HOW DOES
SOCIAL
SCIENCE
WORK?

Reflections on Practice

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Macrosociology of Social Science

Science is an activity of human beings acting and interacting, thus a social activity. Its knowledge, its statements, its techniques have been created by human beings and developed, nurtured, and shared among groups of human beings. Scientific knowledge is therefore fundamentally social knowledge. As a social activity, science is clearly a product of a history and of processes which occurred in time and in place and involved human actors. These actors had lives not only in science, but in the wider societies of which they were members.

—Everett Mendelson (1977, pp. 3–4)

THE MERTONIAN PARADIGM

THE MODERN macrosociology of science begins with the work of Robert Merton in the 1930s. The evidence for the above statement consists of citation counts of journal articles (Cole and Zuckerman, 1975), reports of informants (Storer, 1973, esp. p. xi), and acknowledgments and tracing of ideas in leading works such as Hagstrom (1965). Many sociologists of science since 1960 were Merton’s students or students of students (Cole and Zuckerman, 1975, p. 155), and others took up and developed some of his ideas, for instance, Blissett (1972) and Mitroff (1974a). When the Society for Social Studies of Science (4S) was founded in 1975, Merton was invited to be its first president.

Merton’s first work, his Ph.D. thesis, was a study of the institutionalization of science in seventeenth-century England. This was an “externalist” history of science; that is, it argued for the interdependence of science and extrascientific factors, both ideal and material: Puritan values and economic and military needs and resources. His later sociological work was internalist, ignoring the effects of society on science; Karl Mannheim’s 1929 work, Ideology and Utopia, was externalist, tracing the effects of class membership on the ideas and thought patterns of scientists, and reflecting on the implications for the nature of knowledge.
Thus Merton’s work contributed to a shift from the pre-1940 externalist sociology of science to later internalism.

The Mertonian paradigm for sociology of science was developed between 1938 and 1957 (Storer, 1973), and thereafter Merton and his disciples used it in their empirical studies. Paradigm is used here not in Kuhn’s sense of an exemplary empirical study that is imitated in later empirical work. It is used in Merton’s sense of a set of systematically related ideas, categories, and propositions that is used to suggest empirical research topics and to interpret data.

Merton’s paradigm is a theory of the social characteristics of Kuhn’s normal science. The ideas of the two theorists are very similar. Merton called Kuhn’s book “merely brilliant” (Cole and Zuckerman, 1975, p. 159; Merton, 1977, p. 105), and devoted thirty-eight pages of his 1977 memoir to Kuhn, compared to three pages for Popper. There are numerous positive references to Kuhn in the writings of Merton’s followers, far more than the references to the next most frequently cited authors: Imre Lakatos and Abraham Kaplan.

Merton’s paradigm is functionalist; it is a theory of the structural characteristics that are needed for a scientific community to maintain itself, and what happens when these characteristics are not adequately present. For a Parsonsian functionalist these are naturally among the fundamental facts to be known about any social institution. One conceives of society as an interaction among a small set of institutions, each with its own proper goal: the goal of the economy is to mediate between society and natural resources to produce goods and services, the goal of the family is to socialize new members, the goal of the polity is to maintain the system, and the goal of science is truth. Each goal derives from the various things a society needs to maintain itself. Each institution has its own proper values that direct it to its own goal and that maintain its autonomy from other institutions. Also each institution has its own structure of relations that hopefully operates to produce the needed results. A structural deficiency, or interference by other institutions, weakens the productive process and leads to deficient results, and this deficiency puts a strain on other institutions as well. All existing societies undoubtedly have deficiencies and strains, but they must also be doing something right or they would disappear.

Mertonian functionalism differs from that of Parsons on a few points. For one thing, Merton elsewhere (1949, chap. 1) emphasizes the possibility of functional alternatives, that is, the possibility that a society or institution can maintain itself in more than one way. In other words, a particular characteristic may be sufficient but not necessary for self-maintenance. For example, Merton asserts that though Puritan values in fact contributed to the development of science in seventeenth-century England, other values may have had the same effect had there been no Puritanism (1973, p. 192). However, Merton’s later paradigm does not specify possible functional alternatives for contemporary science. Second, Merton emphasizes functional and structural conflict somewhat more than Parsons does (Merton, 1976, pp. 126-32). A process may be functional for one institution and simultaneously dysfunctional for another, and a process that solves one functional problem will normally make another functional problem worse. Structural conflict normally exists within roles, between norms and counter-norms of a role, between roles within a status, between statuses of a status set, between normatively permitted goals and means (Merton, 1976, chap. 1). Merton asserted that his functionalism combines Marx and Durkheim, while his teacher Parsons’s functionalism is straight Durkheim (pp. 126-29). In Merton’s first book (1949) Durkheim is cited most frequently and Marx second.

The difference, however, is not that Merton recognized social conflict and Parsons did not. It is rather that Parsons transformed empirical conflict into analytical conflict between categories in his theory. Where Merton posited conflicting norms within a role, conflicts that could deeply disturb and disorient a person, Parsons described conflicts between the proper values of two different institutions. Thus the values of a family should be particularist-ascriptive while the values of an economy should be universalist-achievement, and mediating processes were functionally necessary to manage this conflict. However, by family Parsons meant L-subsystem, which included all activities that helped perform the L-function, and not a concrete family. A concrete family participated in all four subsystems, though mainly the L-subsystem. Similarly, the economy did not mean a particular tavern, which could be performing primarily L and I functions, plus a little A and G, but rather anything that contributed to the A-function. Consequently, a paralyzing conflict between ascriptive and achievement values in an individual becomes a conflict between A and L subsystems. Also when Parsons discussed particular groupings he focused on the mediating processes that buffered conflict: phase movement and dual leadership in families and small groups, and hierarchical levels in formal organizations. For Merton, however, society
meant particular communities, and the conflicts were empirically in
those communities.

Third, while Parsons, rhetorically at least, hailed the growth of a
single sociological paradigm—his own—Merton called for a plurality of
paradigms in sociology (1976, pp. 129-43).

For both Merton and Parsons a scientific community (that is, contem-
porary science) maintains itself insofar as it solves two functional prob-
lems: an internal one and an external one. Internally, any community is a
unity of differences or of opposites, an organic unity, as Durkheim called
it. Differences are needed for the division of labor involved in carrying
out the community’s actions; unity is needed to coordinate the actions.
Externally, the scientific community is an interdependent part of society
that yet must maintain some autonomy. It must be interdependent be-
cause the larger society provides the resources and uses the products of
scientific activity; but it must be autonomous because its goal, truth, is
different from the goals of other institutions. Too much autonomy would
lead to stagnation because of declining resources, and perhaps aimless-
ness because the product would not be used by anybody; too little auton-
omy would turn scientists into propagandists, politicians, businessmen,
industrial engineers, or thrust them into some other extrascientific role.

The scientific community solves its internal problem, unity in differ-
ence, by its shared values. Presumably all communities are united in part
by shared values, but the values of the scientific community must be
those that direct scientists to their proper goal. Merton asserted in 1942
that scientists in fact share four basic values: universalism, communism,
disinterestedness, and organized skepticism (1973, chap. 13). Merton
claimed to find these values in statements by leading scientists, in expres-
sions of approval and disapproval, and especially in expressions of moral
indignation.

Universalism is the norm that truth claims ought to be judged accord-
ing to pre-established impersonal criteria, not by the personal or social
attributes of the claimer. The opposite value, particularism, asserts that
people of a particular race, or class, or gender, or at a particular university
or research group, have special access to the truth or to some truth, so
that personal characteristics of the claimer are relevant to judging a truth
claim. Universalism rejects distinctions among scientists and thus in-
cludes them all within the same community, while particularism divides
them by nationality, school, age cohort, or some other category.

Communism is the norm affirming that the products of science be-
long to the whole community since they have been socially produced. A
corollary is that scientists ought to publish their findings and share them
in other ways. The opposite value, private property in ideas, is also
accepted to the extent that scientists are given recognition and esteem
for their achievements. But when scientists keep their research secret, as
with classified research, that retards scientific work by preventing others
from building on the secret achievements.

Disinterestedness means that scientists ought not promote their own
ideas ahead of others’ ideas, but should promote and use all relevant ideas
impartially. This value follows from the first two, which assert that all
truth claims should be treated equally and impartially. The opposite
value, interestedness, would enjoin journal editors to publish certain
ideas and authors and reject others, enjoin teachers to glorify their own
ideas and ridicule and distort others’ ideas, encourage advertising and
rhetoric. Merton does not assert that scientists never push their own
ideas; he is asserting that it is wrong to do so, and if everyone did it there
would be no collaboration, no scientific community.

Organized skepticism means that nothing—no beliefs, facts, values,
techniques, practices—should be immune to questioning and doubt.
Nothing is sacred. This value, however, should apply only within sci-
ence; the scientist as citizen may be emotionally committed to religious,
political, or economic values (1973, pp. 264-65).

A fifth norm, added later, was ethical neutrality toward one’s subject
matter (Elkana, 1976). Approval or disapproval of the people or behav-
or one is studying would bias the scientist toward finding characteristics
that fit a personal ethical attitude. As a result, scientists with different
attitudes would produce different interpretations of a topic, all of them
biased, and science would be fragmented along ethical lines.

Examination of these values reveals immediately that any science
founded on them will inevitably come into conflict with society. All
societies cherish some sacred elements, religious or otherwise, and there
is always the danger or suspicion that the scientist’s habitual skepticism
will spill over into social iconoclasm. Consequently, Merton observes,
there has been hostility to science from the seventeenth century on
because of its apparent questioning of received religion or the political
or economic system. Universalism conflicts with national patriotism, and
Merton reminds us of the nationalistic fervor even of scientists during
the two world wars; indeed, Merton himself expressed some of this wartime
fervor in his 1942 article (1973, chap. 13). Thus insofar as these values
solve the internal problem of unity they worsen the external problem of maintaining interdependence with the society. The two functional problems conflict.

The conflict is manageable insofar as the values of society are not too different from those of science. There will always be some differences—for instance, on patriotism and religion—but some conflict between science and society is desirable to maintain scientific autonomy. Merton argues that in seventeenth-century England the conflict was manageable because certain Puritan values agreed with the values of science: the equality of all men before God was a form of universalism; the belief in reason and experience rather than revelation was a form of disinterestedness and even limited skepticism. He also argued in 1942 that democracy, vaguely defined, embraces values hospitable to science: it affirms universalism, the equality of all people before the law, and rejects discrimination on grounds of race, creed, class, or sex. However, a capitalist democracy, which values private ownership of property, clashes with the value of communism, which forbids private property, and Merton gives examples of this conflict. Scientists are sometimes tempted to patent a discovery and make money on it, or to charge a consultant’s fee for using their expertise to discuss someone else’s research, or even to “pick someone’s brains” and then rush to publish the ideas under their own name. Thus ideas become private property, and free discussion is inhibited by fear of theft.

By 1957 Merton had turned his attention to a third functional problem, that of motivating scientists to produce new knowledge. The original list of norms was negative and procedural, but a growing science needs positive goals, and rewards for achieving them, to keep scientists moving. The motivation function is met by two new norms. One is the achievement of originality, a value directly connected to the goal of producing new knowledge. Scientists ought to produce new ideas, experiments, and techniques, and should be honored and esteemed when they do so. Achievement motivation is encouraged during the training of scientists, but is also carried over from early socialization in an achievement-oriented society. The opposite value, ascription, or being loved for who you are rather than what you have done, is appropriate for families but not for science. Science thus flourishes most easily in an achievement-oriented society.

A conflicting norm is humility, which combines the basic norms of disinterestedness and of skepticism toward one’s own achievements. The requirement of humility serves to moderate the divisive effects of achieve-

ment motivation, since without humility scientists would get absorbed in glorifying their own achievements and belittling others’, and the scientific community would break up into conflicting schools and egotistical individuals. However, the conflict between the achievement and humility norms produces a great deal of ambivalence, as scientists try to be humble and disinterested but still somehow claim the recognition due them.

Indeed, despite the humility norm a great deal of conflict does exist in American sociology, resulting partly from the drive for individual and group achievement and partly from the diffusion of social conflicts into science (Merton, 1973, chap. 3, originally 1961). The conflicts are intense enough to produce stereotyped distortions of opposing views, a breakdown of communication, hostility and name calling, rhetoric and contempt. Yet this conflict is functional for science, up to a point. It increases the solidarity of deviant schools, and thus strengthens them in their resistance to the dominant orthodoxy. The excitement of conflict attracts recruits to the dissenting schools and enables them to maintain the essential skepticism toward orthodoxy. Here Merton takes over the ideas of his student Lewis Coser (1956) about the positive functions of social conflict. Conflict is essential to the maintenance of community in a variety of ways.

In 1965 Merton generalized his 1957 observation that two norms can conflict: he provided a whole list of conflicting pairs (1976, chap. 2). Included in the list are versions of his four original norms: universalism, communism, disinterestedness, and skepticism. Apparently every scientific norm coexists with its own opposite, and scientists must ambivalently obey now one and now the other, according to circumstances. Here is another source of scientific conflict, as scientists urge opposite norms of a pair on each other.

The norms of science are in part maintained by socialization in the larger society, in family, school, and church. But in part they must be maintained by scientists. This responsibility falls mainly on leading scientists, the ones whose achievements have already been recognized and honored. These people already embody the norms of science, one hopes, so they set an example. They can also distribute recognition or censure to others, and especially can encourage dissent, skepticism, and humility. Insofar as they fail to do this or set a bad example themselves, they weaken the scientific community.

It is important to remember that for Merton the health of the scientific
community is not guaranteed. The functional requirements of science are not easily met, especially since they conflict; the norms need not be maintained, the necessary autonomy from society may weaken, dissent and skepticism may be stifled, leading scientists may set bad examples, and so on. For example, Merton himself was not always humble (Lang, 1981, p. 92; Merton, 1976, pp. 59-61). Indeed, the values of science themselves are self-destructive to some extent: "The culture of science is . . . pathogenic" (Merton, 1973, p. 323). Rose Coser (1975) has argued that Merton's primary focus is on conflict and contradiction, on the deviance and disorder produced by social institutions even at their best, and this focus is also apparent in his treatment of science. Similarly, Lewis Coser asserts, "Merton's world is composed of multiple ambiguities, of conflicting and contradictory demands and requirements" (1975, p. 5).

Merton's treatment of the pathology of science is based on his famous 1938 article, "Social Structure and Anomie," reprinted twenty-eight times from 1949 to 1975. The general idea is that culturally approved means may not be adequate to achieve culturally-prescribed ends, so that individuals must either use illegitimate means or fail to achieve ends, or both. They can't win.

He describes four forms of pathology. (1) Plagiarism, and data massaging and selection to fit one's theory. Here the original achievement goal is met by illegitimate means. (2) Repeated publication of trivial work and academic recognition for sheer quantity of publication; here the means of research and publication are used, but the goal of originality given up. (3) Withdrawal from research into teaching and academic administration; here both goal and means are given up. (4) Withdrawal into fantasies of future glory when one's masterpiece is finally published and acclaimed. An extreme example once occurred at the University of Chicago, where a French professor labored for decades on a definitive study of some writer, dropping occasional hints about its profundity; but at his death his will was revealed to contain a directive to burn the manuscript. Since the professor had not sufficiently recognized his genius during his lifetime, he explained, it was unworthy of receiving the fruits of that genius. Merton provides other examples (1973, p. 436).

Nevertheless, Merton affirms his belief that the values of science effectively minimize deviance, and science is basically sound. Hagstrom (1965) similarly asserts that anomie is rare in current science. The signs of anomie would include abundant publication of trivial papers that no one reads, little communication, and no agreement on the prestige ranking of scien-
tists. Hagstrom ignores the first form of pathology: use of illegitimate means such as data massaging. However, various studies such as Sahnner (1979) suggest that massaging is not uncommon. Another technique that Sahnner mentions is to use a computer to search for correlations in multivariate data, then invent a hypothesis that fits the discovered correlations and write a paper that begins by deriving that hypothesis from some existing theory and then claims to test it. Sahnner cites studies of published articles that find the null hypothesis falsified in 80 percent, 97 percent, and 100 percent of the articles.

We turn now to an examination of Merton's own values, through content analysis, in order to locate his paradigm, and of the Mertonian school, in science and in society. That is, we shift to externalist sociology.

In his early articles up to 1942, Merton expressed a rejection of Nazism for its racism and its totalitarian control of science. Against Nazism he asserted democratic values of respect for individual dignity and equal treatment of all without regard to race or creed. After 1942 the "defense of democracy" theme dropped out, and Merton concentrated on the self-maintenance needs of an autonomous science (cf. Hollinger, 1983). He did not participate in cold war anticommunism; for instance, he did not participate in the CIA-sponsored Congress for Cultural Freedom, unlike Daniel Bell or Ed Shils, among others. His chief cold war concern in the early 1950s, and Talcott Parsons's as well, was the McCarthy witch-hunting in the universities; this interfered with the autonomy of science. Marx for him was a sociologist, and Marxists had contributed many valuable ideas and also some bad theorizing to sociology.

From 1949 on, Merton expressed concern over the problems of policy-oriented sociology, and the conflict between science and politics. The political or industrial policy maker is a client who needs sociological research and advice to devise effective policies—for instance, a policy to curb anti-Semitism or to promote racial integration. But policy makers are likely to be shortsighted or have too narrow a grasp of the problem, and in any case they have no appreciation for the policy scientist's more general theoretical concerns. The scientist ought not do poor research to suit a client's narrow needs, and should not oversell science, raise exaggerated hopes, to get research grants. However, since practical problems are many-faceted, the policy sociologist ought to collaborate peacefully and tolerantly with people from other disciplines.

Merton's community was sociology, sharply defined, unlike Parsons, who combined psychological and sociological theory and knew some
economics. But within sociology he welcomed diversity and conflict, arguing that multiple cross-cutting conflicts avoid polarisation and maintain the unity of the discipline. He welcomed dissenting movements, arguing that orthodoxy was deadening to science. He even welcomed the microsociological dissenters against his own orthodox paradigm, while criticizing them gently (1973, pp. 373–75). Merton was a cosmopolitan, but his cosmos was sociology. In the late 1970s, however, he welcomed the convergence of disciplines as a development for the future (1977, p. 113).

Merton believed in the social responsibility of the corporation (1976, pp. 85–89). The increasing moral sensibility of our society calls on corporation leaders to improve employment opportunities for minorities, train the hard-core unemployed, clean up industrial pollution, and provide better public transportation.

From the above we can see that Merton, like Parsons, was a liberal New Deal Democrat committed to government and business action to solve social problems. He was neither a leftist critic of American society like Galbraith (Merton, 1973, p. 125) nor a glorifier of our great American democracy like the anticommitists. In science he and the Mertonians are professionals, committed to their own discipline and its skills and expertise. The discipline is policy-oriented: the professional sociologist’s partner-and-opponent is government. Government provides the financial support that makes large-scale research possible, and applies research findings to social problems. This implies a fundamental conflict between the professional’s commitment to systematic objective truth and the policy maker’s concern with specific, immediate problems. Each shares the other’s concerns, but the partnership prospers insofar as each preserves his own autonomy. Merton’s “professional” location in society determines the particular meaning of autonomy for him: autonomy from one’s patron, government. Similarly, Hagstrom’s worst nightmare is the possibility of political control of research by government funding agencies (1965, chap. 3).

