and tacit knowledge, factual evidence often brings fruitful ideas to mind. To recognize this is not to engage in enumerative induction, pure and excessively simple. (I tried to elucidate these notions in "The bearing of empirical research upon the development of social theory," Merton 1948).

As I have noted, the basic role of empirical research designed to "establish the phenomenon" is at times downgraded as "mere empiricism." Yet we know that "pseudo-facts have a way of inducing pseudo-problems, which cannot be solved because matters are not as they purport to be" (Merton 1959:xv). Social scientists of diverse theoretical and value orientations have found it useful to address this matter of pseudo-facts; as examples, see Zeitlin (1974:1074–75) on the separation of ownership and control in large corporations, Gutman (1976:462–63) on the black family, and Sowell (1981:59) on ethnic education. To repeat myself: "only when tedious recitations of unrelated fact [and fact-claims] are substituted for fact-related ideas does inquiry decline into 'mere fact-finding.' " Otherwise, of course, it is a crucial element in scientific inquiry.

As Neil Smelser reminded me upon reading this piece, establishing the phenomenon has its political dimension as well. In the cognitive domain as in others, there is competition among groups or collectivities "to capture what Heidegger ([1927]1962) called the 'public interpretation of reality.' With varying degrees of intent, groups in conflict want to make their interpretation the prevailing one of how things were and are and will be" (Merton 1973:110–11). In significant degree, that "interpretation of reality" involves establishing the phenomena that are an integral part of it.

The governing question in establishing the phenomenon—"Is it really so?"—holds as much for historical particularities as for sociological generalizations. Strongly held theoretical expectations or ideologically induced expectations can lead to perceptions of historical and social "facts" even when these are readily refutable by strong evidence close at hand. This is not so much wishful thinking as expectational thinking.

In the mode of collective biography called for by *Annual Review,* I turn for a parochial instance to the Department of Sociology at Columbia. In his widely read *The Coming Crisis of Western Sociology,* Alvin Gouldner (1970), himself a much-esteemed onetime student at Columbia (Merton 1982) observed that "C. Wright Mills never became a full professor" there. Having presented this as historical fact, he went on to draw its sociological and moral implications: Mills's " 'failure' may remind us that the serious players [in sociological criticism] are always those who have an ability to pay costs" (Gouldner 1970:15). Despite what I have reason to know was Alvin Gouldner's commitment to scholarship, it appears that an overriding sense of the fitness of things and the expectation linked with it helped to create this pseudo-fact although evidence to the contrary was a matter of public record set down in easily accessible documents (such as the University bulletins with their rosters of faculty members). The evidentiary fact that Wright Mills, in a later academic cohort, had become a full professor at a younger age than the quintessential Establishment figure in sociology, Talcott Parsons, would scarcely have served to illustrate the premise or to reach the conclusion. In effect, Gouldner had tried to transform a historical event that never was into a social phenomenon that sometimes is.

The process of hagiographic creation of pseudo-facts did not stop there. Once set down in scholarly print as facts, pseudo-facts have a way of diffusing and becoming amplified (in the fashion long since established experimentally in the study of rumor). The same politically turbulent year of 1970 which saw the publication of Gouldner's book saw the translation into German of the book by Hans Gerth and C. Wright Mills, *Character and Social Structure* (for which I had happily written the foreword when it was first published in 1953). The German publishers went on to specify the pseudo-fact by declaring that Mills had "lost his professorship during the McCarthy period" ("verlor wahrend der McCarthy-Zeit seinen Lehrstuhl"). Not long afterward, an article in the Yugoslavian journal *Praxis,* also ignoring the biographical entry in the widely available *International Encyclopedia of the Social Sciences* which begins "C. Wright Mills (1916–62) was at his death professor of sociology at Columbia University" (Wallerstein 1968: Vol. 10, p. 362), explained the now elaborated nonfact to the contrary in these decisive terms: "C. Wright Mills was dismissed from Columbia University in USA because of his Marxist orientation" (Golubović 1973:363, noted by Oromaner 1974:7).

It is symbolically apt that a shared interest in the then nascent field of the sociology of knowledge had led Alvin Gouldner to adopt me as mentor when he arrived at Columbia in 1943—the story is told in Merton (1982)—just as a few years before, a similar interest had led Wright Mills, then still a graduate student at the University of Wisconsin, to have me vet his manuscripts in that field. So it was that, 30 years after our first meeting, when Alvin and I were reviewing this episode of the unwittingly fabricated 'fact,' he soon subordinated scholarly chagrin to shared intellectual pleasure in the episode as he agreed that it provided a sociological and methodological parable: Take care to establish a phenomenon (or a historical event) before proceeding to interpret or explain it.[1] As for the Wright Mills I knew, since the time in 1939 when he first sent me those manuscripts—he would probably have hooted at the ideological pieties that invited first the invention and then the successive explanations of these nonevents. Or perhaps Wright's ironic self might have argued for the symbolic if not the historical truth of that evolving myth of his never having become a full professor at Columbia, with all its seeming

[1]After our talk, Alvin Gouldner took quick action to erase that pseudo-fact, noting that "shortly after the publication of the *Crisis* I discovered (from Robert Merton) that my assertion that C. Wright Mills had never been made a Professor at Columbia was in error. Having discovered this, I immediately had this statement removed from the Avon paperback edition of the *Crisis,* which was then in production" (Gouldner 1973:130–131). The import of the episode apparently stayed with Alvin, for years later, and in quite a different context, he took care to observe: "Whether anything *might be* or even *should* have been is one thing; whether it was in fact, is quite another" (Gouldner 1980:281).

implications, and of his ideologically motivated dismissal from that professorship he had never held.[2]

The manifest advisability of establishing the phenomenon before undertaking to explain it has long been recognized in principle if not always observed in practice. They understood this abundantly well, for example, in seventeenth-century England (as I found during my years-long stay in that time and place). Consider only this reminder as set forth by the jurist and orientalist, John Selden, in his widely-read *Table Talk* ([1689]1890:139): "The Reason of a Thing is not to be enquired after, till you are sure the Thing itself be so. We commonly are at *What's the Reason of it?* before we are sure of the Thing." So, too, Bernard Fontenelle, the polymath destined to become a centenarian and the almost but not quite literally *secrétaire perpétuel* of the French Academy of Sciences—he served for only 42 years—was observing in his *Histoire des oracles* ([1686]1908:33): "I am convinced that our ignorance consists not so much in failing to explain what is as in explaining what is not. In other words, we not only lack principles that lead to the true but hold others that readily lead to the false." What Fontenelle did not take occasion to observe, however, is that a certain kind of ignorance advances scientific knowledge.