Merton’s social location also determines the particular meaning of ethical neutrality for him. He is not at all neutral about the desirability of solving social problems—unemployment, poverty, race prejudice, pollution. Nor is he neutral about government and corporations; they are benevolent, responsible problem solvers. The norm of neutrality applies only during research into a problem. The researcher ought not to sympathise with parts of the problem, like the unemployed, since that would bias research: he would tend to overlook the contributions of the unemployed to the problem, and would assign more blame elsewhere. Nor ought the researcher favor one solution over another, for the same reason. The responsibility of selecting a solution belongs to government and corporations, since they are paying for it. The researcher can evaluate and criticize their solution if he has been neutral in diagnosing the problem.

A 1972 article, “Insiders and Outsiders” (in 1973, chap. 5), provides a view of further complexities in Merton’s position in society. The shifts in position in this article suggest either an unstable coexistence of contradictory values or changes in successive drafts. The topic is the black pride movement and the women’s movement, two dissenters against Mertonian orthodoxy. Their specific claim is that blacks have a special insight and understanding of black culture, and that women have a special understanding of women’s world. Merton first associates this claim of “black science” with Nazi “Aryan science,” and pulls out the old 1930s epithets of racism, ethnocentrism, and chauvinism. Against the claim of knowledge by ascription—that is, that women can understand women better than men can just because they are women—he asserts the professional’s claim of knowledge by achieved skills. But then he reverses himself and agrees that downtrodden groups like blacks do have a special perspective on society; because they have been oppressed, they can see things that whites cannot see. They can see oppression, from the inside. Indeed, the very concepts and words of white sociologists hide the social suffering and humiliation of other groups. They objectivize and depersonalize these experiences with concepts like social disorganization that point to an abstract structural problem out there. (Objectification, of course, expresses the researcher’s ethical neutrality). As examples of such “sociological euphemism” Merton cites his own concepts: “Analytically useful concepts such as social stratification, social exchange, reward system, dysfunction, symbolic interaction are altogether bland in the fairly precise sense of being unperturbing, suave, and soothing in effect” (1973, p. 131).

Not many social scientists are able to take the perspective of a critic and see their own shortcomings. Merton’s achievement is rare and impressive.

MERTON’S FOLLOWERS AND CRITICS

Unlike the Popper people, Merton’s followers and his critics are two different sets, and the critics are more in agreement with his post-1961
ideas than the followers are, sometimes. Perhaps Merton has outdistanced his followers. The followers, nearly all students and students of students, have produced quantitative empirical studies that illustrate Merton's earlier ideas. Like good professionals, they have used citation counts, causal modeling, and network analysis because these were the latest scientific techniques, whether or not they especially fit Mertonian functionalism (Ben-David, 1978; Hargens, 1978). With regard to the norms of science, Hagstrom (1965), Crane (1965), Gaston (1970), and Cole and Cole (1967, 1973) have found that for certain samples and fields, the norms of universalism and communism are mostly followed. Deviation is rare (Cole and Cole, 1973, pp. 256–58). Critics found much deviation in other samples. Mulkay (1969) cites the Velikovsky case, in which scientists broke all sorts of Mertonian rules. (But cf. Bauer, 1984, who argues that it was Velikovsky who broke the rules, so thoroughly that scientists were justified in rejecting his claim to be scientific.) The issue, however, is not whether scientists act according to Merton's norms; Merton himself asserted that they often do not, but pointed to expressions of disapproval as evidence that the norms were accepted and enforced as obligations. But they may have been ignoring one norm, and getting disapproval for it, in order to follow an opposite norm, as with Merton's original achievement-versus-humility pair.

Thus the issue is what norms scientists do accept in practice, or believe they accept. Statistical studies of conformity to Merton's 1942 norms do not address this issue, because they fail to inquire about counternorms. Instead, it is necessary to interview or observe scientists closely and individually to see what norms they accept. As Mitroff points out (1974a, pp. 15–18), the problem is that Merton got his 1942 norms from public statements by great scientists from Newton to Freud; but these geniuses may have been providing an idealized or simplified public image of science. Merton's norms and others like them may well exist as ideals, but very different norms-in-use in Kaplan's sense (as discussed in chapter 4) may govern practice. Or a Mertonian norm may coexist uneasily with its opposite, as in Merton's 1961 list.

Blissett (1972), using depth interviews and a mail questionnaire to two samples of physical scientists, found both kinds of normative ambivalence, ideal-practice and norm-counternorm. His respondents accepted universalism in the abstract but particularism in practice; they affirmed that truth claims ought to be judged impersonally, but agreed that the prestige and institutional affiliation of a scientist does affect the acceptance of his work. The statistical studies of Crane (1965) and Cole and Cole (1967) are consistent with Blissett; they found that young scientists at prestigious universities get more publications accepted and other forms of recognition than similar scientists elsewhere. With regard to skepticism and emotional neutrality, Blissett's respondents split about fifty-fifty. Half asserted that scientists ought to be skeptical and detached from their own theories, and half urged commitment and emotional involvement while a theory is being worked out and communicated. Mitroff (1974a) found a great deal of normative ambivalence in his interviews with moon scientists. Some of his questions asked their opinions about Merton-type norms, especially communism, disinterestedness, and skepticism (p. 43). He found other norms asserted in their replies, including counternorms to the original set. In nearly all cases, his respondents asserted the validity of both norm and counternorm. Specifically, many of them, like Blissett's respondents, argued that scientists ought to be emotionally committed to their own theories. They ought to look for supporting evidence, push their ideas in public, repeat them in different forms, sell them. Yet they ought also to respect negative evidence and change or drop a refuted theory. The argument is that true theories and facts are more likely to emerge from a continuing clash of strongly committed partisans than from disinterested observers. The latter may make good judges, but since they don't care which theory is true, they are unlikely to make the sustained effort needed to dig out relevant facts, adapt and develop their theory when it encounters trouble, and get the attention of their busy colleagues. Yet even the partisans ought in principle to be able to accept a strong opposing case. Thus Mitroff's data support Merton's 1963 assertions about the importance of normative ambivalence.

There was much ambivalence about Merton's norm of communism. Many of Mitroff's respondents urged the importance of secrecy to prevent theft; they were obsessed with stealing, both intentional and unintentional. Their rule was, keep your research secret until you are ready to publish, but be sure to publish before the opposition does. The same picture of secrecy, spying, and a frantic race to publish appears in James Watson's Double Helix. Hagstrom (1965) also emphasizes the value of secrecy in a competitive field.

One of Merton's norms is missing: humility. A Newton or a Freud can afford to strike a humble pose in public, because he knows he will get the recognition due him, or if necessary his disciples will get it for him. But
Mitroff’s moon scientists, like Watson and Crick, had to scratch and fight to get their little bit of fame. “The single result that stands out is the intense masculinity of these scientists. And indeed masculinity may be too kind or dignified a word. It is closer to the truth to say that it is their intense, raw, and even brutal aggressiveness that stands out” (Mitroff, 1974a, p. 144). Merton could reply: moon geology and DNA research are both “hot” fields where discoveries are coming in pell-mell. Such fields have always been highly competitive and have attracted the most competitive people. They are not typical (Merton, 1973, chap. 15). Merton’s actual reply (1976, pp. 59–61) so thoroughly misrepresents Mitroff’s statements (1974a, pp. 11–17, 73, 276) that it had best be passed over as a misunderstanding.

But if opposite norms coexist in a community, as Merton proposes, which norm applies in a given situation, or do both apply? Mulkay argues that with opposite norms available, any action could be justified by one norm and criticized by another, and even the same norm could be interpreted differently (1980, pp. 59ff.). Thus norms can be used as arguments in disputes; they divide rather than unite scientists. Mulkay (1979) found just such a situation in Mulkay and Edge’s case study of radio astronomers (Mulkay and Edge, 1973; Mulkay, 1976a); the Cambridge group he studied urged secrecy for themselves, and their competitors urged communism. The competitors called for earlier publication of Cambridge results to allow for wider discussion, replication, and the advance of science; the Cambridge people defended their secrecy as affording them time to check their results, give student researchers recognition, and avoid distortion in the press. The communism-secrecy pair operated just as the competition-protection pair operates in foreign trade policy: competition for others, protection for me. Mulkay cites Gouldner, a Merton student, who observes that “what one conceives to be moral, tends to vary with one’s interests” (Mulkay, 1979, p. 70).

This is just the sort of conflict that Merton called attention to in his discussion of normative ambivalence.

Mulkay and Edge’s case study suggests that institutionalized norms and counternorms do not necessarily unite the scientific community, but provide material for disputes in the race for recognition. If their case is typical, there is no unified scientific community, but only small quarreling networks that form around a theory or problem, put out a series of publications, and then dissolve or merge into other networks. The proper way to study these small networks is microsociological: watch individuals negotiate agreement on their facts and findings, watch them construct evidence, watch them beat down adversaries with the norms of science. The macrosociology of science is pointless because its subject matter is chaos.

According to Mulkay (1975), Merton’s 1942 norms are a prettified picture of science put out by scientists for public consumption. Scientists have indeed announced such norms in public, but they don’t act on them. Merton has made up a “storybook image of science” (Mulkay, 1976b). Randall Collins (1975, pp. 473–79) calls functionalist sociology of science “neocennifer science”—idealized statements of norms that no one follows and empirical research that defends the fairness of scientific organizations. Actually, he says, scientists are very argumentative people. The career goal of a scientist is to get others to read his publications and discuss them. They read others’ publications solely to find ways to get others to pay attention to them. What passes for truth is those ideas that get picked up and repeated, probably those with flashy labels like “Matthew effect.” The proper way to study this endless cacophony is microsociological. Mitroff (1974b) provides a possible solution of this disagreement. He suggests that the norms of communism, emotional neutrality, and disinterestedness apply in a well-established, well-unified community doing “normal” science, while the counternorms of secrecy, emotional involvement, and interestedness apply in new fields or during crisis times. During normal times the problems are well defined, the techniques are established, and the criteria of solution agreed on. In crises or new fields, the problems are ill defined and there is no agreement on facts, concepts, techniques, and solution criteria. Mitroff observes that in his case the arrival of new evidence, moon rocks, permitted the solution of some well-defined problems but not the ill-defined ones. The Mulkay-type microsociologists have studied new fields where there is much confusion, and perhaps here Merton’s macrosociology does not apply. Merton’s own community, the functionalists, was the dominant sociological community from about 1950 to 1970 and was also prominent in political science (later) and anthropology (earlier). Mullins (1973) calls functionalism Standard American Sociology, “faith of our fathers, with us yet.” Consequently, Merton’s experience was in a large community with shared norms. In such a community, people are sure to read each other’s papers, agree with them, praise them, and cite them, so they can afford to be disinterested, humble, communistic, and so on.
EXTERNALIST SOCIOLOGY:
THE SOCIAL LOCATION OF SOCIAL SCIENTISTS

A maximally objective science ... will be one that includes a self-conscious and critical examination of the relationship between the social experience of its creators and the kinds of cognitive structures favored in its inquiry.
——Sandra Harding (1986, p. 250)

Externalist sociology of science begins with the observation that scientists are also members of society. Members means that they locate themselves somewhere, in some social class, status, and roles. Their experience in this location, direct or empathetically shared, inevitably affects their thinking; as Szomtko observes in his eulogistic profile of Merton (1986, p. 38): “The social relations in which a man is involved will somehow be reflected in his ideas.” That is, social scientists are more aware of some problems and developments than others, and they perceive or experience these problems from the perspective of where they are. They empathize with certain people in the problem, or with certain problem solvers, and therefore bring certain interests or values to their diagnoses. Consequently, scientists who locate themselves in different classes or social relations will produce different descriptions, evaluations, and theories. Conversely, one should be able to read back from a theory or disciplinary matrix to the social location that made such a theory possible. The influence of the different social locations on science tends to fragment social science along class, ethnic, or gender lines and thereby weaken the community that Merton regarded as essential to science. His norm of ethical neutrality had the function of weakening external influences and thereby preserving community. However, the counternorm of ethical involvement has the opposite effect of justifying fragmentation along social lines.

The most convenient place to begin examining the social location of social scientists is with the earlier observation of the social location of the Mertonians. The question here is, if the Mertonians are professional sociologists in partnership with government, what kind of science is appropriate to this social location? For the Mertonians, government has overall responsibility for system maintenance and the well-being of society, perhaps assisted by some far-sighted, socially responsible corporations. Sociologists can study some undesirable situation like race prejudice, but it is government's job to manage the situation (Parsons, 1967, chap. 13). Sociologists research and advise, governments act. In other words, Merton's science is a systemic or problem- or policy-oriented science.

A policy-oriented science must look for causes of social or industrial problems, where cause means “some frequently contributing factor which can be affected by policy, and whose change affects the problem.” The politician wants a handle or lever on the problem so he can do something about it, and the scientist's job is to find a lever. The conflict of interest is that the politician wants a handle on this problem, and quick, while the scientist wants to find the variety of possible handles on this sort of problem, and perhaps its connection to other problems. More generally, scientists need not even be studying problems; they can study the causal interrelation of factors that could become involved in some type of problem. In order to understand labeling as a cause of juvenile delinquency, it is necessary to understand the labeling process in its various conditions and circumstances. Nor do scientists have to conclude that this sort of problem has a solution, though scientists who find solutions are more likely to get new research grants, other things being equal.

The problem, or more generally the network of probabilistic causes, must be located out there, not here. The scientist and the politician are not part of the problem, they are part of the solution. Also the problem, or the topic of study, must be limited in time and space, in relation to possible resources for solution. Thus in both the Keynesian and the Parsonian functionalist tradition, the social system or economic system is a national system with a boundary around it, and attention is focused inside the boundary. The Almond-Apter developmental functionalists similarly treat each developing country as a separate unit of study. Studies of international trade will limit themselves to the international economic system and usually focus on one country’s trade.

In short, this kind of science is objective, detached, causal and/or functional, and specialized. The result is the sort of theory that Merton calls “euphemistic”—unemotional, impersonal, preferably quantitative. Also, to express and protect the scientist's professional skill and professional status, some of the concepts and techniques must be technical, just as a doctor's handwriting on a prescription must be illegible except to the pharmacist. For example:
We can write the mathematical expectation of the covariance of consumption and income as:

\[
E \left[ \frac{1}{n-1} \sum_{i=1}^{n} \sum_{k=1}^{K} c_{ik} \gamma_{ik} \right] \\
= E \left[ \frac{1}{n-1} \left( \sum_{i=1}^{n} \sum_{k=1}^{K} (c_{ik} + c_{ik}'\gamma_{ik}) \right) \right] \\
+ \sum_{i=1}^{n} \left( c_{ik} + c_{ik}' \gamma_{ik}' \right) \\
= E \left[ \frac{1}{n-1} \left( \sum_{i=1}^{n} \sum_{k=1}^{K} (c_{ik} + c_{ik}'\gamma_{ik} + c_{ik}\gamma_{ik}') \right) \right] \\
+ \sum_{i=1}^{n} \left( c_{ik} + c_{ik}' \gamma_{ik} + c_{ik}\gamma_{ik}' \right) \\
(\text{Eisner, 1958, p. 987})
\]

In English, the expectation is the same as the expected covariance of permanent and transitory consumption and income in all combinations.

The professional's treatment of the policy maker is quite different. It is not causal, impersonal, quantitative. The policy maker is assumed to be rational, have free will, make decisions on the basis of advice, have political courage in the sense of being willing to take risks that might mean losing the next election, make mistakes that will later regret, even goof up completely and make the problem worse. He is a partner in a dialogue, not an object of quantitative study, prediction, and control.

For example, Gusfield (1976) notes that in drinking driver research the driver is objectivized as a neutral object, the problem. He is neither blamed nor pitied. "The drinking driver stands as an object outside the emotional ambit of the writer and the reader...in constructing him as a neutral object, control is enhanced" (p. 30). Emotional and moral language is reserved for the partner-and-opponent, the politician. Phrases like "patent failure," "pathetic," "look with utter amazement" appear in reference to the politician (pp. 27–30). Here is another function of Merton's norm of value neutrality: it serves to objectivize the problem.

In policy studies, which focus on the policy maker, we get a very different kind of science. It is not objective, detached, and euphemistic. A policy study interprets the policy-making process sympathetically, as the process looks to the policy maker. It describes the policy maker's line of thinking, explains the diagnosis and intended solution, traces the intended and actual implementation, and evaluates the result in terms of the policy maker's goals. It may also evaluate the goals and suggest changes.

We can name these two phases of professional science the Object and the Subject. The Object is out there carrying on. We study its regularities and the probabilistic causes by which those regularities can be influenced; we devise indicators to measure its state or rate of change: unemployment rate, MMPI score, IQ, Gini coefficient. The Subject is here with us, a partner in dialogue. The partner consults, plans, acts; we advise, explain, evaluate, request more money. The two categories are similar to Habermas's "science with an interest in control" and "science with an interest in communication," or hermeneutic science. However, in contrast to Habermas's much more abstract Kantian scheme, here each of the two phases presupposes the other; neither makes any sense without the other. Professional social science is simultaneously controlling for society and hermeneutic for policy making.

What happens if we reverse our location and our line of vision? The Mertonian sociologist looks at a social problem from outside it, detached from it, and indirectly from the perspective of a government agency. What happens if we locate ourselves inside the problem looking out? This is the social location of black or female sociologists, who speak for the downtrodden and oppressed, according to Merton. Many of the symbolic interactionist and structurist studies of the Chicago school fall into the same category. These include Alfred Lindesmith's study of drug addicts, Erving Goffman's study of mental patients and gamblers, Becker and Geer's study of medical students (1960), Suttles's study of slum dwellers (1970), Bruce Jackson's study of prisoners, Rasmussen's study of massage parlors (in J. Douglas, 1976), Ned Polsky's study of hustlers, beats, and others (1967). Anthropological studies of dependent and colonized peoples are also similar, as well as works like Piven and Cloward's Regulating the Poor (1971).

In this sort of science, the scientist becomes part of the "underdog" group through participant observation and tries to understand how these people manage amid deprivation, disrepute, or dependency. He treats them as Subject, constructing a livable world for themselves, or failing and going under in despair. This sort of science is not at all causal, quantitative, detached, objective; it presents people to us as they see
themselves, vividly and directly. Medical students see themselves as "boys in white," in Becker and Geer's phrase (1960), not as Merton's euphemistic The Student-Physician (1957), which objectively locates them out there in the role structure. (Howard Becker pointed out this contrast to me.) Its purpose is not to find causal regularities that can be used to predict and control, but to help us understand and sympathize with these people as human beings, and thereby understand better the human in all of us. It is a hermeneutic science.

Another group at the periphery of Merton's vision is represented by John Kenneth Galbraith the "leftist critic of American society." This group includes mainly economists—the institutionalists—and political scientists—the bureaucratic politics theorists of the 1970s. For these people, government is part of the Object. They describe the dynamics of this Object in technical, objective terms: bureaucratic symbiosis, standard operating procedures (or SOPs), in-and-out, action channels. They show how all people in government including the president are constrained by their position to take certain predictable stands, either partisan or uncommitted, and show how the policy-making process is determined by these organization dynamics. Individuals have a range of freedom or maneuver space, just as they do in functionalist theory, but their basic line of thought and action is determined by their organizational position.

From this perspective, it is naive to think of government as a rational problem solver concerned with keeping the society running. Its "concern" is rather to keep itself running, to keep doing what it does. Kharasch's Third Axiom is: Whatever the internal machinery does is perceived within the institution as the real purpose of the institution (1973, p. 13). "Internal machinery" is defined as whatever the employees do, such as filing documents.

The people at the top levels of government may well see themselves as rational problem solvers, bravely trying to manage one crisis after another in this very complex world, and this self-presentation is accepted by the Mertonians. But the bureaucratic politics people reject this subjective account in favor of detached observation of actual bureaucratic routines. Instead of treating government as a policy initiator, they treat it as the dependent variable, responding to organizational and social pressures in predictable ways. Most of the pressure comes from business—that is, other large organizations in bureaucratic symbiosis with some government agency. The military budget, for example, is a product of organizational dynamics of the weapons firms, who provide employment and campaign contributions to the districts of key congressmen, who in turn vote the funds for the Pentagon, whose bureaucratic need for expansion makes it receptive to the new weapons suggested by the weapons firms. Sometimes the initiative comes from the Pentagon—agency and moves around the cycle from there (Melman, 1970).

The institutionalists do a similar analysis of the corporate sector.

The Subject for the bureaucratic-institutionalists is the people—the consumers and voters and citizens. The scientist is writing for them, not for government. His or her purpose is to show them how they have been fooled by advertising and political propaganda and the symbols of government. And once they realize this he can show them how to influence government and even to counteract corporate influences. The scientists in the consumer-environmental movement are further prepared to act as advisors and spokespersons for the consumers. They can do the technical research on nuclear power safety regulations or waste disposal or food additives, then testify at hearings induced by a consumer organization, then sue the government if necessary to get a regulation enforced. Thus government agencies, like EPA or FTC or OSHA or NRC, become arenas of combat between the people and the corporations, each represented by their own professional scientists (Nelkin, 1984).

For the Merton-type professionals, one of the norms of science forbids carrying scientific controversy outside of science to the people; this is condemned as a low blow, since ordinary people don't understand professional science and can easily be stirred up by simplified and biased stories. Indeed, these "scientists" who go to the people with biased stories must be ideologically motivated and therefore poor scientists. Also, they argue, airing controversies in public weakens the prestige of science. The only proper nonscientific audience is the government agency responsible for dealing with the relevant social problem. But for the environmentalists it is pointless to communicate with government agencies or Merton's enlightened corporations in any language other than lawsuits and threats of lawsuits, since they don't care about problems in society. All they care about is their SOPs. Ordinary people are concerned, since they are the ones affected by the problems; and they are intelligent enough to understand a scientific analysis of pollution or poverty if it is explained in nontechnical language. Indeed, these citizens who can throw off the illusions of political symbolism and corporate advertising (unlike the Mertonian professionals, who get taken in), these Davids, or Lois
Gibbes, who can take a stand against the government and corporations, are pretty brave and even heroic.

The issue here is not whether any of these perspectives on society is the correct one. It is simply that the different locations that scientists take in society give them different angles of vision and therefore produce different kinds of science. Looking at poverty from the outside as a social problem calling for government action produces one kind of theory; looking at poor people from within their midst, as one of them, produces a different kind. Looking at government from the inside as a rational, responsible policy maker produces one kind of political theory; looking at it with detachment as a large organization interacting with other large organizations produces a different kind. Looking at an election campaign from the perspective of individual voters making a public choice produces one kind of theory; looking at it from the perspective of a candidate’s advisers deciding on what image to present produces a different kind. (See Diersing, 1982, for a discussion of different perspectives in social science.)

What induces social scientists to take a particular location in society? Presumably personality factors and education affect individual choices. But the perspectives themselves come into prominence as a result of dramatic changes in society. The 1945 bipolar world that defined the United States and the Soviet Union as permanent opponents brought to prominence the perspective on the USSR as the enemy to be contained and defeated. The sudden appearance of a large number of new world states in the 1950s produced a concern for how these states could be helped to develop politically and economically—a concern that naturally treated the “modernizing elite” in power as the Subject to be advised and helped, while the peoples and institutions were the Object to be managed and modernized.

The liberal professional science of Merton, Keynes, or Almond flourishes in periods of liberal government committed to expanded welfare services, full employment, and foreign aid to modernize the ex-colonial countries. In the United States, this period was about 1933–75 and in Britain 1945–79. It was the period when the economic advisers were established in U.S. councils, when antipoverty and economic development and urban development and foreign aid programs flourished, when research funds and aid to education expanded accordingly. This period, 1933–75 or so, was also the period of a relatively closed U.S. economy, or at least when the internal effects of international competition were not yet manifest, so that problems could be defined within a national or municipal boundary. At such times the liberal idealization of government is plausible, the need for objective problem-oriented research evident, and the research funds available. But when agencies administer on and on, expanding as their associated problems expand, when countercyclical policy gets tangled in stagflation, devaluation, and the political business cycle, when the developing countries are overwhelmed in debt, dictatorship, and dysentery, or when the agencies and research funds are eliminated by a conservative government, liberal science loses some of its aura of practicality and immediacy.

The “underdog” movement grows out of liberal social science as its internal opposite. In the 1950s and 1960s, and also thirty years earlier, liberal science focused its statistical-survey attention on the social problems—the slum dwellers, delinquents, immigrants, criminals, mental patients, addicts, the unemployed. But some sociologists came to see these Objects as Subject, bravely trying to survive in a very difficult world; these scientists reversed their identity and joined their Subject through participant observation. The list includes Saul Alinsky, Clifford Shaw, and William Whyte in the 1930s; Richard Cloward, Herbert Gans, S. M. Miller, and many others in the 1960s. With this reversal also came a reversal of the solution: the underdogs should organize against their oppressor, government, with its slum clearance and highway projects and regulations that produce permanent welfare dependency.

The impetus for the bureaucratic politics movement was the Vietnam War, a grotesque caricature of liberal foreign aid to a developing country. What kind of policy-making process continued this war year after year to no discernible purpose? Schumann (1974) calls it “a bureaucratic war from start to finish,” and Ellsberg writes of the “stalemate machine” (1972). The bureaucratic politics theorists argued that the liberal slogans of responsible government developing and protecting democracy in Vietnam were symbolic politics, a disguise of reality; the reality was bureaucratic routines and presidential image making.

Thus the various perspectives that come to prominence and flourish for two or three decades are reflections of dramatic developments in the world system—to some extent. The 1945 bipolar world that defined the United States and the Soviet Union as permanent opponents; U.S. economic hegemony after 1945 that produced a permanent flow of surplus capital, some of which went into social welfare and research; the dependency of Latin America and South Asia on U.S. capital, with ensuing
revolt and military repression; the necessary export of U.S. capital to strengthen its own competitors in core countries; the resulting opening of the U.S. economy to international competition and the resulting decline in the rate of profit after 1965; the resulting shift away from corporate liberalism by the corporate policy planning groups like the Business Roundtable and the resulting decline in the welfare state and the rise of conservative social scientists; the vastly increased mobility of capital resulting from the postwar need to export surplus U.S. capital, and the resulting decisive weakening of labor; the attack on unions and the defeat of labor in the early 1980s—all were stages in the continuing evolution of the world system which were reflected in social science.

But if the distribution and the changing vitality of social scientific movements reflect changes in society in part, then knowledge also reflects those changes. Knowledge can be lost as well as discovered with changes of perspective, and the growth of knowledge becomes problematic. The pragmatists' belief that science can advance through a reflection on its own methods and a correction of persistent errors assumes scientific autonomy; but if science is socially constrained, the constraints can prevent self-correction.

SCIENCE AND VISION/FANTASY

In heaven, I replied, there is laid up a pattern of it, methinks, which he who desires may behold, and beholding, may take up his abode there. . . . He will live after the manner of that city, having nothing to do with any other.  

—Plato, Republic IX

You don't trust economists. They deal in a make-believe world.  
—Don Sheahan, bond trader, Nikko Securities  
(reported Jan. 16, 1988)

This section deals with what Schumpeter called vision and Mannheim called utopia. When a scientist locates himself somewhere in society, this location gives him a perspective on society; he looks at society from where he is (Subject) to where he is not (Object). Each perspective brings with it a vision, both of self and of not-self. According to Schumpeter (1949), a vision is a conception of a set of phenomena as related and as important, and therefore worth analyzing. A vision sets the task for a research program, the task of analyzing the phenomena, finding their interrelations, and finding the factors that produce or maintain or change them. In addition to this cognitive aspect, a vision also has a valutational aspect: the phenomena ought to be either maintained or changed. The valutational aspect is approximately what the Edinburgh sociologists call interests or concerns (Barnes, 1977, chap. 2, esp. p. 28).

The two contemporary visions that Schumpeter describes are both negative. First, he describes a Keynesian vision of a society stagnating because of chronic underconsumption. The failure to buy enough produces chronic overproduction, which produces unemployment and low capital investment, which produces more unemployment, which produces underconsumption. Economists ought to analyze each of these factors and their causes: the causes of underconsumption, unemployment, low investment. When one has found the causes and the interconnections, one will be in a position to prevent or reduce or counteract underconsumption and stagnation.

The second vision is the neoclassical vision of monopoly as evil. In this case, the cause of monopoly is thought to be obvious: it is caused by the failure to enforce the antitrust laws. Government bungling is the cause of monopoly. (That's what they taught me at Chicago). Research focuses not on the cause, but on the presumed bad effects of monopoly: perhaps higher prices, or lower quantity, or poorer quality, or lack of innovation. Demonstrating one or another of these bad effects will provide justification for enforcing the antitrust laws.

Each of these negative visions has a positive side, and the positive side is what Mannheim called utopia. Keynes's positive vision was of a benevolent, responsible government supplied with good economic advice and accurate economic data, a government that could counteract underconsumption and low investment so as to maintain the economy on a full-employment growth path. The neoclassical positive vision was of a monopoly-free competitive market that could distribute its rewards optimally and fairly.

A positive vision idealizes, and its negative side demonizes. Keynes's benevolent, well-informed, and well-advised government was an aspiration to work toward and a standard for evaluating actual government performance, not a reality. The perfect self-cleansing market devoid of all market failures and market power and externalities was an ideal and a standard, not a reality. Its negative side, government interference and monopoly, describes government as always bungling and monopoly as always inefficient.

Similarly, the anticommunist vision of totalitarianism shows us Stalin's crimes, an ever present thought police, outside agitators stirring up
problems so that we can advise our government how to manage them. The free market vision brings an interest in displaying how the Market works, and also a concern for the various interferences such as the welfare program (Murray, 1984) that ought to be removed. The vision of a self-managing society brings a concern for the dynamics of the world capitalist system, including a search for possible changes that might move us toward socialism and an ecologically sound society.

Schumpeter regarded all four of the above visions as false, except the democracy vision, but he argued that such visions are essential to science. He had his own vision too, of course: the brilliant, creative big businessman, the Henry Ford, who could break through dead routines and galvanize a whole society to new heights of productivity. He asserted that the Keynesian "stagnation" vision and the neoclassical "monopoly" vision had both petered out by 1949, but they had in the meantime been valuable for science. A vision focuses the attention of a school of scientists on a set of phenomena and motivates them to study, search out, analyze. It sensitizes them to empirical traces of the vision, suggests interpretations of seemingly random events, and points to a deeper reality or structure behind appearance.

Thus the stagnation-full employment vision sensitizes one to economic indicators: employment levels and changes, investment, consumption, saving, interest rates, wage rates, price levels and changes. Indicators need to be improved, leads and lags studied, comparative data gathered. The vision suggests connections among the indicators, and these hypotheses can be tested and revised. It suggests new data to be gathered: how investment decisions are made, the effects of temporary unemployment on consumption levels, the actual size of various investment multipliers. Finally, a vision that has been filled out by empirical research can suggest a policy-relevant interpretation of complex phenomena.

A vision is falsified or wears out over time, according to Schumpeter, when the empirical researches it suggests continually run into unexpected and puzzling results, and when its policy-relevant interpretations don't work either. Thus he asserts that the antimonopoly people consistently failed to get solid evidence for the various evils that their models attributed to monopoly. However, in the meantime we have learned a great deal from their research and from Keynesian research, and the few persistent diehards who keep trying may even bring out additional data and correlations.
Perhaps Schumpeter was too optimistic about the possibility of falsifying visions. Both the Keynesian and the neoclassical vision have lived on, with changes, for several decades since 1949. A vision is not a description; it is a desired, or rejected, state of affairs. It is an idealization, so empirical factors that do not fit it can be dismissed as temporary obstacles, or blamed on some envisioned demon. Thus scientists with an anticomunist concern can dismiss apparent political changes in Moscow as superficial, temporary, or even as a scheme to lull the Free World into relaxing its vigilance. And they may be right.

Put differently, actual current events are either obstacles or facilitating conditions for the fuller realization or elimination of a vision. The scientist's interest is in possibility as well as actuality. The Keynesian or Parsonian is interested in imagining possible government policies to reduce unemployment or poverty or racial conflict, and the antimonopolist is interested in possible monopoly side effects of some antipollution regulation or civil rights law. But different and even opposite possibilities can coexist in an actual situation. Thus scientists with different visions can describe the same situation differently and both be correct. What is an obstacle for one can be an encouraging development for another.

For example, Studdert-Kennedy cites two studies of Third World agriculture based on two different visions, each probably correct in its own way (1975, chap. 8). One study by Hamza Alavi is of rural Pakistan and is based on a Marxist vision of workers and peasants rebelling and taking control of their own work. The other, by Clifford Geertz, is a study of rural Indonesia, based on an ecological vision of rural catastrophe through overpopulation, plus a vision of possible technocratic-rational exploitation of scarce resources. Studdert-Kennedy observes that a Marxist like Alavi would interpret Geertz's overcrowded, passive villages as a case of massive alienation, and would look for possibilities of awakening the people through a radical political movement (pp. 193–94). He continues: "But Geertz does not respond to that possibility as a real one; . . . he expects a disaffiliated peasantry to become . . . the victim and accomplice of some form of totalitarian regime" because of the need for comprehensive rational planning to avoid mass starvation (p. 194). For good measure, Studdert-Kennedy mentions two more conflicting treatments of Indonesian agriculture based on different visions: Mary Douglas's vision of Indonesia's possible integration into the emerging world free market, and Balandier's vision of symbolic protest against exploitation. Studdert-Kennedy concludes, "Though an empirical analysis under either of the two perspectives might share a good deal of common ground, at the most crucial level the two interpretations cannot be reconciled, and at this stage the appeal must be to assumptions which cannot be subjected to empirical verification or disproof" (p. 192). In other words, class-based perspectives and their associated visions of possibilities cannot be easily proved or disproved.

Up to here we have considered what a vision contributes to science. Now reverse the picture. Every focusing of attention on something draws attention away from or hides something else. Every sensitization to traces of hidden phenomena can also produce an illusion of nonexistent phenomena. Every interpretation that systematizes a mass of seemingly arbitrary events can also project an imaginary order into them. Or, in general, our own vision sensitizes and guides us to an underlying reality, while other visions produce fantasies and blindness.

For example, the vision of a free market has pushed aside the problem of externalities, has neglected or denied validity to the study of how tastes are formed and changed (as in Homans's statement, "People like the damndest things"), has ignored advertising and addiction, has rejected the empirical study of investment and consumption decisions as irrelevant, has treated market power (except monopoly) as a temporary aberration of no theoretical significance. The concept natural rate of unemployment has brushed aside nonmonetary causes of unemployment as unchangeable and probably unknowable, and the other "natural" rates do the same thing.

Some fantasies are simply exposed in time, leaving the original vision untouched. Steven Possony's 1967 fantasy of a tricontinental people's war that Mao was about to unleash, until U.S. firmness in Vietnam dissuaded him, is now forgotten. Cheryl Payer's 1974 praise of the North Korean development strategy, which avoided the clutches of the IMF and the World Bank, is now exposed as fantasy. Jeane Kirkpatrick's 1987 hallucination of Soviet scheming underlying the January 1987 South Yemen coup was easily exposed by journalists, and Lyndon Larouche's 1976 warning of an imminent World War III was another paranoid fantasy.

Collective fantasies, shared by a whole school or community, are more difficult to give up. Perhaps the persistent negative evidence can still be explained away, or perhaps someone can devise a foolproof version of the theory. Keep trying! Examples are the law of comparative advantage in international trade, still taken seriously despite many fruit-
less attempts to overcome the Leontief paradox, and the related law of one price. Some Shaikh-type Marxists still take the falling rate of profit seriously, long after other Marxists have cited the negative evidence (Hodgson, 1974), have pronounced an obituary on the law (Parijs, 1980), and have pointed out the erroneous assumptions in the mathematics (Roemer, 1981, chaps. 4–6). And these examples are merely derivatives from central visions—the self-regulating market and the perpetual crisis of capitalism. The visions are affected even less by persistent negative evidence.

The worst development is the loss of the distinction between vision/fantasy and reality. When scientists come to believe that their vision is not merely an idealized future goal but the actual deep structure of society right now, further empirical research is unnecessary. Their task is rather to expose this reality for all to see. At this point, science has passed over into propaganda. To assert that the Soviet Union is as of 1983 a classless society, and not merely an approximation or tendency toward, is propaganda. Conversely, to assert that as of 1983 the Soviet Union is a totally Stalinist totalitarian evil empire is propaganda. To assert that the free market works solely by information and persuasion, not power, and to imply that this ideal market actually exists (Lavoie, 1985) is propaganda. Propagandists believe that they are simply revealing the essence of things, abstracted from accidental impurities and embellishments; but this fixed essence has become impervious to empirical research, because any deviations from it can be dismissed as ephemeral accidents. Truly, Lavoie lives in a make-believe world. In his world the market conveys all needed information flawlessly and all planning fails; in the real world there are many different kinds of (imperfect) markets, each calling for, and maintained by, a different kind of planning (Chandler, 1990, and earlier works).

The problem for science is to get the good without the bad, to use a vision as a heuristic guide or framework for research without coming to believe in it as unquestionable reality. The difficulty lies in the valuational component of the vision. The value component is useful insofar as it energizes researchers to search for the empirical workings of the phenomenon, to clarify or revise parts, and to fill in details. But when the interest in the good or the bad is so strong that it demands unquestioning loyalty and action, then science has passed over into propaganda.

So we return by a different route to Merton's (and Mannheim's) requirement of scientific autonomy. Scientists always locate themselves somewhere in society, and use their direct or vicarious experience in that location to guide their research. The experience, idealized as vision, suggests the social concerns, the phenomena and problems that should be studied, and the goals appropriate to those phenomena.