SPECIFIED IGNORANCE

It was Francis Bacon who made "the advancement of learning" a watchword in the culture of science emerging in the seventeenth century. From then till now, efforts to understand how science develops have largely centered on the modes of replacing ignorance by knowledge, with little attention to the formation of a useful kind of ignorance, as distinct from the manifestly

[2]That some (unknown number of) academic careers have been curbed or halted by political or ideological commitments is, of course, a matter of historical record (Lazarsfeld & Thielens 1958, Schrecker 1986). But it was Bernhard J. Stern, not Mills, who, an announced Marxist and cofounder of the Marxist journal *Science and Society*, never advanced beyond a lectureship in the Columbia Department of Sociology, despite departmental recommendations for promotion. Just as again, it was Bernhard J. Stern, not C. Wright Mills, who was attacked by Joe McCarthy as an alleged Communist only to have the University respond by continuing Stern in his marginal post as Lecturer. During the McCarthy period, actual events often transcended social categories such as Establishment and self-declared anti-Establishment figures. Thus, when McCarthy's associate, the then vice-presidential candidate, Richard Nixon, was charged by some Columbia professors with having "violated an elementary rule of public morality" by the way he had accumulated campaign funds, he responded by ransacking the files of the House Committee on UnAmerican Activities and then catapulting nine of those professors onto banner headlines in the New York Daily News and the Chicago Tribune as alleged subversives; the infamous nine included the literary critic Mark van Doren, the philosopher Irwin Edman, the historian Henry Steele Commager, and the sociologists, Robert M. MacIver, Paul F. Lazarsfeld, and Robert K. Merton—but not, as he himself ironically noted, C. Wright Mills.

dysfunctional kind. Karl Popper provides the monumental contemporary exception that illuminates the rule, most powerfully in his analytical essay "On the sources of knowledge and of ignorance" ([1960]1962). The general inattention to the formation of useful ignorance has long obtained as well in the sociology of scientific knowledge [but now see Smithson (1985) along with the early collateral paper on the functions of ignorance in social life by Moore & Tumin (1949)].

These retrospective notes focus on the dynamic cognitive role played by the particular form of ignorance I describe as "specified ignorance": "the express recognition of what is not yet known but needs to be known in order to lay the foundation for still more knowledge" (Merton 1971:191). "As the history of thought, both great and small, attests, *specified* ignorance is often a first step toward supplanting that ignorance with knowledge" (Merton 1957:417).

The concept of *specified* ignorance hints at various other kinds and shades of acknowledged ignorance in science. The familiar kind of a general, rote, and vague admission of ultimate ignorance serves little direct cognitive purpose though it may have symbolic significance in reminding us of our limitations. This kind, however, does not issue in definite questions. And vague questions evoke dusty answers. After all, it takes no great courage, or skill, in the domain of science to acknowledge a general want of knowledge. It is not merely that Socrates set an ancient pattern of announcing one's ignorance. Beyond that, the values of modern science have long put a premium on the public admission of one's limitations or the expression of humility in the face of the vast unknown. Scientists of epic stature have variously insisted on how little they have come to know and to understand in the course of their lives. We remember Galileo teaching himself and his pupils to reiterate: "I do not know." And then, inevitably, one recalls the "memorable sentiment" reportedly uttered by Newton "a short time before his death":

> I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me (Brewster 1855: II, 407).

Or again, Laplace—the French Newton—is said to have put much the same sentiment in a typically Gallic epigram: "What we know is not much; what we do not know is immense" (Bell 1937:172). What the mathematician Bell (1931:204) describes elsewhere as "a common and engaging trait of the truly eminent scientist [found] in his frequent confession of how little he knows" can be identified sociologically as the living up to a normative expectation of ultimate humility in a community of sometimes egocentric scientists. It is not simply that a goodly number of scientists happen to express these self-belittling sentiments; they are applauded for doing so.

But of course these paradigmatic figures in science do not confine themselves to such generic confessions of ignorance as may reinforce the norm of a decent humility without directly shaping the growth of scientific knowledge. They repeatedly adopt the cognitively consequential practice of specifying this or that piece of ignorance derived from having acquired the added degree of knowledge that made it possible to identify definite portions of the still unknown. In workaday science, it is not enough to confess one's ignorance; the point is to specify it. That, of course, amounts to instituting, or finding, a new, worthy, and soluble scientific problem.

Thus, as I have had occasion to propose, the process of successive specification of our ignorance in light of newfound knowledge provides a recurrent sociocognitive pattern:

> As particular theoretical orientations come to be at the focus of a sufficient number of workers in the field to constitute a thought collective, interactively engaged in developing a distinctive thought style (Fleck [1935] 1979), they give rise to a variety of key questions requiring investigation. As the theoretical orientation is put to increasing use, further implications become identifiable. In anything but a paradoxical sense, newly acquired knowledge produces newly acquired ignorance. For the growth of knowledge and understanding within a field of inquiry brings with it the growth of *specifiable and specified ignorance:* a new awareness of what is not yet known or understood and a rationale for its being worth the knowing. To the extent that current theoretical frameworks prove unequal to the task of dealing with some of the newly emerging key questions, there develops a composite social-and-cognitive pressure within the discipline for new or revised frameworks. But typically, the new does not wholly crowd out the old, as [long as] earlier theoretical perspectives remain capable of dealing with problems distinctive to them (Merton 1981:v–vi).

It requires a newly informed theoretical eye to detect long obscured pockets of ignorance as a prelude to newly focussed inquiry. Each theoretical orientation or paradigm has its own problematics, its own sets of specified questions. As these questions about selected aspects of complex phenomena are provisionally answered, the new knowledge leads some scientists both within and without the given thought collective to become aware of other, newly identified aspects of the phenomena. There then develops a succession of specified ignorance.

As a case in point, consider the sociological theory of deviant behavior as it was developed in four thought collectives. (I draw upon the summary in Merton 1976.) Initiated in the 1920s, E. H. Sutherland's ([1925–1951] 1956) theory of differential association centered on the problem of the *social transmission* of deviant behavior. Its key question therefore inquired into the modes of socialization through which patterns of deviant behavior are learned from others. But as the brilliant philosopher of literature, Kenneth Burke, has reminded us: "A way of seeing is also a way of not seeing—a focus upon object A involves a neglect of object B" (Burke 1935:70). In this case, Sutherland's focus on the acquisition of these deviant patterns left largely untouched specifiable ignorance about the ways in which the patterns emerged in the first place.