However, effective empirical research requires some detachment from the visions and interests that drive one on, and even some skepticism. Detachment enables one to accept data and results that do not come out as expected, and enables one to follow leads that point toward different explanations and theories. Detachment enables one to remember that the vision is an idealization/demonization, that reality is more complex, that other visions also can capture important possibilities and aspects of reality. Too strong an attachment to one's class position turns one into a propagandist, unable to distinguish between vision/fantasy and deep reality, unable to concede the validity of other visions.

Merton was right.

NOTE

Those social scientists who recognize no vision at all in their research might try a different approach. I suggest that they think about the method they use in their research. Perhaps they locate themselves in science as a practitioner of a method, and their experience with this method might help them understand the visions of other scientists.

Every method has problems, pitfalls, weaknesses. In one's own method, these are experienced as challenges to be overcome. A good experimenter is aware of experimenter effects, biased samples, subjects' interpretations of the experiment, the problem of ecological validity, robustness, construct validity, and so on. During a long schedule of experiments one hopes to solve these problems one by one with proper care, and one looks forward to a time when most of the problems will have been managed and a definitive result achieved. That is vision, though not Schumpeter's version.

In present research, one is always dealing with difficulties; but underlying this process is the ideal method, practiced by a good experimenter, that will eventually produce truth decades from now.

Other methods, however, are regarded from the outside with a more skeptical eye. Those researchers too are dealing with problems, perhaps too routinely or clumsily, but they are not really aware of the more basic weaknesses of their method. A good researcher can probably achieve...
results with any method, but these are ordinary, competent researchers. Experimenter effects and subjects' personality effects, for instance, can never be fully managed, and the attempt to do so produces even worse distortions. Realistically, it's hopeless; definitive results are not possible. That is lack of vision, which we save for others' methods.

IT IS TIME once again to give chaos its due, and as always bring some order to it. The Mertonian scientific community was unified by shared norms and counternorms which enabled scientists to work together and achieve a shared truth. But Merton's critics such as Michael Mulkay and Randall Collins have maintained that these norms are fictions that scientists have invented as part of their struggle for professional prestige and foundation grants. Scientists are not disinterested, not self-skeptical, and certainly not humble, the critics assert; they push their own ideas dogmatically and persistently. They do not judge others' ideas on universalistic criteria, but accept ideas that are similar to their own or useful to them, and criticize, misrepresent, or ignore the rest. In short, they act like Mitroff's brutal egotists, the moon scientists.

What then prevents scientists' communication from degenerating into Agassi's criticism of all against all? How can shared truth emerge out of egotistic strife? Ravez has raised the issue most directly: How does it happen "that out of a personal endeavour which is fallible, subjective, and strictly limited by its context, there emerges knowledge which is certain, objective, and universal"? (1971, p. 71). He devotes part II of his book to this issue. Or, if "certain, objective, and universal" knowledge is too much to hope for in the social sciences, how can we get even plausible, temporary, partial understanding out of egotistic self-display?

The microsociologists find an answer in close-up detailed studies of particular scientific episodes. These studies reveal how the disorder that is always present in scientific work is continuously transformed into order. The order and unity that the Mertonians attributed to an ideal community of all scientists is now found in the minute-by-minute activities of one, two, or three researchers. The techniques these researchers use are not unique to science. Scientists are social beings like the rest of us; they maintain order in their social relations and activities in the same ways that other people do.
Thus the microsociological approach requires us to begin with individual research and move step by step toward actual, not idealized communities and eventually to public knowledge. Perhaps if we move carefully forward a step at a time in this fashion we will get a more secure result than if we try to solve the whole problem at once.

The microsociological researchers divide themselves into a number of groups who differ slightly on method, theoretical background, and theoretical focus of interest. There are the ethnomethodologists—Garfinkel, Lynch, Livingston, Sacks, Woolgar, et al.—deriving from Schutz, Merleau-Ponty, and Heidegger; the constructivists—Knorr, Latour, Sutton, influenced by Bourdieu; the Edinburgh school—Barnes, Bloor, Law, Shapin, MacKenzie, Edge, Dean; the empirical relativist or Bath School—H. M. Collins, Pinch, Travis, Pickering, W. Harvey; and many others not easily classifiable—Mulkay, Restivo, Chubin, Potter, Yearley, Krohn, Whitley, Weingart, Gilbert, et al. Knorr-Cetina and Mulkay (1983), a good survey of the whole field, distinguishes eight research programs including the weak program, the mild program, and discourse analysis in addition to the above. Apparently they are as divided as the science that they study. As for their predecessors, Ravetz, who is often cited, asserts that Kuhn "has, as it were, created the new paradigm which we all follow" (1971, p. 73).

There is disagreement here between the Mertonians and the anti-Mertonians; both claim Kuhn as their own. The Mertonians focus on Kuhn's scientific communities, since these communities are the real source of knowledge; the anti-Mertonians argue that Kuhn provided a scheme by which social factors could influence the content of science, while for Merton the content of science should be independent of social influences (Mulkay, 1980; Barnes, 1982). The microsociological issue of social influences on science, which Edinburgh people such as Barnes and Bloor have emphasized, has been discussed in the previous chapter. Restivo (1983) argues convincingly that the real Kuhn was a Mertonian, while the microsociologists are all anti-Mertonian. (See also Law and French, 1974.)

The microsociologists are unified by a journal, Social Studies of Science, coming out of Edinburgh. They all use variants of the ethnographic or participant-observer method; they watch and listen inside a laboratory or observatory, try their hand at staining slides and mixing chemicals, tape-record and analyze lab discussions, informally interview the researchers afterwards. In textual analysis, they compare successive versions of an article with what they have observed and with interview material. The purpose is to find out what actually happens inside one lab and then watch the results get transformed into journal articles, controversies, and finally public knowledge.

Nearly all these case studies deal with experiments in a laboratory, so their conclusions should apply most directly to experimental psychology and especially to animal experiments. We can also consult our own experience to see how relevant they are to other social science methods.

THE RESEARCH PROCESS

We begin with the ethnomethodologists, the most meticulous and detailed of all the laboratory observers. The stated aim of the ethnomethodologists is to rediscover the problem of social order in the details of scientific practice (Lynch et al., 1983, p. 205). Disorder breaks out constantly; as Knorr-Cetina observes, "A day in the laboratory will usually suffice to impress upon the observer a sense of the disorder within which scientists operate, and a month in the lab will confirm that most laboratory work is concerned with counteracting and remedying this disorder (Knorr-Cetina and Mulkay, 1983, p. 123). Lab rats get rambunctious and refuse to let themselves be operated on properly; or one will emerge from its box at the start of a trial, yawn, scratch itself, and amble back in again when it is supposed to be running a maze in record time. A human subject will request time off to light a cigarette. Materials are not in uniform quality; some equipment is not the most appropriate, but it will have to do; the tissue slide is not cut or stained properly to show the relevant cells clearly, the telescope must be properly adjusted to get the signal (Garfinkel et al., 1981). The researcher must constantly clear a path through these circumstances. "Actual scientific practice entails the confrontation and negation of utter confusion" (Latour and Woolgar, 1979, p. 36). Norms and counternorms are irrelevant to this problem (Lynch, 1985, p. xiv). One technique is to make on-the-spot decisions: will this count as a trial, will this botched slide count as one instance, would it be fair to give that lazy rat a little nudge? (Notice the norm there.) Another technique is to negotiate agreement; if one researcher sees something and the other doesn't, the first can offer the compromise statement that he might have seen something resembling it. Thus he saw
the resemblance and the other saw the contrast. Or one can negotiate the loan of better equipment, by rearranging the research so as to interest the possessor of the equipment.

The order that the researcher produces is mainly a temporal order (Lynch, 1985, chap. 3). This is Heidegger’s “temporalization” (pp. 53, 76). The parts of a process must go in a certain sequence: the tissue section must be cut first, then stained, then mounted. Several sequences can be fit together, so a waiting time in sequence 1 can be filled with a step from sequence 2. Or a sequence can be interrupted to deal with a malfunctioning apparatus. This temporal order reappears as a spatial order on the lab table: pieces of equipment are arranged in groups according to their place in the sequence, and often used tools are set up where they will be handy. These sequences and locations are then repaired and adjusted when disorder breaks in. For example, Schrecker reports that when a sequence moved too fast for him he almost knocked over a flask and then set it in the wrong location, thus producing disorder; as a result, his hands “were engaged in a territory whose spatial arrangements did not adequately exhibit the sequential organization any longer” and he KNOCKED OVER THE FLASK. Then he reestablished order by picking up the flask again (Lynch et al., 1983, pp. 228–29). As Lynch et al. comment, “Much of what evidently makes up the orderliness of scientific activities is not worth talking about” (p. 208).

We can divide this ethnomethodological order into two parts. One is the sequences and spatial arrangements as they should properly or ideally be, apart from accidents. The other is the actual constructed order that corrects accidents and breakdowns so as to reestablish some approximation to the ideal order. The second order derives from the first (Bourdieu, 1977, p. 3). Thus the two kinds of (negative) disorder that Lynch distinguishes—which he calls “oops!” and “what went wrong” (1985, pp. 115–18)—exist only as negations of the ideal or proper order.

The ethnomethodologist observes the production of the second order, the constructed order, but the first is not observable by his method. Short sequences can be understood internally; the slide must be stained and mounted before it can be photographed. But the photographed slide is a step in a larger sequence; where does this order come from? Lynch notes that there seems to be some overall research design, though he could not find it himself. He vaguely speculates that the lab director might know what is going on, but the researchers themselves are only carrying out

orders (1985, pp. 63–64). The first order, the overall research sequence, thus comes from outside in the form of orders. As Knorr-Cetina concludes, “The scientists’ laboratory selections constantly refer us to a contextuality beyond the immediate site of the action” (1981, p. 81).

The first order, the research design, can also be discovered by participant observation. However, one does not discover it by watching; that yields the second or constructed order. One discovers the research design by listening and asking questions—of the research director, not the assistants who carry out orders. This was in fact my objective (Diesing, 1971); I was looking for actual research designs. I listened, asked questions, read field diaries and published accounts of “first days in the field.” After listening, one can watch, but what one sees is different from what the ethnomethodologist sees. The latter, looking into a cage in the lab, sees a bit of water and perhaps spilled water; I see a reinforcer.

The actual research design is different from the official research design published in a later article or book, and also different from the actual research sequence, the second order.

From several research designs we can induce rules of method by noticing what works and what doesn’t work. These rules are heuristics, advice on how to proceed to get results. They belong to Kaplan’s “logic in use” and Simon’s “logic of discovery” (1977). For instance, in mathematical modeling one rule is: to model a complex situation, locate the fundamental relation in it, model the simplest version of that, and then add the complications one by one. This does not always work; so there is a second rule: take a few standard models off your shelf and try to see the situation as one of these models. Next focus on the difference between model and situation and try to fit it into a variant of the model. If this does not work, then, as a third rule, try to break the situation into parts and model one part . . .

Experienced researchers have learned a number of such rules or heuristics and include them in their research design. The problem of order is to apply these rules to particular research situations to produce actual research. This is what the constructivists study (among other things). For example, the researchers in Latour’s lab accidentally discovered a brain hormone, which they nicknamed “somatostatin” (Latour, 1981a). The problem then was to construct analogs to this hormone that would be more effective. An analog is a chemical with the same structure as somatostatin except that one or more components are different.

The first step in the research design was necessarily to determine the
structure of somatostatin. Since different researchers normally interpret lab happenings differently (and since variants of the hormone may have had different structures), this was a matter of negotiating agreement (Latour, 1981a, p. 54). The structure they compromised on had $2.6 \times 10^{22}$ possible analogs. Search rules, heuristics, were needed to guide the search through this enormous space to find the few best analogs. Latour studied the search process after it had gone on for five years and produced 286 analogs. How were these 286 selected for manufacture and testing?

Latour induced at least four rules: (1) Delete each of the fourteen original components, one at a time. (2) Replace each component, one at a time, with alanine, a promising amino acid. (3) Ditto for tyrosine. (4) Replace each component with a right-handed version. These rules were parts of the first order, the ideal search process. But they could not be applied systematically because of the disorder that had to be accommodated. “When you get closer to the research process, the multiplicity and the chaos increase” (p. 58).

One accident was the attempt of some researchers to try this brain hormone on the pancreas. A new diabetes drug would be worth millions, so the pharmaceutical industry moved in on the research, and the criterion of effectiveness was revised. The main researcher happened to receive a supply of an amino acid and tried it because it was handy. Another researcher suggested that the agreed-on structure of somatostatin was wrong and was missing one part. Still another researcher found that results with one analog suggested new possibilities. Here we are not dealing with “Oops!” disorder, but with sudden opportunities, insights, and intuitions that ought to be incorporated into the search process. The outcome, the second order or actual research process, combines these emergents with the rules in an unpredictable though fairly rational sequence. “To understand the research process one has to look exactly in the middle of order and disorder” (p. 61).

Latour calls the research process “bricoleur.” Knorr, in a similar case study, writes of “tinkering” and “making things work” (Knorr, 1977; Knorr-Cetina, 1981, chaps. 1–2). Latour also cites Heidegger’s saying “Gedanke ist Handwerk”—scientific thinking is a handicraft (Latour and Woolgar, 1979, p. 171). It is a form of practical reasoning, not of pure logic. Bourdieu calls it “regulated improvisation” (1977, chap. 2).

I believe a similar process occurs in social science research. For example, in our crisis bargaining research, part of Glenn Snyder’s ideal research design was to make it interdisciplinary. (See Snyder and Diesing, 1977.) So he set up a seminar in 1964 and invited about fifteen people from all sorts of disciplines. After two years, four senior faculty from four fields remained, so he “made it work” with us. Later, two junior researchers quit because they refused to accept a military research grant (Project Themis); we managed, sadly, without them. One later returned; he found National Science Foundation money acceptable. A basic rule of historical method is to study the first-hand reports of all major participants in a conflict, to get a well-rounded view, and this requires facility in several languages. For instance, one cannot understand the 1958 Lebanon crisis adequately without reading Camille Chamoun’s memoirs, in French. I read some atrociously one-sided case histories that were based on only English-language sources. But Snyder had to make do with the available language skills, and assigned the cases accordingly.

Where do the rules of scientific method come from? When a researcher is constructing an ideal research design, where does one get the parts? Some of the rules, such as: Try alanine at each location, or Try a game model on a conflict situation, come from experienced success. One could say they are deducted from a general rule: If it works, use it again. Other rules, such as the historian’s rule: In studying a crisis read everyone’s memoirs but don’t believe any of them, come from experienced failures, like the atrocious propaganda pervading the international politics literature. Ravetz (1971, pt. II) focuses on the latter kind of rule, the kind based on past failures. He argues that the craft of scientific research consists chiefly of knowing the pitfalls that await a research project and designing the research to avoid them. In other words, the first or ideal order is constructed out of a foreknowledge of the disorder that lies in wait. Instead of puzzling over “what went wrong” accidents, as the ethnomethodologists’ subjects do, one builds devices to forestall them. But since no one can foresee all the pitfalls and opportunities that come up, tinkering is still necessary. The second-order or actual research process accommodates the planned research design to whatever comes up.

So far we have moved two steps from the immediate, phenomenologically constructed second order of the ethnomethodologists—adjusting telescopes, picking up flasks—toward the goal of objective knowledge. (1) We have moved from the second, constructed order to the first order of a research design which incorporates heuristic rules of method, and a craft or Handwerk which adjusts these rules to the circumstances of the research situation. The rules tell how to work effectively with certain types of materials or problems, and how to avoid common pitfalls (for
example, T. Barber, 1976). The craft is a practice of diagnosing specific pitfalls and opportunities. (2) Both rules and craft are derived from previous research experience and transmitted by apprenticeship, informal conversation, and the writings of methodologists. Thus they transcend the immediate research situation and draw in a collective experience, a social practice.

The product of research is still private, located in the lab or the research group. The next step therefore must be to make the private results public by publishing them. (The following account is based mainly on Knorr-Cetina, 1981, chap. 5; Gilbert and Mulkay, 1981; and Latour and Woolgar, 1979.)

The constructivists and discourse analysts emphasize that there is always a difference between the actual, private research process and the published account. The goal of the private process is to reach agreement in the research group; the goal of the public process is to persuade some selected scientific community. Consequently, the research results must be recontextualized, taken out of the private context and transformed to fit the public context.

The published paper must begin where the intended readers presumably are, that is, with the existing theory and data that they accept. It can then either build on what is accepted, or criticize and undermine it. In either case, the research problem must be located in existing theory, not in the actual research. Since the results, the solution, are already available, a problem can be selected or manufactured from the literature to fit the available results. There must be abundant citations from the literature to connect the paper to what is already accepted, and to show solid scholarship.

The style of the paper must be persuasive. It must use impersonal language and proceed methodically step by step, as though anyone could easily replicate the results. The method is idealized as standard procedures and automatic results. In other words, the actual process is reversed in the paper: actually the results are constructed, crafted, by tinkering with available materials, but in the paper the results inexorably come straight from Nature as the researcher clears a path and then passively stands aside.

Here as elsewhere the constructions of the constructivist laboratory observers come closest to the practice in experimental psychology. In anthropology, for instance in Latour’s lab ethnography, an impersonal-replicability style would make no sense. Instead, the accounts are necessarily personal but matter-of-fact, listing step by step what “I” did, as though things actually happened in that inexorable, purposeful, methodical order. The clinical paper has a different style again; it omits the analyst’s misinterpretations and fumbling and mentions mainly the key interpretations that led to a flood of insight.

In the theoretical section, the rhetoric guides the reader into the proper attitude toward a theoretical assertion. "A long held assumption" or "People have believed" signals that a theory is about to be demolished; “Experimental results show” announces the Voice of Nature revealing the truth. “A few sociologists have come to see” or “There has been a growing recognition” (Gilbert and Mulkay, 1981, p. 269) announces a new truth that the article will build on.

To illustrate the results persuasively, the writer selects the best slides or the best quantitative tables. Or, if people are involved, one selects especially apt quotations, as in the present work, the lab ethnographers’ reports, or the clinical case history. Here as elsewhere the ethnmethodologists’ quotations are the most meticulous phonetically and temporally, with silences measured to 0.1 sec.; but also selected for persuasiveness. If the best tables and charts are not so good, one can explain away the difference from one’s conclusions, citing interfering factors and the crudeness of the instruments. Or one can tinker, trying various leads and lags or correcting for presumed sampling error. In short, as Latour and Woolgar (1979) assert, the purpose of the research paper is to persuade the readers that no persuasion is occurring.

Finally, the writer must anticipate possible objections and answer or deflect them. The writer imagines specific critics reading the paper, locates points they could attack, and supports the argument at these points with a data table, quotation from some authority, or at least a citation that will deflect the critic’s attention.

Do these tactics work? No. They serve rather as an entrance ticket to the public arena; a paper that did not use them would be rejected by editors and referees as poorly written. Once the paper is published and enters the public arena, different mechanisms come into operation to determine the fate of the researchers’ constructions.

THE PUBLIC ARENA

The rationality of the public arena is midway between the unreflective order of everyday research—arranging things on a lab table, scheduling
interview appointments—and the objective, public knowledge we seek. Consequently, we should expect to find a complex, many-sided process, subjective in some ways and more objective in others. The microsociologists have focused on one aspect of this process, the interpersonal or social exchange aspect. But this focus does not deny the existence of others: the political aspect (discussed in chapter 8), the cognitive aspect (chapter 9), and the personal aspect (chapter 10). One must understand all of these aspects to understand its complex rationality. A quick and simple characterization, perhaps in terms of information theory or cybernetics, such as a mathematical modeler or a mathematical thinker would want to make, is bound to be one-sided and inadequate. Nor is it enough to list a series of factors, social, cognitive, political, that influence public discussion, as though one or two of these factors produces a rational outcome and the others interfere. Rather, it is necessary to see the process as social, to see it as cognitive, as political, and so forth, to understand it. It is all of them simultaneously.

The first step in dealing with a publication that enters the public arena ought to be hermeneutic. In order to deal with a new work of science, the reader must first understand it. This involves interpreting it within the author’s tradition, not the reader’s tradition. One discovers the author’s tradition by studying positive citations, references to accepted theory (accepted in a tradition), references to erroneous or outmoded theories (other traditions), and biographical data. If the tradition is different from one’s own, one must move into it by reading all the major works from the beginning on, and if possible by talking to members of the tradition to see how they think. Then one has a context for interpretation. In some cases it may also be helpful to locate a personal context, the author’s personality, so that some passages or the whole structure can be interpreted as moves in a personal process. Consider, for example, the stiff supercautious writing style, full of distinctions and qualifications and classifications and boundaries, of Talcott Parsons’s writings around 1950 or so, which loosened up in his later works. That’s part of his personal kind of functionalism, a part that produced many remarkable boundary and classification-type insights when he got it under control. In practice, social scientists normally omit the hermeneutic process. If the work is written in their own tradition, they understand it and relate it to their own thinking and research. If it is written in a different tradition, they impose their categories and beliefs on it, and misunderstand it. Then they evaluate it as false.

In the case of literature reviews, reviewers will sometimes select experimental reports to fit their own categories, and then simplify and interpret them to fit their theories (Berkowitz, 1971).

The following account of the public arena is based on Bourdieu’s fundamental article “The Social Conditions of the Progress of Reason” (1975). See also Bourdieu (1977); Latour and Woolgar (1979), which builds on Bourdieu; and writings of the Edinburgh School such as Pickering (1981).

In Mertonian sociology, the main social process in the public arena is the exchange of recognition for original achievement. Scientists who build on and extend accepted knowledge, or who correct an error or resolve some puzzle, ought to be recognized for their contribution to knowledge. Those who provide more evidence for what is already known ought to be recognized for achievement but not originality. Original error ought to receive no recognition.

Who would most readily evaluate an article as an original achievement? Someone who understands it and who shares its assumptions and categories (accepted knowledge)—that is, someone working in the same tradition. In a Mertonian context, the primary obligation to recognize falls on the elite of the community, since they have the responsibility to maintain the community in working order. But the elite are precisely the people whose theorizing constitutes the accepted knowledge of the community. Consequently, the article they recognize recognizes them by building on their work and extending its applicability. It is an exchange of recognition, a gift exchange. When the elite say, “Well done, thou good and faithful servant” (Matthew 25:14–29), they do so because the servant has brought back a good profit for them on the capital they have provided.

If we aggregate these gift exchanges within a community, we see that since the elite are always one partner in the exchange, they get the most recognition. Their ideas are being used, extended, applied, supported, and their works are cited. This is the Matthew effect—“Unto him who hath shall be given”—though not Merton’s version of it.

Other members of the community are also likely to recognize the work, in part because they understand it and agree with its “accepted knowledge.” Their public recognition is also part of a gift exchange. They give recognition and receive ideas they can work with and data they can cite in support of their work. But insofar as ordinary members of a community are competitors for group recognition, they can be expected
to be more qualified in their recognition, accepting part and criticising part (for example, Knorr-Cetina, 1981, chap. 4; Latour, 1982). Their recognition says to the community, "recognize her (and thereby my work which she builds on), but recognize me too."

The simplest form of recognition is the citation, which says, "This publication exists." A positive citation says, in addition, "and is worth reading." A few words of praise are better, and building on the work is still better. The recipient of such a gift incurs an obligation, unless the recipient is a member of the elite. In that case, it is normal homage, and can be acknowledged with a plain citation. However, the elite can also bestow recommendations for jobs, research grants, and editorial acceptance. Rejection of a work as false is most likely to occur when the reader misunderstands it because it is in a different tradition. This concept is the hermeneutic version of Kuhn's "incommensurability." Misunderstanding consists of forcing one's own categories and problems onto the work, and then finding that (1) its reasoning is confused, (2) it evades the important problems, and (3) it repeats old errors. For example, analytic philosophers normally misinterpret the microsociologists as relativists (for instance, Freudenthal, 1984). Relativism in their categories is an absurd position, illogical, an ancient error; it means that no knowledge is possible at all. This is not what relativism means for the Bath School (see, for example, H. Collins, in Knorr-Cetina and Mulkay, 1983). Next, one of the nagging problems of the analytic philosophers is that since some facts are more or less theory-laden, how are the conclusive empirical tests essential to a real science possible? The microsociologists do not answer this question because it is not a problem for them; hence their work evades the real issues for the analytic philosophers. Another such problem is: How can rationality, the domain of philosophers, be separated out from irrational factors, the domain of mere sociologists? Again, the sociological works evade this important issue because their concept social is different from that of the philosophers. I mean to pick on the analytic philosophers, but I could cite much of the literature of the social sciences as well.

Next, a reader who has worked through the hermeneutic process and come to understand a work in a different tradition may still reject it. Consider the previous example of the relation between rational and social processes. Analytic philosophers such as Laudan (1981) argue that social factors can properly be cited in explaining errors of reasoning or research, but are irrelevant for explaining correct reasoning and research design. If a writer adds 2 and 2 and gets 4, we do not need a social explanation; we merely observe that the writer knows how to add, knows how to reason. The sociologist who understands this distinction will reject it because it assigns the lower realm of error and irrationality, the realm of bad science, to sociology and reserves the realm of good science for philosophers. Thus Knorr-Cetina (1988) argues that social factors are not interferences, but instruments of knowledge. The experiment's sensitivity to indicators is learned; the ethnographer's use of talk to probe and interpret overt, visible happenings is learned; and so on. Conversely, the rare analytic philosopher who understood the microsociologists' argument that reasoning in the public arena is a social process would still reject it, because it leaves little or nothing for analytic philosophers to do. This argument in its sociological aspect is a struggle over turf, analogous to the Berber families' struggle for control of agricultural land (Bourdieu, 1977). In addition, it is a struggle over social status, since people who study the logic of good or real science are of a higher status than those who study the bad stuff.

However, not all problem formulations, distinctions, and definitions within a tradition can be understood as a claim for exclusive jurisdiction. Some scientists call for joint jurisdiction by several disciplines or traditions, and these formulations would be accepted by members of the included traditions. For example, the Katona-type empirical studies of consumer behavior stake a claim for survey researchers in this area, without disputing the legitimacy of economists' theoretical work on the consumption function. The two disciplines can collaborate.

Finally, competitors within a community might reject part of a publication while citing and agreeing with most of it, for reasons given above. Rejection tactics vary according to whether the publication has been understood or not. If it is misunderstood, the simplest form of rejection is to mistranslate the argument into one's own categories and thus make it absurd. Or one can omit the argument and call names—for instance, calling the hermeneutic philosophers "hermeneuts." Or one can exaggerate or simplify the argument, driving it to an extreme, to "show where it logically leads" or "make it more interesting" (Newton-Smith, 1981) before refuting it. Or one can pick out a weak spot, a careless detail, and exaggerate its importance as evidence that the whole work ought to be rejected. Or one can point to questions it does not answer, questions that are important in one's own theory but not in the publication.

If one understands the publication, one can reject it by "decon-
structing" it, revealing the private research process, the decisions and assumptions, behind it. Some traces of the private process can be dug out of the publication: the footnotes listing the assumptions behind particular numbers in the table, the selection of the sample, the construction of an index, the reasoning justifying the use of some indicator to measure a depth variable, the use of one definition of probability rather than another, the absence of reference to some French or German memoir or history and the use instead of a secondary source, the list of interviewees and evidence of gullibility toward one interviewee, the degree of aggregation of the data which obscures some effects and brings out others, reasons for discarding certain data which, coincidentally, would have eliminated the discovered correlation (Landsberger, 1970), a laconic "For reasons unnecessary to relate, the selection was limited to . . . " (West, 1945, p. vii). Then in each case one can suggest an alternative process of tinkering that would yield a different result. A different indicator, a different interview, a different measure of consumer income, different questionnaire items, a different lead or lag, will always give a somewhat different result. Indeed, one can carry out the alternative process in one's own experiment or mathematical model. Other traces of the private process can be found by interviews with participants, or decades later in memoirs and letters. For example the letters of Freud and Keynes have recently been used to deconstruct publications by these scientists.

This is the process that is systematized in Murray Levine's "adversary model" (1974). The rational function of deconstruction is to test the published data for external validity. Only data that survive cross-examination in the public arena ought to be accepted as valid.

The point that the constructivists emphasize is that all scientific results are constructed, not just the false ones. Consequently, all can be deconstructed. Some decisions and selections and assumptions and adjustments must always be made. This does not mean that deconstructions are always valid when they move a publication from the "objective" to the "subjective" category, merely that they are always possible.

Scientists tend to use a double standard in the public arena. They describe their own work in terms of an idealized method to hide their construction; but they deconstruct opponents' work. The elite in a community maintain agreement by accepting insiders' work as instances of idealized method, but they deconstruct dissenting works, pointing to the difference between actual and ideal methods and calling for conclusive evidence and complete controls (Blissett, 1972, p. 117).

The worst fate a publication can suffer is to be ignored. This happens when there is no community that can use or build on its ideas and data, and also no community whose turf is threatened by it. Such a work exists in an empty space; it neither comes out of a current research tradition nor leads into an enrichment of some tradition. Who now has heard of Scudder Klyce's Universe! This was a large, scholarly systematization of existing knowledge, as of the 1930s, but it did not relate to the research interests of any active group of scientists, so no one paid any attention to it. Similarly Ludwik Fleck's 1935 book, Genesis and Development of a Scientific Fact, remained unread and unknown until the late 1970s, when the constructivists suddenly developed similar ideas and started citing Fleck as supporting evidence (1979, intro.).

Given these characteristics of the public arena, or in Bourdieu's terms the scientific field (1975), what strategies are available to the young scientist starting a career? Bourdieu distinguishes two, a succession strategy and a subversion strategy.

A succession strategy consists of being a "good and faithful servant" in some existing community. The simplest way is to become a student of some community leader; or one can join by paying homage to a leader, using his theories, and building on them. This strategy guarantees readers for one's writings. It also is likely to get recognition and recommendations from the leader and qualified recognition from other followers. The main payoff goes to the leader, whose theory is being developed; but the follower accumulates citations, gets included in conferences and edited volumes, gets recommended for research grants or included in research projects, gets invited onto convention programs. That way one can quickly make a name for oneself within the community, or in Bourdieu's terms accumulate symbolic capital. Soon other people will discuss and build on one's ideas, and if one can compete effectively with other followers, one gets promoted to the position of close associate of the leader, part of the community elite. Ideally, as the leadership ages, one can succeed the old leadership, if the community still exists.

A subversion strategy consists of criticizing the paradigm of some community and setting up a new paradigm (in Merton's sense of paradigm). This strategy is risky because it almost guarantees rejection at best and the limbo of noncitation at worst. Consequently, most young scientists will avoid it. For example, Bill Harvey (1981, pp. 148ff.) describes how a student, Holt, accidentally got an experimental result that flatly contradicted some laws of quantum mechanics. He could have declared
that he had disproved those laws, but he would promptly have been buried under an avalanche of rejection, insult, and misrepresentations and then forgotten. So Holt took the safer tactic of declaring publicly that his experimental result was erroneous. He then tried for two years to find the error, hopefully following up all the critical objections his teachers could invent. He failed; the result was consistently replicated. However, his efforts earned him the forgiveness of the community leaders. Holt had had bad luck, they concluded; his experimental results were consistently wrong, but it wasn’t his fault. He was a good experimenter.

That one didn’t work according to Levine’s model.

A subversion strategy, Kuhn’s “revolution,” is more promising if there is a small group willing to pursue it together. Then they can announce a new paradigm, cite each other’s writings, set up conferences published as edited volumes, and (they hope) make a big enough splash to attract disciples. If they succeed, they immediately become the community elite, accumulating symbolic capital rapidly in a stream of publications and citations and appointments and research grants. The microsociologists are an example of a group pursuing a successful subversion strategy. Their early articles are full of bold pronouncements that something new is happening, something important is being studied for the first time, a start is being made, science is now advancing.

Two circumstances, external and internal, provide the social conditions for a successful subversion strategy. Externally, political circumstances may suddenly produce an age cohort of disaffected young social scientists. Consider New Left Marxism, for example. The few American Marxists of the 1950s—Sweezy, Baran, Marcuse, Aptheker, Genovese—were ignored or scolded; Sweezy and Aptheker had no university appointment. After 1965 the civil rights movement, the student movement, and the vicious, evil, lying, murderous U.S. foreign policy produced a large number of graduate students who rejected existing conceptual frameworks and looked for a theory that would explain U.S. foreign policy; hence, Marxism.

Internally, an existing community may get so large and successful that there is little recognition left for newcomers. Nor is there prospect of long-run rewards. The succession is already decided, and indeed the successors have already established several variants with their own subcommunities, as in the many varieties of functionalism in the 1960s. A few of the new age cohort may establish close relations with one of these successors. The rest face a bleak future in the functionalist or symbolic interactionist community; hence, microsociology.

Bourdieu (1977) mentions a third strategy, an insult strategy. An insult is a challenge to a duel, and the winner exhibits prowess and gains recognition. If the challenge is thrown down to a powerful leader, even a draw or small loss exhibits one’s prowess and gains one recognition. Examples are all the criticisms of Talcott Parsons and Herbert Blumer in the 1970s, beginning with Gouldner (1970) and his “Parsonian” epithet. A book criticizing one of these two was certain to get journal reviews, whether the reviews were critical or not is irrelevant. Nor were Herbert Blumer’s complaints that he had been thoroughly misrepresented relevant; truth was not at issue here, but recognition of critical ability.

An insult strategy is most effective if it comes from within one community and criticizes a leader of a different community. The critic gets recognition both for critical ability and for upholding the accepted truths of the community. Thus it is a variant of the succession strategy. A safer but lower payoff tactic is to criticize an ordinary member of a different community. Such challenges must be accepted, and the result is a dispute. Disputes are normally inconclusive; they begin and end in disagreement.

These three strategies are the ones open to new entrants, though established scientists such as Gouldner can also adopt an occasional insult tactic. The elite are in a different strategic position. They already have all the symbolic capital they need: anything they write will get published, read, discussed, cited, used; they can have research grants almost for the asking; a hint will bring them offers of university appointments. Their task is rather to maintain harmony among their followers, arrange for an orderly succession, and promote alliances with other communities; that way their works will continue to be influential in ever widening circles. All of Merton’s system-maintenance tasks become rational self-promotion strategies for Bourdieu (1975).

In particular, established leaders do not have to respond to challenges, or can turn them aside with a nod of friendly recognition. They have no need to demonstrate prowess, and it might even be undignified to respond to an unknown challenger. Their followers can take care of him. It is also rational to welcome a bold new paradigm and offer an alliance with it. Consider for example Bernard Barber’s 1982 review of Barnes’s T. S. Kuhn and Social Science. Barber is a student and longtime
close associate of Merton; Barnes of the Edinburgh School is one of Merton’s earliest critics. His book continues Barnes’s attempt, begun in 1970, to appropriate Kuhn’s enormous prestige for the anti-Merton forces. Barber’s review is most friendly and generous. He heaps praise on Barnes from all sides, then chides him gently for misrepresenting functionalism, and asserts that functionalists mainly agree with Barnes. He then asserts that functionalists have always been constructivists, but have been unable to do lab ethnography and the like because of ignorance and a lack of expertise; he welcomes the Edinburgh studies. After two very subdued criticisms of Barnes, he concludes with a call to “give up prejudice, stereotypes, hostility, and defensiveness.” This call for an alliance with the microsociologists, a call Merton also has issued, is rational; but the microsociologists’ prejudice, stereotypes, and hostility was also rational in 1975–82. They had to get established, accumulate symbolic capital, before they could make peace. In political science, David Easton issued a similar welcome to the New Left political scientists in his 1968 presidential address before the American Political Science Association.

Do social scientists really have to follow one of these rather sordid strategies? Can’t they just quietly search for truth? Bourdieu’s answer would be that a few fortunate individuals may have different strategies available, but most new social scientists are compelled by market forces to pick a succession or subversion strategy.

First, to be able to search for truth one must at least have a university or research appointment, since searching for truth takes time. But to get tenure one must publish, and to publish one must do research—experiments or surveys or field work or case studies. But such research usually requires a research grant, and the bigger the grant the bigger the project and the subsequent publications. But to get a grant one needs previous publications, that is, previous research . . . .

Latour and Woolgar call this cycle the investment cycle; publication requires research, which requires a research grant, which requires publication (1979, chap. 5, esp. p. 201).

The new social scientist cannot break into this cycle immediately; one must first borrow some working capital. One can do this either by getting recommendations from a teacher who is a community leader or subleader (succession strategy) or by attracting attention with proclamations of a new paradigm (subversion strategy). The working capital must rapidly be transformed into publications. These in turn can be the basis for a small individual grant leading to a few more articles. Such articles can be the basis for a bigger grant involving hired researchers or his own graduate students and leading to a book or two, and perhaps a successor grant. At each step one’s symbolic capital, one’s recognition, is larger, and at each step one must invest this capital in new research, and so on endlessly.

Latour and Woolgar contrast their investment cycle with the Mertonian gift exchange, recognition for original achievement, or actually recognition for recognition. The gift exchange, a precapitalist process, is external to research. It provides a presumed external motivation to publish, but does not affect the research process itself. Latour and Woolgar’s scientists don’t need gifts; they need jobs and research grants. They are caught in the investment cycle, which forces them to publish and publish, and which determines the direction of their research: their research is directed to getting more grants.

Why should new social scientists let themselves get caught in this rat race? They are forced to do so by the market, that is, pressure from competitors. There are always others who could get included in a research project or who could get hired. There are always others applying for the research grants, and most do get turned down. Nor are the competitors equal; some are well-established, well-known scientists who, by the Matthew effect, are more likely to get the available grants. Those who get turned down for grants and tenure will not have much time or resources to search for truth.

In short, the search for truth must accumulate ever more symbolic capital and reinvest it, or fail and disappear. Accumulate! Accumulate! That is Moses and the prophets!