Upon identifying that pocket of theoretical neglect, Merton (1938a) proposed the theory of anomie-and-opportunity-structures, that rates of various types of deviant behavior tend to be high among people so located in the social structure as to have little access to socially legitimate pathways for achieving culturally induced personal goals. The Sutherland and Merton theories were consolidated and extended by Cohen (1955) who proposed that delinquency subcultures arise as adaptations to this disjunction between culturally induced goals and the legitimate opportunity-structure and by Cloward & Ohlin (1960) who proposed that the social structure also provides differential access to *illegitimate* opportunities. Since that composite of theories centered on socially structured *sources* of deviant behavior, it had next to nothing to say about how these patterns of misbehavior are transmitted or about how these initial departures from the social rules sometimes crystallize into deviant careers, yet another sphere of specifiable ignorance.

That part of the evolving problematics was taken up in labeling (or societal reaction) theory as initiated by Lemert (1951) and Becker ([1963] 1973) and advanced by Erikson (1964), Cicourel (1968), and Kitsuse (1964). It centered on the processes through which some people are assigned a social identity by being labeled as "delinquents," "criminals," "psychotics," and the like and how, by responding to such stigmatization, they enter upon careers as deviants. In Becker's words: "Treating a person as though he were generally rather than specifically deviant produces a self-fulfilling prophecy. It sets in motion several mechanisms which conspire to shape the person in the image people have of him" (Becker [1963] 1973:34). With this problem as its focus, labeling theory has little to say about the sources of primary deviance or the making of societal rules defining deviance. As Lemert (1973:462) specified this ignorance: "When attention is turned to the rise and fall of moral ideas and the transformation of definitions of deviance, labeling theory and ethnomethodology do little to enlighten the process."

It is precisely this problem that the conflict theory of deviance took as central. Its main thrust, as variously set forth by Turk (1969) and Quinney (1970), for example, holds that a more or less homogeneous power elite incorporates its interests in the making and imposing of legal rules. It thus addresses questions neglected by the earlier theories: How do legal rules get formulated, how does this process affect their substance, and how are they differentially administered?

The case of deviance theory indicates how a dimly felt sense of sociological ignorance was successively specified for one class of social phenomena. But it is not yet known whether scientific disciplines differ in the practice of specifying ignorance—in the extent to which their practitioners state what it is about an established phenomenon that is not yet known and *why it matters* for generic knowledge that it become known.[3] Such specified ignorance is at a far remove from that familiar rote sentence which concludes not a few scientific papers to the effect that "more research is needed." Serendipity aside, questions not asked are questions seldom answered. The specification of ignorance amounts to problem-finding as a prelude to problem-solving.

It is being proposed that the socially defined role of the scientist calls for both the augmenting of knowledge and the specifying of ignorance. Just as yesterday's uncommon knowledge becomes today's common knowledge, so yesterday's unrecognized ignorance becomes today's specified ignorance (Merton 1957:417, Popper [1960] 1962, Sztompka 1986:97–98). As new contributions to knowledge bring about a new awareness of something else not yet known, the sum of manifest human ignorance increases along with the sum of manifest human knowledge.

STRATEGIC RESEARCH MATERIALS (SRMs)

Establishing the phenomenon and specifying ignorance link up with a third pattern of scientific practice that has long been of interest to me. This is the ongoing search, variously evident in the various sciences, for "strategic research material" (a cumbrous nine-syllable phrase better shortened to SRM). By SRM is meant the empirical material that exhibits the phenomena to be explained or interpreted to such advantage and in such accessible form that it enables the fruitful investigation of previously stubborn problems and

[3]Mathematics, of course, has a long tradition of publishing fundamental problems (long ago, in the form of challenges). Upon reading this portion of the chapter, my colleagues, Joshua Lederberg and Eugene Garfield, informed me of their episodic interest in institutionalizing what amounts to the specification of ignorance. For one expression of that interest in print, see Garfield's (1974) "The Unanswered Questions of Science." Lederberg has made me the beneficiary of his 1974 permuterm bibliography entitled "Unsolved Problems" in the various sciences and has referred me to a specimen volume entitled *100 Problems in Environmental Health* (McKee et al 1961). My attention was also redirected to that superb and lively anthology I had misplaced, *The Scientist Speculates: An Anthology of Partly Baked Ideas* (Good et al 1962), which is designed "to raise more questions than it answers." Of particular interest is the piece in the anthology happily entitled "Ignoratica" by one Félix Serratosa who ascribes the essential idea of a "science of unknowns" to the explosive imagination of that prolific and often paradoxical Florentine critic, novelist, poet, and journalist Giovanni Papini. However that may all be, it can be said in self-exemplifying style: That the specification of ignorance is indispensable to the advancement of knowledge, I do not doubt; whether disciplines do differ notably in the practice of such specification, I do not know. Since the phenomenon is not yet established, I do not undertake to explain such possible variation. But one can still speculate . . .

the discovery of new problems for further inquiry (Merton [1963a]1973:371–82). SRMs take differing forms in the various disciplines: among them, the (location) strategic research site (SRS) and the (temporal) strategic research event (SRE). Differing in operative detail, these forms have much the same functions. Just as the invention of new technologies for scientific investigation can facilitate the advance of scientific knowledge, so with the finding or creating of SRMs.

The concept of SRM provides a guide to the understanding of certain turning points in the sciences. Problems that have long remained intransigent become amenable as investigators identify new kinds of empirical materials that effectively exhibit the structure and workings of the phenomena to be understood. An inventory of SRMs in the history of the various sciences would, of course, run to unconscionable, not to say unmanageable, length, but even this capsule account has room for a conspicuous few, drawn from various times, places, and disciplines.

At times, scientists create an SRM or select one by design; at other times, they come upon such material serendipitously, recognizing its strategic character for the study of a particular problem only afterward. The seventeenth-century father of embryology, Marcello Malpighi, provides an SRS of the first kind: He elected to examine the lungs of frogs microscopically because of their great "simplicity and transparency" and thus observed for the first time so fine a feature as the capillary, not otherwise observable through microscopes of the time. It was this SRS—a "microscope of Nature" as the metaphor has it—that enabled Malpighi to see the blood move through the capillaries and thus helped him to round out Harvey's understanding of the greater circulation of the blood (Wilson 1960:165; Adelmann 1966).

A truly classic case of the second, serendipitous, kind of SRS was inadvertently provided by the Canadian trapper, Alexis St. Martin, when he suffered a gunshot wound that opened a large and permanent fistula into his stomach. This enabled his physician-and-friend, the early nineteenth-century physiologist, William Beaumont, to "look directly into the cavity of the Stomach, and almost see the process of digestion," as he put it in his notebook upon going on to his long series of pioneering experiments. The successful use of this serendipitous SRS in turn led the French chemist, Nicolas Blondlot (father of the hapless René), to create SRSs systematically by introducing similar fistulas in animals. But it was Beaumont's ingenious use of the singular fortituous SRS that deeply impressed the incomparable physician-humanist, William Osler, who held that it had led this "backwood physiologist" to the most consequential contributions to the physiology of digestion made in the nineteenth century. So impressed was Osler that, upon St. Martin's death—57 years after his scientifically fruitful accident (and his subsequent fathering of 20 children)—he wanted to conduct a postmortem examination and to deposit that strategic stomach, hole and all, in an appropriate museum. To round out the episode, I should report that intent was not translated into event: Osler refrained, upon receiving a warning telegram from St. Martin's French-Canadian community that read "Don't come for autopsy; will be killed" (Osler 1908:159–88, Cushing 1925: I, 177–79).

The early geneticists and especially the more recent molecular biologists hit upon a multitudinous variety of materials that strategically exhibit processes of reproduction and replication and lend themselves to the requisite research. In touching upon these, I surely indict myself as one of those benighted characters who insist on carrying coals to Newcastle, faggots into the wood, owls to Athens, and the concept of SRM to biology. I can only plead that biology is a self-exemplifying case: the history of biology itself provides strategic research materials for the study of the selection and consequences of strategic research materials.

Some time ago, there were, of course, Mendel's pea plants and then, de Vries' "pure species" of evening primrose with the ensuing complex story of his discovery of "mutation" (Mayr 1982:742–44). Harriet Zuckerman's unpublished inventory [1964] of research materials utilized in Nobel prize-winning work is fairly saturated with SRMs that gave rise to new lines of genetic inquiry and discovery. Among the many, I note only Morgan's choice of the fruit fly, so " 'easily and cheaply bred in the laboratory' " (Morgan in Allen 1975:331); Beadle & Tatum's "daring and astute selection of experimental material," the red bread mold *Neurospora crassa,* enabling them to advance biochemical genetics; Tatum & Lederberg's choice of *E. coli K-12* leading to the discovery of genetic recombination in bacteria and laying a "foundation of bacterial genetics and what has flowed from it" (Zuckerman & Lederberg 1986; Lederberg 1951, 1986); and to go no further, "that material of great convenience for studying many aspects of virus behavior," the filtrable virus bacteriophage (fondly shortened to phage) which, after their first collaboration in 1940, Delbrück and Luria converted into the SRM of that thought collective known as "the Phage group" that has contributed so much to the rise of molecular biology (Cairns et al 1966).

The recurrent pattern is one of identifying lineaments of materials that make them strategic for investigating a range of otherwise inaccessible scientific problems. Outside the sphere of genetics, Szent-Györgi's Hungarian paprika provided a rich source of ascorbic acid, enabling him to discover the role of Vitamin C in biological combustion, just as the newly available germanium and silicon crystals enabled Shockley, Bardeen, and Brattain to discover the transistor effect. Understandably, research workers become devoted to—not to say, captivated by—their fruitful SRMs. Hodgkin pays tribute to the nerve fiber of his giant squids as "an absolute gold mine," opening up all sorts of possibilities for study of physiological mechanisms in the transmission of messages.

My colleague, the neurobiologist, Eric Kandel, has also been known to wax eloquent about his prime SRM, the sea snail *Aplysia californica,* with its large and accessible nerve cells allowing investigation in molecular terms of such complex processes as learning and memory. Looking back on an earlier "encounter between neurobiology and molecular biology," he observes:

> These intellectual precursors shared an experimental approach that depended on model building and therefore on a willingness to study preparations that best exemplified the phenomena of interest. This led to a search for conveniently simple systems that provided abundant material. Thus, geneticists interested in inheritance in higher organisms first studied *Drosophila* and *Escherichia coli;* crystallographers first analyzed keratin and hemoglobin; and molecular biologists interested in replication of DNA studied bacterial viruses. Although the impetus was to understand complex phenomena, study was governed by optimization of simple experimental systems and by the presumed universality of the phenomena chosen for study (Kandel 1983:891).

Quite evidently, then, the biological sciences have long involved the search for SRMs and their sustained intensive investigation. That experimental tradition is at a considerable remove from the largely nonexperimental work in the social and behavioral sciences. Nevertheless, in those disciplines also we observe a hunt for empirical materials, research sites, and events that are judged strategic for investigating a generic scientific problem and for identifying new problems. Still, there is at times a profound difference in the orientations of biological scientists and social scientists toward the phenomena they establish and investigate. To a degree, that difference relates to the well-known distinction proposed by the philosopher Wilhelm Windelband (1884) and substantially developed by his student, Heinrich Rickert ([1902] 1921). That is the distinction between the *Naturwissenschaften* (readily translated as "the natural sciences") and the *Geisteswissenschaften* (not as readily and variously translated as "the human sciences," "the social sciences," or perhaps as "the sociocultural sciences"). Associated as he was with Rickert in several respects, Max Weber (1922) nevertheless transcended the Windelband-Rickert distinction between the natural sciences as adopting methods exclusively designed for nomothetic or generalizing objectives and the social sciences as exclusively adopting quite different methods for understanding the idiographic or individual character of a sociocultural reality.

For in place of this drastic, all-or-none choice between the two methodological orientations, one may choose the composite of intrinsic interest in understanding the particular "historical individuals"—for example, the capitalistic society of nineteenth-century England or the French Revolution or, for that matter, the Great Depression of the 1930s—*and* of an instrumental interest in those sociocultural phenomena as instructive specimens leading to discovery of general regularities which can then be drawn upon to understand other historical individuals. Thus, Sorokin (1925) examines a variety of revolutions over the centuries—from ancient Rome to our own time—to arrive at his nomothetic or generalizing work, *The Sociology of Revolution,* and to reach an understanding of the Russian Revolution he experienced at first hand. Or again, Thomas & Znaniecki (1918–1920) examine the historical case of *The Polish Peasant in Europe and America* both for its distinctive ("unique") characteristics and for its presumably generic patterns of social and personality change.

In short, it is being proposed that the history of sociological inquiry has its own complement of researches which relate variously to the use of strategic research sites and events: In one type, the empirical case is selected wholly because of intrinsic interest in it as a historical individual on grounds of its relevance to values *(Wertbeziehung),* which Rickert held to be distinctive of the *Geisteswissenschaften.* In another type, the empirical case is regarded wholly as an SRS or SRE leading to provisional generalizations. And in what I take to be the most felicitous mode, the concrete materials hold both intrinsic interest as involving human values and instrumental interest as an SRS or SRE that may advance our general sociological knowledge.