The pressure to accumulate has actually increased since 1945, first because of the increasing number of social science researchers competing for recognition and research funds, and second because of the increase in available research funds. According to Ravez (1971), the increasing competitive pressure has gradually transformed science into what he calls “industrialized science.” Ravez was describing the natural sciences, but some of his observations may apply also to the social sciences, in lesser degree.

1. The rate of scientific discovery and of obsolescence has been speeded up. With several competitors working in the same area, the first to submit a good research proposal or journal article will get the recognition, and the others will have to supply the recognition by citing and building on the winner’s work. A good research proposal, in turn, must
be up to date in its citations; and if it can cite unpublished reports of others' work in progress, that is even better. The tempo is fastest in experimental psychology, according to a citation study reported at a 1976 meeting of the 4S (Society for Social Studies of Science). I am told that you are out of it if you stop scanning the experimental psychology journals for about three years.

2. In order to maintain a winning tempo of research, experimenters have to organize their research more efficiently by increasing the division of labor and tightening research schedules. A big winner like Raymond Cattell has to have several research projects running at all times. He himself, the PI (principal investigator), has to concentrate on designing the projects, hiring researchers, writing grant applications and expenditure and progress reports, in a steady stream. His assistants, the co-1, supervise the research and help him write the final reports, journal articles, and convention papers. Graduate students do the actual research and earn coauthorships. These experimenters are disciples of the mythical Dr. Grant Swinzer, interviewed by D. S. Greenberg in 1966, head of the National Animal Speech Agency (NASA), which had its origins in the president's challenge to the nation to teach an animal to speak by 1970 (Greenberg, 1967).

3. A by-product of grantsmanship is the information crisis. Each PI writes an initial report of the theoretical or social problem with which the proposed research will deal, as a way of alerting colleagues in the field. The article is a rewrite of the grant proposal. The researcher reports the research design and current progress in a conference or convention paper, which is published in the convention proceedings, a nonbook. The final report to the funding agency, which concludes with a statement of the urgent need for more research, must of course also be published. Sometimes revised versions are later published in an edited volume or festschrift, and an expanded version with later results may be published as a book.

The purpose of these publications, except for the book, is first to claim proprietorship in the research design and data for citation purposes, and second to advertise the results in a variety of public forums aimed at different readers or listeners. But because of the flood of publications, few people read these reports, in the natural sciences at least. Ravetz cites a paper by Urquhart which reports that, of the journals in the Science Library in London, about half were not used at all and another quarter were used once in the year of his survey (1971, p. 49). Researchers scan the abstracts to find references they can cite in their arguments, but citation does not require reading the whole article, to say nothing of replicating it or checking its citations.

In summary, as graduate students begin to move into the public arena they are faced with the imperative of building up and reinvesting their symbolic capital in continuous research. The successful ones, the ones you have heard about, manage to move into an existing or new community that can use their publications as material for its own productive efforts. They in turn find in the community the materials they need for the manufacture of knowledge—ideas, data, research techniques, discussants, and critics. Community members help each other get a wider audience by citing each other's work, but they also compete for the approval of this audience. The most efficient producers and advertisers gradually (or suddenly in new communities) move into a leadership position in which their ideas are used and cited by newcomers. As their leadership and their symbolic capital becomes more secure, they become spokespersons for the movement, editing surveys of latest developments (Knorr-Cetina and Mulkay, 1983) or writing a definitive account of its achievements. They also move into foreign relations, deploping the bickering and misunderstanding that goes on among different communities, calling for a start at interdisciplinary collaboration, and developing a broader theoretical synthesis that includes other communities as parts or allies of their own community. If their fame is great enough they can even build an empire (Talcott Parsons), assigning various research communities their proper places in a great collaborative scientific advance.

We have now progressed far enough to be able to catch a glimpse of our ultimate goal, truth or knowledge. Knowledge is a manufactured commodity that has both use value and exchange value. The researcher "buys" the ingredients for his product by citing the earlier producers whose ideas he is using. Then he puts together his own product and tries to get it published. The payoff to the earlier producers is the citations and the use of their idea; these provide recognition—that is, symbolic capital. If the new product is not published, it fails, of course; but if it is published and not cited, it also fails. After a few years have passed, it probably will not be cited; other, more recent works by competitors will be cited and used instead.

The successful producer avoids such failure by producing for a definite market, some active research community. Then, having accumulated symbolic capital, one has to use it within a few years to get the next
product published, read, and cited. One cannot put capital into a bank for fifteen years while raising children and studying others' works. By that time, one's name and products will have been forgotten; one's capital will have lost its value and one must start over.

We see that knowledge or truth is a very perishable commodity, less perishable than grapefruit, but more perishable than even American-made cars. It has to be used within five to ten years, before it spoils. A very few products last for several decades, but most products have little or no value from the start (Fuller, 1988, pp. 29–30).

**SCIENTIFIC COMMUNITIES**

If the fate of a publication in the public arena depends on how it fits into the current interests of some research community, then the growth of scientific knowledge depends in its social aspect on how those communities develop over time.

Bourdieu, with his theory of symbolic capital, and Mulkay (1980, pp. 19–21), suggest that a successful community goes through a typical cycle that should last a bit longer than a generation. It should begin explosively as a small group which attracts attention by bold pronouncements of a new paradigm, a new subject matter, a new foundation, a new start. These statements are actually announcements of an opportunity for original achievement. The statements attract entrepreneurs eager for recognition, who rapidly produce original achievements in the new subject matter. Their payoff is recognition by founders and newcomers, and they in turn pay tribute to the community founders whose ideas they cite and develop. The members of the community recognize one another and thereby accumulate symbolic capital; but the real payoff, the surplus value, comes from recognition by newcomers. As the community expands, the level of competition increases and the opportunity for recognition decreases, so the attractiveness of the community to newcomers declines. Thus the rate of expansion and of surplus value production declines.

The main disciples dominate the research projects, conferences, and graduate students. The followers become junior researchers in projects run by subleaders, and their work is cited under the subleader's name. Newcomers with almost no symbolic capital, small entrepreneurs in an oligopolistic market, find it very difficult to get recognition and an independent research grant; and, having published, they find that few will cite or use or even read their work. A few newcomers manage to develop some small unique product that other researchers can use, some statistical technique or concept.

At this stage, it no longer pays to join the community, except as a student of some main disciple or subleader, and the prospect of founding a new community becomes more attractive. One or possibly two of the founding attempts succeed, and a new community appears and rapidly reaches critical mass.

Meanwhile the main disciples in the old community differentiate their theories in order to retain recognition or even expand their following. If they merely repeat the original formulation, the credit goes to the founder or founding group, and they are forgotten. As a result, the community disperses into subcommunities with variant theories. However, at this stage the community stops growing and may decline in numbers, as followers and newcomers rush to join some new movement. The leaders' peace proposal to the new movement is rudely ignored and the old theory is attacked, simplified, and distorted—for instance, the microsociologists' distortions of Merton. Some of the variant subcommunities no longer attract followers, and they stop growing and are forgotten. One or two subcommunities may expand for a while and become independent movements with their own main disciples and subcommunities.

As the original founding group dies off, the original community has disappeared. In its place are two or three aging former subcommunities whose elite were subleaders in the original community. New scientists move into the still expanding new communities and ignore the old theory. They are taught a distorted version of it and regard it as obviously false. Its failure shows off the success and promise of the new theory by contrast.

The above is a simplified sketch of American sociology in the last forty or fifty years. (The main source is Mullins, 1973, updated in 1983.) Mullins uses citation counts to trace the trajectory of seven main theory groups during the 1960s. Functionalism and symbolic interactionism, which had their explosive growth in the 1950s, had stabilized and had a settled hierarchy with subcommunities. Five new theory groups appeared in the 1960s: exchange theory, causal models, ethnomethodology, radical critical sociology, and Mullins's own network analysis. In his 1983 update he reports that two of the new groups, ethnomethodology and the radical critical theorists, continued to expand successfully in the 1970s, while
network analysis and causal models seem to have declined and become special techniques. Another small group, the forecasters or futurologists, have collapsed. Functionalism has essentially disappeared, while a variant of symbolic interactionism has been unexpectedly revived in the 1970s. However, its final disappearance cannot be long postponed. Mullins writes of the United States; but functionalism came later to Germany and is still very much alive there (J. Berger, 1982). Berger (p. 353) quotes Habermas (1981, p. 297): "Keine Gesellschaftstheorie kann mehr ernstgenommen werden, die sich nicht zu der von Parsons wenigstens in Beziehung stellt." That is, you have to study Parsons.

One sign of the passing of functionalism is its reduction at the hands of critics to a single concept: functional explanation. This counterfeit concept was invented by nonfunctionalists who were trying to fit the theory into their own categories and thereby "clarify" it. Then it was used by other social scientists as an example of an error that their theory avoided. Their theory consequently was superior to functionalism; social science had progressed.

An early critic was Nagel (1961). Nagel was a logical empiricist; he conceived of scientific explanations as deductive-nomological: A causes B; A, therefore B. The occurrence of B, the effect, is explained by the previous occurrence of A, the cause, plus the causal law connecting A and B. But functional explanations did not look like that, after Nagel had "clarified" some of them. They seemed to be backwards: the function of A is to cause B; B is needed, therefore A. Illogical, even teleological, Nagel comments.

Another source was Stinchcombe's 1968 book on causal modeling. Stinchcombe discusses a variety of ways in which causes can be interconnected. One way is by feedback: A causes B; part of B feeds back on A and shifts it. He called that model "functional," a very loose, imaginative metaphor. Stinchcombe in the 1960s and after was eclectic in his thinking; he used ideas from various sources without being exclusively committed to any one theory. His 1978 work, Theoretical Methods in Social History, criticizes Parsonian functionalism and emphasizes individual rational choice rather than generalized functional problems. "A functional strain...creates troubles for the people in it" (p. 120). His 1983 work on the forces of production looks vaguely Marxist. His 1985 article on social insurance illustrates his "functionalist" feedback model: rising levels of social tension activate a mechanism which selects a social insurance policy. Feedback from changes in tension levels sustains or changes the policy, until a lowering of tension stops the mechanism. In all cases, Stinchcombe suggests, the final policy is a contributory transfer payment scheme in which workers pay for their own insurance (1985, p. 419).

The main recent sources of the concept of functional explanation seem to be G. A. Cohen (1978), and Elster (1979). (For later discussions by these two writers, see the chapters and references in Roemer, 1986.) These two in turn refer to Stinchcombe and Nagel. Elster as of 1979 is a rational choice theorist, as are the critics who cite him (such as Hechter, 1983, p. 10; 1987, p. 24; and Heijdra et al., 1988). For Elster, one must explain an event by showing how it resulted from the rational choice of one or more people. But neither the Stinchcombe model nor the Nagel model refers to anyone's rational choice, so they are not explanations. Functionalism is unscientific. Cohen's plea that a "functional explanation," while unsound, is at least the start of an explanation, an explanation sketch, is rejected. Game theory will provide the explanations. As an enthusiastic game theorist, I cannot disagree; but such a development would supplement functionalism, not invalidate it. 8

A decade from now some eager innovator can write "Who now reads Talcott Parsons?" and treat this as a sign of scientific advance. (See also Bryant, 1983, though Bryant's question is factual, not rhetorical.) Several decades later some enterprising Lou Schneider-type sociologist can resurrect Parsons and declare to an incredulous discipline that there were some good ideas way back there. Or in psychology, a Jerry Fodor can assert that Gall, the nineteenth-century founder of phrenology, "appears to have had an unfairly rotten press" (1982, p. 14) and that Gall actually had a pretty sound basic approach. Faculty psychology, misrepresented and dismissed in one paragraph in Baldwin's 1911 Dictionary of Philosophy, is an approach well worth pursuing today (pp. 23–24).

Pickering (1985) reports a similar development in British high-energy physics of the 1970s. Research and publication in this field depends crucially on access to the one British atom smasher, and access is controlled by a peer review committee. By 1970 the old high-energy community had fragmented into several subcommunities, each represented on the peer review committee. Suddenly a new community appeared with a new research program. The new program had less explanatory power than the old, according to Pickering, but it was new. Its members gained access to the committee and, since they were unified, they outvoted the scattered old groups. Within two years the old groups were almost shut out of research. Thereafter graduate students were trained only in the
new program, and a continental group of physicists designed a new machine to the specifications of the new theory. Here was a revolution without crisis or anomalies or, indeed, without progress.

In summary, if we look at scientific communities through the concepts of the microsociologists we find an intellectually random coexistence and succession of communities. The rapid growth of a new community draws members from older ones, and the diminishing opportunities for original achievement in mature communities induces entrepreneurs to try founding new research programs. A few ideas from an old theory are translated into the new frameworks; the rest are caricatured, ridiculed, and forgotten. Some communities become very large, with complex theories and extensive empirical work, and last for decades; others flare up and decline in a few years.

The rate of appearance of new research programs depends on the amount of investment capital available for new research. Thus Mullins (1983) notes that the 1960s were a time of rapid expansion both of sociology departments and of research funds. The result was the appearance of several new research programs. In the 1970s and even more in the 1980s both funding and jobs stopped expanding and then declined; hence, no new theories and the decline of some old ones.

We will give Latour the last word on the work of the microsociologists (1981b, p. 212): "Knowing what a science is made of, we should not want to develop one. . . . We do not want and do not intend to be scientists."

Perhaps Merton was right. Perhaps humility is essential to science after all.

We seem to have lost our way, and need to pick up a different thread to follow. Bourdieu (1975) suggests that the course of social science is politically determined; so also does H. M. Collins (in Knorr-Cetina and Mulkay, 1983, p. 96). Randall Collins (1989) presents historical evidence that philosophical schools have flourished and expanded when they had strong political support, and have shrunk without such support; perhaps the same is true of social science. Let us explore this possibility next.

FOR THE MERTONIANS, social science is an autonomous subsystem of society unified by its own institutionalized values. The values have to be maintained by socialization and by rewards handed out by elite scientists for conformity. However, Mertonian empirical investigations have complicated this scheme by suggesting that all or nearly all norms are accompanied by equally valid counternorms. The resulting normative ambivalence produces conflict as well as unity, since scientists can urge norms on others while they themselves follow counternorms. Shared values thus can serve as rhetorical devices in disputes, rather than as unifying forces. Consequently, the unity-disunity balance must be determined by something other than the norms-counternorms themselves.

Similarly, the norm of value neutrality, which maintains autonomy, is opposed by the counternorm of value commitment or involvement, which reduces autonomy. Value neutrality and value commitment each require or presuppose the other, as with the other norm-counternorm pairs. For instance, the Mertonian-functionalist commitment to social self-maintenance through government requires detachment, value neutrality, in the study of social problems, and vice versa. Consequently, the degree of scientific autonomy also depends on the nature and strength of social and political influences on science.

But the microprocesses of negotiation, construction, and accumulation of symbolic capital (discussed in chapter 7) provide neither unity nor autonomy; they produce a kaleidoscope of shifting theories, research networks, and presumed facts. What, then, maintains the unity and autonomy of the social sciences—or are these qualities illusions?

In this chapter we take up the issue of autonomy by looking at some political influences on social science. Mendelsohn et al. (1977, pp. 17–20) and van den Daele (1977, pp. 40–48) argue that Merton neglected the political control of natural science in England after 1660. They assert that scientists researched topics of interest to the political authorities and
but Merton's norm of humility will not correct overconfidence, since confidence is a matter of personality dynamics. And since people who are successful in the social science rat race tend to have a great deal of self-esteem, the outlook for improved objective knowledge is poor indeed. The prospect brightens only if we survey the obvious past progress of some field from the laughable errors of thirty years ago through the discoveries and improvements of our predecessors, culminating in our own brilliant illuminations and achievements.

DO SOCIAL SCIENTISTS' personalities find expression in their work? Yes, they do. Personality effects are a class of experimenter effects, and appear in all methods involving direct contact with people—experimentation, interviewing, survey research, clinical research, and ethnography (Rosenthal, 1966). "Experimenter who differ in anxiety, need for approval, hostility, authoritarianism, status, and warmth tend to obtain different responses from their experimental subjects" (Rosenthal, 1983, p. 91).

We can arbitrarily distinguish three kinds or levels of personality influences for study. First, there are the direct effects on other people, mentioned above. These include the extensively studied experimenter effects on subjects, interviewer effects on respondents, clinical and ethnographic effects on subjects, and the effects on other social scientists in discussion. Personality effects are interferences to be systematically investigated and controlled; but they are also an essential resource in research. The researcher's personality should normally be nonthreatening, reassuring, to set the respondent at ease and prevent a defensive response. Thus the ethnographer studying a dependent, low-power or low-status group cannot appear to represent external or internal authority. In interviewing, the race of the interviewer should normally match that of the respondent. But sometimes a particular kind of personality effect is a tool for a more active probing method. Thus a somewhat authoritarian analyst can bring out the oedipal problems of an analysand for clinical study and working through, and matched female and male interviewers can be used to study how political opinions express attitudes toward gender (Hoag and Allerbeck, 1985).

Second, there are the influences of a researcher's personality on his concepts and values, and on the problems that attract his interest. Consider for example the concept of authority. What authority means to a person depends on what kinds of authority one has experienced and on
what sort of responses, coping mechanisms, and internalizations one has developed during these experiences. Thus Bruno Bettelheim, a fairly authoritarian person himself, used to say in class that Adorno et al., authors of The Authoritarian Personality (1950) had the wrong concept of authority. They thought only of bad authority, rigid, punitive, self-centered; but there was also good authority that maintains necessary order and direction, which they did not study at all. They gave authority a bad reputation. In addition concepts of authority are central for some writers such as Max Weber and Reinhard Bendix and peripheral for others, probably depending on the centrality of authority relations in their upbringing. Thus Namewirth observes that in biology the concept of DNA as giving detailed orders to the cell as it develops is an authoritarian concept. An alternative concept would be the interaction or collaboration of several influences during development. Both concepts have some usefulness in understanding DNA (Bleier, 1986, pp. 25–28).

Or consider the concept of nature. For some, nature is neutral matter to be informed in Aristotle's sense, raw materials to be sorted out, transformed, cleared away, and used up. For others, nature is a wild and dangerous challenge—swamps, alligators, mountains—to be conquered, tamed, filled in and paved over. For still others, nature is a quiet, slow-moving place where one can escape the stresses and strains of city life. In some primitive cultures, nature was the source of all life, to be respected or even worshipped and not to be changed at all. More recently, nature has been regarded as the ecosystem that surrounds and sustains us; we, as the conscious part of the ecosystem, have the responsibility of maintaining and improving its complex functioning. These various concepts express personal and cultural differences.

Third, there are the ways in which researchers cope with data and concepts and problems, their cognitive style. Variables include the familiar Rorschach variables: focusing on wholes, parts, or details; emphasis on shape, color, or gray areas; motion; popular and animal responses; inability to perceive certain objects. Social science data are usually somewhat ambiguous, like the ink blots in a Rorschach test, so people with different cognitive styles will make different things of them.

PERSONALITY EFFECTS

I shall present two extreme, obvious examples of personality effects. The first example illustrates a good effect that uncovers otherwise hidden data; the second illustrates a bad effect that interferes with inquiry. In both cases, we do not inquire or speculate about the personality that produced these effects; we merely observe the effects on other people.