Karl Marx provides us with a prime early instance of this last type. In the preface to the first German edition of *Capital,* he begins with a not uninteresting allusion to the logic of inquiry adopted by physicists and then goes on to the rationale for adopting a particular site for his own inquiry:

> The physicist either observes physical phenomena where they occur in their most typical form and most free from disturbing influence or, wherever possible, he makes experiments under conditions that assure the occurrence of the phenomenon in its normality. In this work I have to examine the capitalist mode of production, and the conditions of production and exchange corresponding to that mode. Up to the present time, their classic ground is England. That is the reason why England is used as the chief illustration in the development of my theoretical ideas (Marx [1867] 1906:12–13).

Marx goes on to elucidate the choice of England as an SRS(ite) by maintaining that the country which is "more developed industrially only shows, to the less developed, the image of its own future." And then, almost in the manner of an early biologist assaying a potential SRS, Marx assesses the research value of his elected case by noting that "The social statistics of Germany and the rest of Continental Western Europe are, in comparison of those of England, wretchedly compiled." Although this SRS scarcely has the same quality of exhibiting closely reproducible regularities on demand as SRMs in the physics to which Marx refers, it does not seem too much to suggest that Marx's choice of his SRS has had its own array of notable consequences, cognitive as well as social.

The sociological literature is chockfull of work that combines intrinsic interest in the particular sociocultural case with instrumental interest in it as leading to provisional general conclusions. Here, it is enough to instance Max Weber's monumental volumes (1910–1921) in the sociology of religion, with their intensive sociological analyses of Protestantism, Confucianism and Taoism, Hinduism and Buddhism, and ancient Judaism. The idiographic analyses of these historical materials which hold great intrinsic interest for many of us are powerfully joined with their instrumental use as SRSs leading to nomothetic hypotheses about such abstract sociological problems as the relations between institutionalized ideas and social organization as well as the modes and dynamics of structural interdependence of seemingly unconnected social institutions—all this best exemplified by the interplay between religious ideas and economic developments, not least in the prototypal case of ascetic Protestantism and the emergence of modern capitalism.

Other founders of modern sociology worked with a variety of strategic research sites and events. Durkheim, of course, notably so in his analyses of the division of labor, suicide, religious ceremony and ritual, and moral education, among others. As Hanan Selvin pointed out to me in correspondence (1976) on the evolving concept of SRS, Durkheim's first empirical study of suicide in 1888, antedating his famous monograph by a decade, rested on the strategic selection of "European nations as the units of recording and analysis. The availability of suicide rates as [assumed] indicators of national unhappiness was surely what led him to make this choice." Elsewhere, Selvin (1976) notes how Durkheim adopted a more fine-grained SRS to analyze—as it happens, erroneously—relationships between the proportions of German-speaking people and the suicide rate in 15 provinces of Austro-Hungary, this in an effort to identify the effects of German culture on the suicide rate while presumably neutralizing the effects of possible genetic dispositions to suicide. It is in this context, after the manner of one subjecting natural history to systematic analysis, that Durkheim (1888) emitted the metaphor: "Austria offers us the complete natural laboratory"—a kind of metaphor often echoed by Park, Burgess and others of that remarkable group of sociologists that made the city of Chicago a sociological "laboratory."

Familiar empirical materials were put to unfamiliar theoretical use. Thus, Durkheim (1899–1900) and, in kindred fashion, George H. Mead (1918) elected to tackle the problem of the social bases of moral indignation, integral to an understanding of mechanisms of social control, by turning to situations in which people react strongly to violations of social norms *even though they are not directly injured by them.* Systems of punishment and behavioral responses to violations of deep-seated rules provided an SRS not so much for the then-and-since traditional problem of the deterrent effects of punishment in curbing crime as for the problem of their other societal functions; in Mead's language, the "uniting all members of the community in the emotional solidarity of aggression." In an Excursus of the kind to which he was much given, to the lasting benefit of the rest of us, Simmel ([1908] 1950:402–408) focused on "the phenomenon of the stranger" in order to analyze how "the unity of nearness and remoteness in every human relation is organized" just as, in direct theoretical continuity, Park (1928) focused on the behavior of immigrants as providing strategic materials for coming to understand the structural bases of "the marginal man"—the men and women who, living in disparate social worlds, do not feel at home or fully accepted in any of them.

Following upon these and many another early prototype, the exponentially growing numbers of sociologists have adopted a numerous variety of strategic research sites and events. But of all these, nothing more can be said here. Instead, I obey the injunction of the Editors of *Annual Review* to make this prefatory piece as personal as I can bring myself to do and close out these capsule notes on the concept of SRM in two steps. First, I want to examine a turning point in the history of psychoanalysis that I have often singled out as a classic instance of an acute theoretical sensibility—to wit, Freud himself—transmuting seemingly trivial phenomena into strategic research material (however different present-day appraisals of that material may be). This invites attention to the apparently paradoxical theme of the occasional, perhaps frequent, importance in science and scholarship of what appear to be humanly insignificant phenomena. From that historic episode I move to three distinctly minor efforts on my own part to set forth an explicit rationale for adopting various kinds of sociological SRMs.

The "Trivial" as Strategic Research Material

It was back in the 1940s that I first found myself focussing on Freud's analytic decision to study seemingly trivial mistakes in everyday life as "strategic" in the sense being developed here:

> . . . in noting that the unexpected fact must be 'strategic,' *i.e.* that it must permit implications which bear upon generalized theory, we are, of course, referring rather to what the observer brings to the datum than to the datum itself. For it obviously requires a theoretically sensitive observer to detect the universal in the particular. After all, men had for centuries noticed such 'trivial' occurrences as slips of the tongue, slips of the pen, typographical errors, and lapses of memory, but it required the theoretic sensitivity of a Freud to see these as strategic data through which he could extend his theory of repression and symptomatic acts (Merton 1948:507).

Freud had signalled his intention of transmuting these seemingly trivial matters into basic theoretical matters by the emphasis given them in the title of the book where he first dealt systematically with them: *The Psychopathology of Everyday Life: Forgetting, Slips of the Tongue, Bungled Actions, Superstition and Errors* (Freud [1901] 1960). He proceeded to group these varied mishaps in the coined word-and-concept, *Fehlleistungen* (a psychological oxymoron translated in *The Standard Edition . . . of Freud* by the made-up Greek-like word, "parapraxes"[4] but as Bettleheim has tellingly noted, best rendered as "faulty achievements." Returning intensively to these same matters 15 years later in his *Introductory Lectures on Psycho-Analysis,* Freud forcefully states the case for his focussing on these "apparent trivialities":

> It is to these phenomena, then, that I now propose to draw your attention. But you will protest with some annoyance: 'There are so many vast problems in the [wide] universe, as well as within the narrower confines of our minds, . . . that it does really seem gratuitous to waste labour and interest on such trivialities. . . .'
>
> I should reply: Patience, Ladies and Gentlemen! I think your criticism has gone astray. It is true that psycho-analysis cannot boast that it has never concerned itself with trivialities. On the contrary, the material for its observations is usually provided by the inconsiderable events which have been put aside by the other sciences as being too unimportant—the dregs, one might say, of the world of phenomena. But are you not making a confusion in your criticism between the vastness of the problems and the consciousness of what points to them? Are there not very important things which can only reveal themselves, under certain conditions and at certain times, by quite feeble indications? (Freud [1916] 1961:26–27).

Freud is telling his audience that the seeming insignificance of these "phenomena" for everyday life says nothing about their significance for psychological science. That observation on the strategic theoretical value of such slips and errors holds quite apart from their evidentiary value for Freud's own theory that they result from repression [as is clear from the thoroughgoing analysis of Freud's "flawed reasoning" and from the review of alternative explanations of these phenomena by the philosopher of science, A. Grünbaum (1984:190–21)].

I cannot dwell on the enduring theme of the potential importance of the seemingly trivial in science and scholarship as it has appeared over the centuries—the seventeenth, for example, was chockfull of this theme, both as understood and as misunderstood. A few archetypal observations to this effect in our own century must serve. Having been taxed from time to time for

[4]As others have held and as Bettelheim has emphatically observed, this awkward term is a misleading translation of a central concept. Unable to improve upon Bettelheim's analysis, I do service by transmitting it here: "Freud coined *Fehlleistung* to signify a phenomenon that he had recognized—one that is common to the various ways in which our unconscious manages to prevail over our conscious intentions in everyday occurrences. The term combines two common, strangely opposite nouns, with which everybody has immediate and significant association. *Leistung* has the basic meaning of accomplishment, achievement, performance, which is qualified by the *Fehl* to indicate an achievement that somehow failed—was off the mark, in error. What happens in *Fehlleistung* is simultaneously—albeit on different levels of consciousness—a real achievement and a howling mistake. Normally, when we think of a mistake we feel that something has gone wrong, and when we refer to an accomplishment we approve of it. In *Fehlleistung,* the two responses become somehow merged: we both approve and disapprove, admire and disdain". (Bettelheim 1983:86–87).

attending to the apparently insignificant, Veblen (1932:42) took one occasion to observe that "All this may seem to be taking pains about trivialities. But the data with which any scientific inquiry has to do are trivialities in some other bearing than that one in which they are of account." And inevitably, in these reminiscent pages, I am put in mind of how this matter was being reiterated by teachers at Harvard during my time as a student and instructor there. Here is the biochemist and self-taught social scientist, L. J. Henderson, typically diluting his cogent observations by his passionate Paretan insistence that social scientists really must learn to quell their passions:

> This illustration has been chosen because, among other reasons, it is a simple case that is likely to seem trivial. Note well, however, that nothing is trivial, but thinking (or feeling) makes it so, and that we must ever guard against coloring facts with our prejudices. There was a time not so very long ago when electro-magnetic interactions, mosquitoes, and microorganisms seemed trivial. It is when we study the social sciences that the risk of mixing our prejudices and passions with the facts, and thus spoiling our analysis, is most likely to prevail (Henderson [1941] 1970:19; see also Bernard Barber's comment introducing this passage in Henderson's oral publication which Barber arranged to have put into print).

Whether Henderson alerted Talcott Parsons to this theme of the possible scientific importance of otherwise trivial phenomena, I cannot say. He may have done so during his close editing of Parsons' masterwork, *The Structure of Social Action* (1937), for, as the Preface gratefully states and as we young colleagues of them both knew, Henderson had "subjected the manuscript to important revision at many points, particularly in relation to general scientific methodology. . . ." In any case, Parsons picks up and develops the theme in the important section entitled "Theory and Empirical Fact," which virtually opens his immensely consequential treatise:

> A scientifically unimportant discovery is one which, however true and however interesting for other reasons, has no consequences for a system of theory with which scientists in that field are concerned. Conversely, even the most trivial observation from any other point of view—a very small deviation of the observed from the calculated position of a star, for instancc—may be not only important but of revolutionary importance, if its logical consequences for the structure of theory are far-reaching (Parsons 1937:7–8).

In summary, then, it has long been recognized in a variety of disciplines that there is no necessary relation between the socially ascribed importance of the empirical materials under study and their importance for the better understanding of how nature or society works. The scientific and the human significance of those materials can be, although most emphatically they need not be, poles apart. This is often lost to view when a charge of triviality rests wholly on a commonsense appraisal of the subject-matter alone, as it often is, for example, by satirizing members of Congress. In gauging the human significance of the sociological *problem* rather than the empirical materials, many of us have argued, we sociologists have found no better general criterion than that advanced by Max Weber in the concept of *Wertbeziehung* (value relevance). Their values may lead scientists to refuse to work on certain scientific problems—for example, research that will lead to still more catastrophic weapon systems—or may lead them to focus on other scientific problems—for example, research on cancer or on the social mechanisms that perpetuate racial discrimination. There still remains the question of identifying the research materials that enable one to investigate these humanly important problems most effectively, the question of hitting upon strategic research sites or events.

In saying all this, I prefer not to be misunderstood. It is surely not the case that SRMs for investigating a particular scientific problem must be humanly trivial. Nor is it being said in the mood defensive that there is no authentically trivial work in today's sociology any more than it can be said that there was no trivial work in, for instance, the physical science of the seventeenth century. Our journals of sociology may have as impressive a complement of authentic trivia as the *Transactions* of the Royal Society had during its first century or so. But these are trivia in the strict rather than the unthinking rhetorical sense: They are inconsequential, both intellectually and humanly. The central point is only this: The social and the scientific significance of a concrete subject can be—although, of course, they need not be—of quite different magnitudes.

Some Personal Choices of SRMs

Responding again to the Editors' amiable reminder that these prefatory essays generally call for personal moments, I sketch out three abbreviated rationales, early and late, for my selecting or adopting certain empirical materials that seemed strategic for studying particular problems in sociology and social psychology.

I think back to the ancient days of 1943 and my interest, largely stimulated by my newfound collaborator, Paul F. Lazarsfeld, in understanding the workings and consequences of mass propaganda. "The radio marathon," then a wholly new historical phenomenon, promised to provide a strategic case for investigating the collective behavior of mass persuasion. In the course of 18 consecutive hours on the air, the pop singer Kate Smith, widely identified as the sincere patriot incarnate, spoke a series of prepared texts on 65 occasions, and elicited the then unprecedented sum of $39,000,000 in war bond pledges (Merton et al 1946). From the start, the concrete idiosyncratic and behavioral materials were delimited from their potential scientific interest: "Although her name inevitably recurs time and again throughout the book, this is *not* a study of Kate Smith." Rather, the collective bond-drive would "provide a peculiarly instructive case for research into the social psychology of mass persuasion."