Neil Friedman cites a number of experiments with IQ tests that correlate tester-subject race with IQ scores. The results were that black subjects scored lower with white testers than with black testers, and white subjects scored lower with black testers than with white testers (1967, pp. 114–16). Here the race rather than the personality of the tester is the influence that either brings out or hides the subject's intelligence. Friedman observes, "It seems that Negroes learn very young to 'play Negro' with a white examiner." (p. 116). Note that Friedman focuses on the IQ of black children as the problem; he later taught at Tuskegee Institute in Alabama.

Friedman continues with a case report, quoted from Riessman (The Culturally Deprived Child, 1962, pp. 49–50) that shifts from race to personality: A few years ago a birthday party for a member of the staff at a well-known psychological clinic played a novel role in the test performance of a Negro child. Prior to the party this boy, whom we shall call James, had been described on the psychological record as "sullen, surly, slow, unresponsive, apathetic, unimaginative, lacking in inner life." This description was based on his behavior in the clinic interviews and on his performance on a number of psychological measures including an intelligence test and a personality test. His was not an unusual record; many culturally deprived children are similarly portrayed.

On the day of the birthday party, James was seated in an adjoining room waiting to go into the clinician's office. It was just after lunch hour and James had the first afternoon appointment. The conclusion of the lunch break on this particular day was used by the staff to present a surprise birthday cake to one of the clinicians who happened to be a Negro. The beautifully decorated cake was brought in and handed to the recipient by James' clinician who was white, as were all the other members of the staff. The Negro woman was deeply moved by the cake—and the entire surprise. In a moment of great feeling, she warmly embraced the giver of the cake. James inadvertently perceived all this from his vantage point in the outer office. That afternoon he showed amazing alacrity in taking the tests and responding in the interview. He was no longer sullen and dull. On the contrary, he seemed alive, enthusiastic, and he answered questions readily. His psychologist was astonished at the change and in the course of the next few weeks retested James on the
tests on which he had done so poorly. He now showed marked improvement, and she quickly revised not only the test appraisal of him on the clinical record card, but her general personality description of him as well.

Here the white tester’s act of love, and James’s observation of its warm acceptance by a fellow Negro, overcame James’s defensiveness and opened a deeper level of James’s personality to clinical observation.

The second extreme example comes from philosophy. It consists of personality effects on other philosophers—that is, rhetoric. The example is Adolf Grünbaum’s treatment of hermeneutics in The Foundations of Psychoanalysis (1984, pp. 1–94). Grünbaum’s rhetoric begins mildly with such terms as “ludicrous” and “not a scintilla of evidence” (p. 14), “stone age physics” (p. 20), and gradually gets more sarcastic and scornful: “cognitive tribute to the patient” becomes “cognitive monopoly” (p. 21) and “truly formidable epistemic powers” (p. 30); “mutilation” becomes “ontological amputation” and “ontological stultification” based on an imported ideological objective (pp. 43–45); Grünbaum’s comments run from “mirabile dictu!” (p. 48) to “fundamentally incoherent” (p. 49), “incongruous” and “pernicious myth” (p. 52), “guilty of legerdemain” (pp. 53, 65), “piece of philosophical malfeasance” (p. 53), “the entire hermeneutic enterprise is ill-conceived” (p. 54), “interpretive meaning—whatever that is” (pp. 59, 65), “my charge of utter emasculation” (p. 60), and “naive, if not smug, dismissal of the completely unsolved problem” (p. 65). And so on.

Grünbaum’s sarcasm and scorn are a challenge to a fight, or debate, as philosophers call it. But in a fight the purpose is to beat up the adversary and cast him out, not to understand him. Consequently, the hostility expressed in his rhetoric interferes with his understanding of hermeneutics; he wants to fight, not to understand. It also dissuades others from discussing the topic with him, unless they too want to fight.

We turn now to the second kind or level of personality influences, influences on the scientist’s concepts, values, and choice of problems.

A CASE STUDY: MAX WEBER

Arthur Mitzmann’s study The Iron Cage (1970) is based on Marianne Weber’s biography of her husband, Max Weber’s autobiographical account of his years of nervous breakdown, letters of Weber and his relatives, interviews with survivors, official records, and all of Weber’s writings published and unpublished. The method is historical case study, in which all available documents are checked against each other and pieced together like a jigsaw puzzle to form a coherent story of intertwined motives, themes, and influences.

Portis (1986, chap. 2) gives a somewhat different interpretation focusing on Weber’s breakdown, which he describes as primarily an identity crisis. However, this topic does not concern us here.

According to Mitzmann, the family dynamics that imprisoned Max were the conflicting expectations of his father and mother plus the contradictory demands of his mother, as reinforced by the German culture of the time. Weber’s writings expressed his successive interpretations of these demands and constituted in part an attempt to distance himself from them and overcome their grip on him. Portis emphasizes the strong internalized demands of the mother (1986, pp. 30ff.), but adds that some writings also were part of his search for a clear identity separate from the mother.

Weber’s paternal ancestors were expelled from Salzburg by the Catholic archbishop for their Lutheran Ketzerrei (as were some of my paternal ancestors); his grandfather became a rich capitalist and his father was a comfortable, authoritarian civil servant, an ally of Bismarck and the power structure. His mother’s ancestors were expelled from France for their Calvinism, and his mother, who hated sex, urged her Calvinist morality of total asceticism, hard work, and compassion for the poor on Max.

Weber’s early writings contain themes that express his interpretation of these demands: the conflict between political responsibility, power, and conscience (Mitzmann, 1970, pp. 37, 51); the argument that Bismarck (power, father’s ally) ought to be limited by moral idealism (mother) (p. 35); and the argument that the East Prussian agricultural workers want to leave the patriarchal house at any price (pp. 95–96). Here the patriarchs are the Junkers, allies of the authoritarian father whose house Weber was then trying to leave literally and figuratively. The Protestant ethic represented Weber’s own compulsive work—aimed, hopelessly, at gaining his mother’s approval (pp. 171–74): “By identifying the work ethic of his mother’s Calvinist ancestry as a device which formerly gave evidence of divine grace but now served only as a ‘housing hard as steel,’ Weber was focusing his intellect on his own experience in order . . . to liberate himself from it” (p. 173).

In the years after 1910, the main themes in Weber’s writings are the functional rationalization of modern life, involving bureaucratization (fa-
ther) and ascetic self-control (mother), opposed by the releasing force of charismatic leadership (p. 179). According to Mitzmann, charisma represented for Weber a feminine, erotic, emotional release from the iron cage of compulsive rationality, corresponding to a release Weber was then experiencing (pp. 299ff.). However, the liberating effect of charisma is doomed to gradually disappear in a new bureaucratic routine; bureaucratic and political rationalization will always overcome Eros. The same absolute opposition between Eros and authority (functional rationality) and the same pessimistic prognosis appeared in the German upper middle-class culture of the time, as expressed in Heinrich Mann's Blue Angel and Thomas Mann's Buddenbrooks, haunting and terrifying works.

What evidence does Mitzmann provide for his contention that the major themes in Weber's writings express his current libidinal experiences? First, the major concepts in his writings — bureaucracy, asceticism, rationality, charisma, authority, power, conscience — correspond to the major interpersonal influences and conflicts in Weber's life (availability bias). One concept, tradition, is missing from Mitzmann's account. Other social science concepts of about 1900 such as anomie, collective conscience, conspicuous consumption, general market equilibrium, work as objectification and self-development, are foreign to Weber's experience and writings. For example, work for Weber was an exercise in asceticism, an endless struggle to control one's own desires, not a form of self-fulfillment.

Second, the changing treatment of these concepts, for instance in four successive drafts of Weber's study of East Prussian agriculture, corresponds in detail to changes in Weber's personal circumstances. Third, Weber's interpretation of these aspects of modern society and his stern and pessimistic prognosis correspond to their personal meaning for him. Contrast his treatment of rationality with that of Pareto, Walras, Veblen (the instinct of workmanship), or Marshall. In none of these contemporaries of Weber does rationality appear as deadening, compulsive bureaucratic authority.

Now, supposing that Mitzmann is more or less correct, can we judge Weber's sociological work to be defective insofar as it expressed his personal strivings and conflicts? Obviously not. The projection of his internalized parental identities on to his subject matter, German society, sensitized him to aspects of modern life that were hidden from sociologists like Durkheim, Pareto, and Veblen. In particular his understanding of the dynamics of (German) bureaucracy, the aloof authoritarian father, was an intellectual achievement that generations of sociologists with somewhat different personalities could build on. Conversely, since every focusing of attention, every sensitivity, negates other areas of attention and other sensibilities, Weber's personality excluded other aspects of modern society such as those studied by Veblen and Marshall.

What can we learn from Mitzmann's study? First, knowing Weber's family constellation, life history and identity crisis, and cultural milieu helps one to understand some otherwise puzzling concepts, pairings, contrasts, and omissions in his writings. Second, seeing how Weber's personality and culture expressed itself in his writings might help us recognize ourselves in the problems we pick to study, the tactics we use, the conclusions we try to reach or avoid.

For example, we can appreciate Weber's treatment of scientific method better when we read about the internalized demands his mother placed on him. Scientific method for Weber was an embodiment of those demands: scientists ought to practice total asceticism, self-denial, painstaking and compulsive concern with fact. Method demands complete control and repression of one's values, feelings, desires, imagination. No personality influences are allowed. Of course, the old Adam in us, our original sin, breaks through anyway and contaminates our research, but we must try to hold it back. If you think that way about research, notice it.

Third, we can generalize, superficially, from this one case by classifying the themes that appear in it and allowing for variations of each theme in other cases. Every personality is different, but most social scientists have a mother and a father, and usually also sisters and brothers. Brothers and sisters we cannot deal with, as they do not appear in Mitzmann's study.

German fathers are often authoritarian, like Bettelheim and like Weber's father, though the authoritative rules and expectations can vary greatly. Weber's identification of authority with bureaucratic rationality, national political responsibility, power, and agrarian patriarchy is a particular combination. Given this sort of father, a son can identify with authority; submit in hopes of receiving the benediction of masculinity; or rebel. Perhaps all German sons do all three, as Weber did, but in different combinations and circumstances. Weber's rather remote and limited empathy with the East Prussian peasants is a particularly weak example of rebellion, suitably displaced a good distance away. Mainly he submitted, fruitlessly; bureaucracy remained an external constraint for him.

With a nonauthoritarian father, one pattern is for the mother to
devalue the father by contrast with her internalized authority figure, her own father. In that case, the son's task is to live up to the mother's internalized ideal, by endless achievement. Each success implies a devaluation of the real father, with resultant guilt, but no rebellion. Such a pattern may perhaps be found in some compulsive high achievers who publish continuously.

The demands of Weber's mother for (1) rebellion against the tyrannical father and (2) total sexual asceticism and compulsive work are an unusual maternal constellation, whose expression in Weber's thought has been discussed above. Such demands often come from the father, and can be rejected in part; that they came from the mother meant that Weber had no internal feminine alternative to counterpose against authority. Instead, love and temporary liberation came from outside Weber's marriage, just as charisma comes from outside the iron social order and brings temporary release. Weber had no concept of Mother Nature, the all-enveloping biosphere that sustains us, as it appears in the writings of ecologists like E. F. Schumacher. Indeed, he had no concept of nature at all. Nor did he have a concept of social totality, the central concept in Lukács's contemporaneous work (1923). Such "maternal" concepts are foreign to Weber's thought; instead, his mother gave him the Protestant ethic.

GENDER AND SCIENCE

Up to here we have looked at only half of the picture, the masculine half. All the categories and contrasts derivable from the Weber study apply only to men. We now call on feminist writers to lead us into the other half, and also to provide an external perspective on the masculine half. (The main references for this section are Gilligan, 1982, chap. 1; Harding and Hintikka, 1983; Keller, 1982, 1985; List, 1985; Belenky et al., 1986; and Kreisky, 1986.) Actually Maslow anticipated most of the feminist ideas, though vaguely and impressionistically (1966, p. 35, chap. 8–11). The feminist ideas are in an early stage of development, and are not to be taken as fixed and final. They are based on extensive interviews and on observation of children's play.

According to the feminist writers cited above, gender differences begin to develop in the first few years of life, from differences in the relation to the mother. Given adequate maternal care, babies at first experience a single undifferentiated process of living, and only gradually differentiate self and mother as two parts of this process. Later the father also becomes specified as the guardian and gateway to the outside world, the person who provides adventure, exploration, and protection against the unknown, and who has the skills to deal with things out there. The feminist writers do not approve of this type of child care, merely note it as fact.

Boys soon learn that they are different from the mother and more like the father. Thus they must manage a double separation from the mother, one in defining the self as a separate person and one in defining their gender as male. Separateness is the key to growing up, and separation involves giving up, denying, rejecting the infantile unity with the mother. Masculinity is an achievement, one that is always threatened by some surviving remnant of infantile habits, some unsuspected practice that can be shamefully exposed with the taunting cry, "Sissy!"

Girls in contrast learn that they are like the mother, so the infantile unity does not threaten their femaleness but on the contrary confirms it. Consequently, they do not need to reject and deny their infantile sense of selflessness. As a result for girls their femininity is relatively secure but their sense of separateness, of being a unique person with clear boundaries, is more fragile. For girls the separation is rather from the world outside the family, and they need the other, the father, to lead them outside and protect them. The path to the outside world takes them away from themselves. For boys, in contrast, the outside world is their realm, the place where they find and assert themselves as masculine by showing their skills and independence.

These differences appear and are reinforced in children's play (Gilligan, 1982, chap. 1). Small girls tend to play indoors or near home in small groups or pairs, and play cooperative or take-turn games. Boys tend to play outdoors in larger groups, and their games are competitive and argumentative. Competition expresses and reinforces their sense of autonomy; and achievement, winning or playing well, demonstrates that they are skillful like the father and therefore masculine.

Competitive games, played by separate but equal individuals, require rules, and rules require a judicial process of argument and judgment. Thus boys learn to think in terms of rules and their application to cases by seemingly impartial arguments. They want to win, but win fairly by skill rather than by cheating.

Masculine science continues these childhood themes, plus others that appear later in life. For the male scientist, his subject matter is the
outside world, the arena in which he must demonstrate his skills and thus his masculinity. He is not part of that world, but separate from it just as he is separate from other people, and just as his discipline is separate from other disciplines. The world is neutral material which he transforms by his work, or controls and organizes according to some plan of action. It must become familiar territory through which he can move confidently and whose changes he can predict and control according to his purposes. In order to do this he must focus his attention on the world, not on himself. His inner states are not part of the outside world, so attending to them is not science, not man’s work, but daydreaming.

There are three kinds of masculine relations to other scientists. First, in order to demonstrate skills the male scientist must learn them and must learn his way around the world, so he needs a mentor or mentors to guide and train him. He also needs to receive the blessing of manhood, of equality, from this father figure. After that he can identify with the mentor by extending the mentor’s theory and research, and/or can rebel and assert independence. In Weber’s case the mentor, Hermann Baumbarten, came from the maternal side of the family and thus strengthened the maternal influence on Weber’s thought and made bureaucracy an external phenomenon.

Second, with other scientists within his discipline he is competitive, either individually or cooperatively in teams, as in childhood games. The prize for winning can be individual recognition, citations, invitations; but it also includes acceptance and broader use of the theory or method that one’s team is pushing. The most intense competition, Bourdieu reminds us, is with competitors for research funds.

Third, scientists from other disciplines are outsiders or even strangers, to be ignored or treated politely. However, if the outsiders pretend to know anything about the discipline, then they are politely but condescendingly put down. If they insist on horning in, they are put down more firmly. Extreme examples are Karl Brunner’s repeated expressions of contempt for sociological (Keynesian) concepts in economics, plus his Popperian contempt for sociology in general. Nelson Polsby (1981) explains the total falsity of elitism by observing that the elitist theorists are sociologists who know nothing about politics and never will.

A female scientist can also take the masculine route, as in Eva Kreisky’s case (1986): by finding a mentor (Josef Hindels) who can lead her into the outside world of men and their impersonal, factual discourse, away from the inner personal world of women, away from herself. But this route involves denying one’s own feminine integrity and is unsatisfactory, as Kreisky found. A feminine social science, speaking abstractly, involves denying the sharp distinction between inner-personal and outside-impersonal world, and also the sharp distinction between the scientist and her object of study. Just as the slogan of the New Left women’s movement, “The personal is the political,” denied the separation between personal family life and outer, impersonal political life, so the slogan of feminist social science could be “The personal is the social.” “Human beings are socially constituted, and have emotions, beliefs, and abilities only insofar as they are embedded in a web of interpretation that gives meaning to the bare data of inner experience” (Scheman, in Harding and Hintikka, 1983, p. 232).

In this kind of social science, the scientist becomes a part of her subject matter or relates closely to it, empathetically sharing her experiences. Instead of organizing and systematizing it according to her theories, she listens to its voices, “lets the material speak to her,” “becomes part of the system,” lets it tell her what to do next (Keller, 1982, p. 599). She accepts the other’s views, patiently shares in gossip and small talk to get the feel of how the other thinks, watches or helps the other develop over time, watches her own participation, and learns about herself along with the other (Belenky et al., 1986, pp. 112–23).

Since her need is more to participate than to demonstrate individual prowess and achievement, her relations with other social scientists are primarily cooperative. Instead of pushing her own ideas, she is sensitive to other points of view and tries to include them in her own thinking.

Gilligan calls the feminine kind of knowing “connected” and the masculine kind “separate.” Connected knowing is kennen and separate knowing is wissen (Belenky et al., 1986, pp. 100–01).

The weaknesses of both kinds of science express points of strain in the two paths of personal development. The characteristic weakness of the little girl is her fragile sense of self as a unique individual. Apart from overcompensation à la Ayn Rand, such weakness would express itself as a tendency to lose one’s own point of view in the process of appreciating others’ ideas. Elsewhere I have described this weakness as being good at losing arguments, because one gets interested in the opponent’s line of reasoning, looks for presuppositions that would make it plausible, extends it to related topics, looks for its strong points and limitations, and
in the process forgets or loses interest in one's own claim. Another version of this weakness would be a tendency to get pulled into various areas of research because there is no area or discipline that is one's own.

One characteristic masculine weakness is to overcompensate for the little boy's precarious sense of masculinity, that is, for one's fears about one's professional ability, by developing an egocentric self-esteem, a self-image of unfailing competence and infallible reasoning power, and scorn for everyone else. Such an attitude expresses itself in a lack of interest in others' ideas and research, emphatic and forthright statements of one's own ideas, rejection of any criticism of one's research, and insistent pushing of one's own work in all available forums. Examples here are abundant; one thinks of Mitroff's brutally masculine moon scientists, insisting on their own ideas and scornful of others'. (See also List, 1985, on masculinity in scientists.)