Severely condensed, the stated attributes of this assumed strategic research event were these: First, it was a "real-life" situation, not an isolated, recognizably contrived situation of the kind that limits the transferability of laboratory findings in social psychology to the world outside. Second, the bond purchases provided a behavioral index of effective persuasion which, however crude, was far better than the hypothetical pencil-and-paper responses common in the laboratory research of the time. Third, there was reason to suppose that the event would be emotionally freighted in varying degree for listeners, both those who pledged bond purchases and those who did not. Fourth, unlike field studies of other collective behavior, such as race riots, we would have full and sustained access to parts of the developing collective situation in the form of content analyses of the recorded broadcasts. Fifth, the self-selected individuals and groups engaging in this behavior would come from widely differing social strata rather than being drawn, after the fashion of the time (and, often enough, today) from the dependent, rather homogenous aggregates of college students dragooned as "subjects" by their instructors. Finally, it was assumed that this attempt at truly mass persuasion would link up with identifiable sociocultural contexts.

In the course of the study, we did find such social phenomena, among them, the operation of "pseudo-Gemeinschaft" (the feigning of common values and primary concern with the other as a means of advancing one's own interests); processes in the formation of what we described by the new concept of "public image"; and a pervasive public distrust. Not least, in unanticipated and self-exemplifying fashion, the study reactivated a sense of the moral implications of the framing of scientific problems in one or another fashion, leading to a specific elucidation of the Rickert-Weber idea of *Wertbeziehung* that questioned a naive form of positivistic orientation common at the time:

> [The] social scientist investigating mass opinion may adopt the standpoint of the positivist, proclaim the ethical neutrality of science, insist upon his exclusive concern with the advancement of knowledge, explain that science deals only with the discovery of uniformities and not with ends and assert that in his role as a detached and dispassionate scientist, he has no traffic with values. He may, in short, affirm an occupational philosophy which appears to absolve him of any responsibility for the use to which his discoveries in methods of mass persuasion may be put. With its specious and delusory distinction [in this context] between 'ends' and 'means' and its insistence that the intrusion of social values into the work of scientists makes for special pleading, this philosophy fails to note that the investigator's social values do influence his choice and definition of problems. The investigator may naively suppose that he is engaged in the value-free activity of research, whereas in fact he may simply have so defined his research problems that the results will be of use to one group in the society, and not to others. His very choice and definition of a problem reflects his tacit values (Merton et al 1946:187–88).

And so on in further specifying detail drawn from this study of a public event involving mass persuasion and the workings of what came to be described as "technicians in sentiment." The point of dwelling on these matters, I suppose, is simply to note, once again, that however focused an SRS is for the investigation of previously identified problems, it may lead to other, unanticipated, findings and problems.

By the way of necessarily quick conclusion, two contrasting episodes also involving my own work may illuminate a general point regarding SRMs in sociology: Studies of social institutions, social movements, and other macrosociological inquiries require little explicit rationale, since their relation to values is taken as self-evident but the selection of seemingly peripheral, innocuous, or 'trivial' social data as strategic for investigating basic sociological problems, does, precisely because of the seeming distance of the data from prized values.

Back in the 1930s, when the sociology of science was far from having been legitimated as a scholarly field of inquiry, even the historians of science most critical of a study of the social and cultural contexts of the efflorescence of science in seventeenth-century England (Merton [1938] 1970) did not question its scholarly relevance. Some were even prepared to accept, on probation, its substantive hypotheses of linkages between Puritanism and the emergence of the new science as well as its hypotheses of the partial shaping of foci of scientific interest by economic and technological developments of the time. Some went on to take friendly note of the use in that study of the then newly developed procedures of prosopography (analysis of collective biography) (Stone 1971:50–51, Shapin & Thackray 1974:22) and the quantitative analysis of changing scientific foci through content-analysis of the new journal of the new science, *Philosophical Transactions*. *Wertbeziehung* gave immediate scholarly warrant to the "subjects" under study.

Not so, however, with another study of mine two decades later. In it I had elected to focus on a recurrent phenomenon in science over the centuries, though one which had been ignored for systematic study: priority-conflicts among scientists, including the greatest among them, who wanted to reap the glory of having been first to make a particular scientific discovery or scholarly contribution. This was paradoxically coupled with strong denials, by themselves and by disciples, of their ever having had such an "unworthy and puerile" motive for doing science.[5] The initial and subsequent response to that study is captured in a remarkably candid account by the historian of science and editor-in-chief of the 16-volume *Dictionary of Scientific Biography*,

[5]Those self-deprecatory words are Freud's. Still, his biographer and disciple, Ernest Jones (1957, III:105) writes that "Freud was never interested in questions of priority which he found merely boring," thus providing another case of fashioning a biographical pseudo-fact although abundant and accessible evidence testifies otherwise. Elinor Barber and I have identified some 150 occasions on which Freud exhibited an interest in priority. With typical self-awareness, he reports having even dreamt about priority and the credit normatively due scientists for their contributions (Merton [1963] 1973:385–91).

Charles C. Gillispie (1974:656–60). A colleague at a distance, he first responded with considerably less than enthusiasm. I can do no better than have him tell the telling story:

> Some years ago, probably in early 1958, Merton sent me an offprint of . . . his presidential address to the American Sociological Association on "Priorities in Scientific Discovery" [(1957) 1973:286–324]. It starts by noting (pp. 286–87) "the great frequency with which the history of science is punctuated by disputes, often by sordid disputes, over priority of discovery." As I read on, dismay overtook amusement at the parade of eminent scientists arguing and frequently quarreling with each other, not over what the truth was, but over who had it first, Newton or Leibniz, Newton or Hooke, Cavendish or Watt or Lavoisier, Adams or LeVerrier, Jenner or Pearson or Rabaut, Freud or Janet. Sometimes the great men themselves abstained from contending in the lists of professional recognition for title to their intellectual property only to have their claims championed by disciples or compatriots. All too clearly the particular instances that Merton adduced in a number of variations on the theme of intellectual possessivcness could have been multiplied almost indefinitely.
>
> In a note of acknowledgment to Merton, I wrote that, although it seemed surprising that the phenomenon was so nearly universal an accompaniment to scientific discovery, I did wonder whether the matter wasn't a bit trivial. I don't believe I also said "unworthy" but recollect that such a dark thought was in my mind (Gillispie 1974:656).

I do not recall Gillispie actually having said "unworthy." He did, however, signal his friendly concern over my having lavished so much attention on the distinctly minor "subject" of priority-conflicts. But a change in his own theoretical perspectives on the scope and character of the historiography of science evidently led to a changed perception. He no longer took the descriptive raw materials of priority-conflicts as the subject-matter in hand; rather, he came to see them for what they were being redesigned to be: as strategic research materials for identifying the reward system distinctive of the social institution of science, one in which peer recognition of original scientific work was the golden coinage of the scientific realm. Gillispie also came to see that the sociological analysis of priority-conflicts as SRMs led one to find a contradiction between that reward system and other parts of the social and normative structure of science, such as the system of free and open communication (at least, for scientists outside the world of industry).

Gillispie indicatively describes this shift in perception:

> Only a few years later, when I began to study and teach materials in the social and institutional as well as the more traditional internal and intellectual history of science, did I come to take the full thrust of what he had in fact said, and said clearly and convincingly. It was that such behavior occurs in service to social norms; that norms arise in the life of real communities governing the conduct of their members; that the phrase 'scientific community' is, therefore, no mere manner of speaking about some shared pleasure in the study of nature but refers to an effective social entity; and that, within its membership, which is bounded professionally and not geographically, two main sets of norms constrain behavior and do so in ways that conflict, the one enjoining selflessness in the advancement of knowledge, and the other ambition for professional reputation, which in science accrues from originality in discovery and from that alone. The analysis exhibits the scientific

community to be one wherein the dynamics derive from the competition for honor even as the dynamics of the classical economic community do from the competition for profit, and neither of those statements is in any way incompatible with agreeing that the competitors characteristically like their work and choose it for that reason (Gillispie 1974:656).

Gillispie goes on to report that the substance of James Watson's *The Double Helix* (1968) came as no surprise. After all, that confessional account of intense competition and marginal if not sharp practices in the author's quest for a Nobel variously exemplified what was set down in the sociological analysis of intellectual property and the race for priority in science which had appeared a decade before.

One further observation will round out this impromptu case study of a strategic research site in the sociology of science. Were Charles Gillispie reflecting on his shifting response to that early study of priority-conflicts today, after the quite recent spate of concern over the occurrence of fraud in science, he might have elucidated his account further. He might have gone on to observe that the study had proposed the strongly stated hypothesis that contradictions between the reward-system and the normative system of science made for such pathologies as the occasional felonies of plagiarism and the cooking of fraudulent data, the presumably more frequent misdemeanors of hoarding one's own data while making free use of others' data, and the breaching of the mores of science by failing to acknowledge the contributions of predecessors, the collective giant on whose shoulders one stood to see a bit or, rarely, a great deal farther. Gillispie might have noted that this 1957 paper was the first to set out a sociological analysis of fraud in science, a good many years before currently publicized cases of such scientific felonies had forced widespread attention, both scholarly and popular, to the phenomenon (Zuckerman 1977, Broad & Wade 1982). Now for me to visit this observation on Charles Gillispie might be taken as a self-exemplifying claim to priority (as no doubt it is). But the chief point is less a matter of priority than of attending to the sources of that early sociologically grounded focus on the phenomenon of fraud in science. That focus derived theoretically from anomie-and-opportunity-structure theory and empirically from the selection of priority-conflicts as a strategic research site. And this, in turn, suggests that once problems are theoretically identified, materials that were previously peripheral or of no interest at all become reassessed as, in effect, strategic research materials.

CODA

It is now plain why the preceding pages are described as fragments. Obviously, much more is needed to establish these three patterns of scientific practice as phenomena, to specify our current ignorance about each of them in the form of new feasible problems, and to propose a range of research materials strategic for their solution. To my way of thinking, that is work for the near future.

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APPENDIX: THE MENU

(On the suggestion of several readers by way of providing context, I append the list of subjects from which these three were drawn.)

I. Patterns of Scientific Practice

1. Establishing the Phenomenon
2. Specified Ignorance
3. Strategic Research Materials
4. Fact as Theory-Laden: A Periodic Rediscovery
5. Naive Falsificationism: When Trust Theory, When Trust Fact
6. Unanticipated Consequences of the Reward System in Science: A Model of the Sequencing of Problem-Choices (with R. C. Merton)
7. The Self-Fulfilling Prophecy in Scientific Work
8. Toward a Sociological Theory of Error:
   - 8a. Patterned Misunderstandings in Science and Learning
   - 8b. Fallacy of the Latest Word
   - 8c. The Phoenix Phenomenon
9. Disciplined Eclecticism
10. Confirmation and the Fallacy of Affirming the Consequent
11. A *Fortiori* Reasoning in the Design of Scientific Inquiry
12. Tacit Counterfactual History
13. The (William) James Distinction: Acquaintance With and Knowledge About
14. The (Kenneth) Burke Theorem: Seeing as a Way of Not-Seeing
15. The (L. J.) Henderson Maxim: It's a Good Thing to Know What You Are Doing

II. Patterns in Transmission, Change, and Growth of Scientific Knowledge

1. Selective Accumulation of Scientific Knowledge: Paradox of Progress
2. OBI: *O*bliteration (of Source of Ideas, Methods, or Findings) *b*y *I*ncorporation (in Canonical Knowledge)
3. "Trained Incapacity": A Case of OBI
4. Cognitive Conduits for the Blurred Central Message
5. The Retroactive Effect in the Transmission and Growth of Knowledge
6. The Matthew Effect II: Accumulation of Advantage and the Symbolism of Intellectual Property
7. Oral Publication and Publication in Print
8. The Scientific Paper as Tacit Reconstruction of Knowledge
9. Insiders and Outsiders: Privileged Access to Knowledge
10. The Adumbrationist Credo: What's New is Not True; What's True is not New
11. The Symbolism of Eponyms in Science
12. Fathers and Mothers of the Sciences
13. Fraud and Other Deviant Behaviors in Science: A Case of Goal Displacement
14. Taboo Knowledge
15. Givens: The 'Of-Course Mood' in Scientific Discourse
16. Francis Bacon as Sociologist of Knowledge
17. Organized Skepticism: The Social Organization and Functions of Criticism in Science and Scholarship

III. Neologisms as Sociological Concepts: History and Analysis

1. On The Origin and Character of the Word *Scientist*
2. Self-Exemplifying Ideas: in the Sociology of Science and Elsewhere
3. Influentials: Evolution of a Concept
4. Institutionalized Evasions and Other Patterned Evasions
5. SED: Socially Expected Durations as a Temporal Dimension of Social Structure
6. Homophily and Heterophily: Types of Friendship Patterns
7. "Whatever Is, Is Possible": A Brief Biography of the Theorem
8. Opportunity Structures: A Brief Biography of the Concept
9. "Haunting Presence of the Functionally Irrelevant Status": The Structural Analysis of Status-Sets
10. "Phatic Communion": Malinowski's Need of a Cognitive Conduit
11. Comte's "Cerebral Hygiene" and the Presumed Dangers of Erudition for Originality

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