A closely related weakness is to get absorbed in the competitive aspect of science in order to affirm one's own masculinity and disparage that of others. This tendency is manifested in inconclusive disputes characterized by scorn and sarcasm for the opponent, misrepresentation of his position by taking some statement out of context or pushing his position to an extreme to "make it interesting" (Newton-Smith, 1981), or putting it into the context of one's own theory, and above all never admitting to any error of one's own. Team competition takes the form of attacking some other theory or research tradition again and again and exposing its longstanding errors, disconfirmations, and pseudo-scientific character.

Exaggerated competitiveness is even more prevalent in philosophy (cf. Moulton, "The Adversary Method in Philosophy," in Harding and Hintikka, 1983). In philosophy colloquia one sees people in the audience striving in excitement, eager to tear up the speaker's argument and expose its errors and non sequiturs. Nicholas Rescher begins his book on The Strife of Systems (1985) with this observation:

The ranks of philosophy are in serious disarray. Theory confronts theory, school rivals school in implacable opposition. Disagreement and controversy prevail to such an extent in this discipline that one can safely endorse the quip: If two people agree, one of them isn't a philosopher.

Philosophers use another technique to separate themselves from their subject matter. They can claim to be unimpressed with the empirical details of their topic, because their concern as philosophers is only with its logic—for instance, the logic of theory testing or the logic of practical decisions. Thus they simultaneously claim separateness and superior status as the guardians of reason and logic.

A third masculine weakness mentioned by the feminists is the tendency to control or dominate one's experimental subjects and to use one's knowledge as an instrument of control (Keller, 1982, 1985, chaps. 5–6). Control is supposed to relieve the anxiety of separation by ensuring that the object of study remains in one's grasp, without giving up one's separateness. I'm afraid I have trouble understanding the argument on this point.

Enough on the subject of weaknesses.

If these abstract accounts of masculine and feminine science are correct, one would expect masculine types to feel comfortable with experimental and formal methods and aggregate data analysis, and feminine types to gravitate toward ethnographic participant observation and interviewing. Gilligan's research, for example, uses interviews organized as panel studies; Belenky et al. interviewed 135 women intensively over several years. Both interviewing and participant observation bring one into close relation with one's subject matter, and in the latter case even requires one to become part of it. Roe's study of anthropologists and clinical psychologists (1952) brings out "feminine" personality characteristics in people who chose participant observation as their method.

Formal methods and aggregate data—wholesale price index, percent change from trough of cycle—separate one from people and allow one instead to deal with numbers and mathematical objects by abstract, formal reasoning. Experimentation brings one into physical proximity with people, the S; but the emphasis in this method is on control. The S need not be experienced as whole people but as future numbers; and if the experimenter wants to reach out to them, he must worry about experiment effects. Another more defensive type of control is theoretical, the construction of elaborate systems of classification and careful distinctions in the style of Talcott Parsons. A person in command of Parsons's theory can assign every event to its proper category and thereby understand it.

Having made a distinction between feminine and masculine science, let us be suspicious of it, as Whitehead advises. It is too neat and simple. The feminist writers themselves warn that they are not claiming that all or most men inevitably practice masculine science and all women, feminine science. There are large differences among both men and women scientists, and the association of gender with type of science is a very partial, empirical one. Gender has something to do with it. Nor do they
claim that "masculine" science is bad and "feminine" science is good. Social science can be both an instrument of domination and control and a means of communion and self-transcendence. Gilligan writes of "the integrity of two disparate modes of experience that are in the end connected" (1982, p. 174).

In addition, they assert that the two kinds of science are blended, except in extreme cases. Gilligan writes of "the interplay of these voices within each sex" (1982, p. 2). She emphasizes that separation is only a stage in male development, a stage that is often followed by a return to the other and an empathetic appreciation of the other as subject. The masculine need to dominate and control can stay in the background except in cases of anxiety about competence. Conversely, girls can go through a stage of separation and difference in later life also. Bem (1974) has long asserted that feminine and masculine are separate dimensions, both present in everyone in varying degrees and combinations. Consequently, feminists would probably assert that the approach of the present chapter is itself masculine: it begins automatically with men, then turns to "the other half"—a separate but equal pretense. The idea of a purely feminine other half is a projection men make; women have as many sides to them as men do. Men and women are not that different.

Other feminist writers call for a revision of the connected-separate scheme. Belenky et al. (1986) ignore the first three years of life and study women's development from childhood to old age. They assert that in their cases connected knowing, when it occurs at all, is an achievement of adulthood or even late maturity. It appears sooner in families where the mother has independent views and expresses them in the family, where the relatively nonauthoritarian father listens to wife and even children, and where both parents can grow with their children. It does not appear until late in life, if at all, in cases of inadequate, authoritarian, hostile maternal and paternal care. For some women, motherhood is a challenge that develops more connected knowing in later life.

Westkott (1986) asserts that in at least one case, connected thinking was a result of the little girl's powerlessness. She was required to obey her father and take care emotionally of her mother, so she learned to be sensitive to her mother's feelings. This case suggests that separate thinking in science could express the power of the scientist over his subject matter; connected thinking, caring, and accepting, could express subordination. Hare-Mustin and Marecek agree: "Typically, those in power advocate rules, discipline, control, and rationality, while those without power espouse relatedness and compassion" (1988, p. 459). But Belenky et al. disagree; they argue that true connected thinking is appropriate for women's work, promoting human development, and does not express subordination (1986, pp. 188–89).

Still other feminist writers reject the connected-separate scheme in whole or in part. Some call for a science that is neither masculine nor feminine, and argue that the masculine-feminine distinction is itself sexist. Harding (1986, chap. 7) observes that African men and women are also raised by mothers, yet do not fit the Gilligan scheme; nor are all Africans alike. There are many kinds of men—northern and southern Italians, northern and southern Germans, etcetera—and many kinds of women, as the diversity of feminist thought attests. Spence rejects Bem's two-dimensional scheme and argues that a rapidly expanding body of data points to an incredibly complicated multidimensionality. There are many "masculinities" and "femininities" and many combinations (1981, pp. 144–47). Harding (1986, chap. 10) concludes that the current diversity and confusion of feminist thought is appropriate at this stage, since things are changing.

But let us resist the temptation Harding sets for us. Let us first learn what we can from the first group of feminist writers cited. Later we can return to enjoy the full complexity of feminist thought, one strand at a time.

What does the revised scheme tell us about social science? If we each have "masculine" and "feminine" sides, and if these sides also have many variations, then the issue is not two kinds of social science, but the interplay of two aspects of science. It may be that one aspect is normally dominant and the other subordinate, and in that case we could speak of "masculine" and "feminine" science; but we must remember that the other aspect is usually available as well. We need not call the two aspects masculine and feminine, if that sounds too sexist to some feminist writers. Nor need we insist on their presumed childhood origin, if early childhood is too psychoanalytic a topic for some readers. The two aspects of science in their many variations are obviously present all around us, wherever we look. Whether power is involved is another issue.

A masculine science is predominantly self-centered and externalizing, while a feminine science is predominantly other-centered and internalizing, in Roy Schafer's sense (1968). A self-centered scientist thinks his own thoughts, makes his own observations of fact, and has his own feelings; that is, he organizes his own experience. Then he puts or pro-
jects his thoughts and facts on to the external world in order to understand it. He treats the data he has created with his experimental or recording apparatus as news from the external world and applies his categories and theories to them to organize and adjust them. He also checks his ideas and hypotheses against his data for goodness of fit, hoping to eventually devise a theory that matches the data and therefore reveals the laws or structure of the world.

Similarly, to understand the work of another scientist he applies his categories and concepts to it; insofar as they do not fit, the work is objectively unclear or confused. He also applies his data to the hypotheses he has projected into the other’s work; insofar as they do not match, the hypotheses are false. He treats his subsequent feelings of scorn as a response to objective characteristics of the work.

An other-centered scientist does not make so sharp a separation between herself and the people she is studying. Social science is social, and in social situations we share feelings and thoughts and experiences easily and naturally. But science tries to go beyond such everyday sharing to reach the more hidden thoughts and feelings of the other, to weaken the barriers of culture, social class, personality, history, even gender, that muffle and distort our communication. Insofar as we can reach others, we join them and become part of their world. We think their thoughts, share their feelings, take their values and self-presentations as our own, play the roles they provide for us. That is internalization. If we are successful, they will respond; otherwise they will correct us.

In this sort of science, data come from the other, from interviews and observed daily life and free associations and documents. But insofar as we have become part of the other, our reactions and experiences also become data, though imperfectly and unreliably.

Similarly, to understand the work of another scientist, an other-centered person tries to find her way into the world of that scientist, to think and feel and value like the other. However, this cannot be done intuitively and empathetically, since the work does not respond or correct us or cue us; instead, it involves intellectual, hermeneutic techniques.

In the present volume, the philosophies of chapter 1 and 2 emphasize externalization, and the philosophies of chapters 4 and 5 emphasize forms of internalization.

For an example of extreme externalization, consider Grünbaum’s *Foundations of Psychoanalysis* (1984). Grünbaum has two logical schemes which he imposes on various writings to interpret and evaluate them.

Material that can be so interpreted he declares to be scientific; material that does not fit is incoherent, confused, naive, or “whatever that is,” or is ignored. One scheme is the logic of science; this consists of testing causal hypotheses. We test a causal hypothesis by looking for evidence that confirms it and also disconfirms a plausible alternative hypothesis. If a critic can state a plausible alternative hypothesis that was not disconfirmed, the test is inconclusive. Scientists necessarily test causal hypotheses if they are scientists; whatever else they may do is of no particular interest and is classified as heuristics, confusion, or pseudo-science. To understand a theory, then, one must find the causal hypotheses it asserts and the tests they have undergone.

Grünbaum’s second scheme is the logic of intentionality, which he takes from Kurt Baier, an OL philosopher. He applies this scheme to writings that claim to discuss intentional action, with the same results.

He also externalizes in discussions of his own work. In a 1986 symposium involving thirty-eight commentators on his *Foundations of Psychoanalysis*, Grünbaum’s awareness is limited entirely to his own ideas. He shows no interest in or awareness of any of the highly diverse and highly interesting lines of thought presented; his comments consist entirely of defenses of his 1984 work. He is quite clear about his own argument and quick to recognize any distortion in it, but quite muddy about opponents’ views, when he mentions them at all. In other words he treats the discussion as an attack and defense of his ideas.

Grünbaum’s work also illustrates the positive contribution a consistently externalizing person can make to science. He projects his logic-of-science scheme onto Freud’s writings consistently and clearly and thereby brings out that aspect of Freud’s thought: Freud, the logical empiricist. Freud did in part think of his work as an exploratory neurology, a natural science that tries to discover the laws of the nervous system. He had other thoughts too, but Grünbaum is not attracted and distracted by them; he sees only what fits his scheme. The result is a clear demonstration that insofar as Freud thought he was conclusively testing a fundamental law of the mind, he was mistaken. Grünbaum’s argument supports Habermas’s claim that Freud’s hypothesis-testing stance was a self-misunderstanding 1971, chap. 11). That is not what Freud was doing. A Lakatos or Stegmüller or Gadamer projection would give a different picture of what Freud was doing, and expand our understanding of Freud.

For internalization, I think of Clyde Kluckhohn, of whom his wife asserted, “He acts very differently when he’s down there with the Na-
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does not enable one to penetrate beyond their self-understanding and self-deception. For that, one needs to take some distance, skeptically, and project some theoretical scheme onto the material to see what order it imposes or exposes, and then test it by returning and trying to act according to the projected scheme. But this sort of skepticism requires a self separate from the other, a self into which one can withdraw and think one's own thoughts.

Another difficulty with excessive internalization is that the others can recruit the too-gullible scientist for their own purposes. Ned Polsky provides an example in his study of hustlers, beats, and others (1967). One shady character whom he had befriended suggested casually that Polsky might have a storage space for a gun he wouldn't be needing and didn't want to have to carry around all the time. More innocently, members of the Fox Project (Gearing, 1960) found themselves drawn into the political and economic problems of the Fox Indians and participated in working out solutions, until some of us found ourselves peddling Indian paint kits to hobby stores. (I couldn't sell any.) Redfield (1960, p. 82) tells of the ethnographer who became so absorbed in the intricacies of Zuni ceremonial that he became a Zuni priest and stopped writing ethnography.

To avoid such weaknesses, each social science method should have its own blend of externalizing and internalizing processes. Participant observation, the most thoroughly internalizing method, should be a dialectic of attachment and detachment—joining in and then withdrawing to reflect and reorganize, alternating credulity and skepticism, appreciating particular features and then generalizing by comparison with other cases (Redfield, 1960). Havens (1986) prescribes the same balance for client-centered therapy: "The language of empathy moves the therapist into the patient's space. . . . It is not the quintessential experience, however, because empathy aims at merger or identification rather than a working distance" (p. 85). "Empathy is credulity operationalized: the goal is to be 'taken in' and in the process to locate another. Interpersonal statements are skepticism operationalized: the idea is not to be taken in, not to allow the patient to settle assumptions or projections upon the therapist" (p. 91).

One could also add the reverse: the patient should not be taken in either, should not be induced to introject the analyst's diagnosis. Maintaining a working distance leaves room for the patient's skepticism, and allows the patient to withdraw and think his own thoughts.

Vahos. I think of Robert Rietz, a thoroughly nonmilitary person to me, who when visiting the Fox Indians became a war veteran, a proud, patriotic legionnaire with helmet and uniform, because that was how his Indian friends saw him, for their own masculinity-affirming purposes. I think of Erving Goffman's Presentation of Self in Everyday Life (1959), based on a year's stay with Scottish islanders. Goffman describes life as his hosts saw it, from the inside of the home outward: the back rooms where the self is prepared and repaired, the front room or stage where the self is presented, and precarious encounters in the outside world. This book puzzled me for many years until, having met Goffman and watched him blend smoothly into the scene, I realized that s/he was describing primarily the presentation of the feminine Scottish self in this very family-centered society. How naive of me to have assumed that self meant masculine self!

Havens (1986, pp. 190–91) provides another example from case material by H. F. Searles. "Searles . . . describes having felt almost moribund while working with one patient. He first attributed this to fatigue, since he had been working hard, but said that gradually 'transference material emerged with made it clear he was reacting to me variously as his chronically depressed mother, and as a long-senile grandmother who had lived nearby, largely as a vegetable, during a considerable portion of his developmental years.' " Searles had simply taken the patient's cues without knowing it and had empathetically played the role assigned him—that of depressed vegetable.

Both externalizing and internalizing processes need to be corrected or supplemented at their weak points by the opposite process. An externalizing process, projecting a theoretical scheme onto a complex mass of data—say, a game model onto a diplomatic case history—can bring great clarity and order if the scheme is appropriate to the data. Insofar as it is inappropriate, it distorts or simply loses what is there. But to see whether it is appropriate, the modeler must get outside himself and his model, either by empathetically immersing himself in the case history or by hermeneutically studying some different treatment of the same material. Alternatively, the model builder could begin by moving into the case history, becoming other-centered, and seeing what models the actors in the case suggest to her. Then she can withdraw into herself, work out the model, and project it back into the case.

Internalizing the culture of the people one is studying enables one to participate and to give a vivid report of how they see themselves, but
The various kinds of interviewing and sample surveys range from semiclinical depth interviews, which yield small case histories, to voting surveys which yield essentially aggregate data; but they all maintain their own distinctive balance between research-centered and respondent-centered requirements. In ordinary attitude surveys, the balance is most nearly equal and the conflict the sharpest. A good interviewer must (1) phrase the questions in the respondent's language, empathetically catch the respondent's meanings, pursue the respondent's hinted qualifications and doubts and clarifications; but also (2) standardize the questions, control and standardize the interaction, firmly preserve the original variables that are being measured. The interviewer must (1) resemble the respondent in dress, speech, race, to set the respondent at ease; but (2) maintain role distance to avoid influencing the respondent. In depth interviewing, the standardization requirements nearly disappear, but are still present in the selection of respondents, the list of topics to be brought up at some appropriate time, and the controlled respondent-interviewer interaction. In voting surveys the respondent-centered requirements are weakest; they consist mainly of very careful phrasing of the standard questions ("Did you happen to vote . . . "). Here the empathy and sensitivity occurs mostly in the pilot test, where the interviewer tries out phrasings to see how they sound to the respondents. Once the right responses have been elicited, the questions and the nonverbal communication context are mostly locked into place for the actual survey. However, it is also desirable to select empathetic, nonthreatening interviewers who can notice and quietly manage the nonverbal context of the interview, smoothing the way for the respondent.

Oakley (in Roberts, 1981, chap. 2) describes her depth interviewing as an unstable, conflict-soaked experience. The methods textbooks she tried to follow pulled her toward a more impersonal, standardized relationship, while her respondents pulled her toward deeper personal involvement. For instance, they asked her questions, expressed anxiety and a need for information and support that she could provide. Those were not good methods texts that she cites; they were still dominated by the logical empiricist caricature of science. The Belenky et al. (1986) depth interviews show a better balance between standardization and rapport: standard questions, including some from earlier studies for comparison; free discussion of the answers and deep involvement in each case; quantitative treatment of some recurring variables; and qualitative study of the cases as wholes.

Experimentation is usually a primarily externalizing method, with its emphasis on the experimenter's hypotheses and variables and its concern with control of other variables. Yet the E must still be concerned with how the controls feel to the S and how the independent variable will be interpreted by the S. If experimenters mistakenly think they are in perfect control, as Clark Hull did, they will fail to see how their controls induce a response set in the S that biases the results (Bruner, 1973, pp. 137–39). The good E will realize the importance of an other-centered concern with how the experiment looks to the S. Experimenters might deal with this concern by taking a turn as S in pretesting. Or they may get absorbed during testing with "the experiment as a social occasion"—the tacit interpersonal exchanges that cluster around the official, impersonal process and affect the data. Even a formal modeler, say, one working out a model of optimal deterrence under uncertainty, may get absorbed in a case study, imaginatively taking various diplomatic roles, to clarify some obscure deductive implications. Conversely, a political scientist constructing a case study can use a formal model, in the "detachment" phase of construction, to interpret some obscure diplomatic or congressional interaction or to suggest new interview questions.

Note that we have now required a double balance of the researcher. The researcher should first find the method whose internalizing-externalizing or attachment-detachment requirements most nearly fit his or her own personal tendencies. Then within the method s/he must again find the right balance. It is doubtful that many will succeed perfectly at this double task.

The feminist writers could comment that all this discussion of balance is very nice in the abstract; but as methods are actually practiced, the "masculine" components are usually too strong for optimum results. They could also comment that research occurs in the "private" arena where some researchers can allow their "feminine" side to come out quietly. But little of this side appears in the written reports; and the public arena of publications and convention papers is still stridently and aggressively masculine.

**COGNITIVE STYLES**

Warning: do not get so fixated on the gender issue that you project it everywhere in social science. The externalization–internalization dimension is only one of several dimensions of cognitive style. In this section
we briefly survey other dimensions to sketch a broader picture. Or rather, we survey other ways of conceptualizing the same material. Did you think psychologists of science agreed on the dimensions of cognitive style?

The term cognitive style refers to the characteristic ways in which individuals conceptually organize their environment (Goldstein and Blackman, 1978, p. 2). A masculine way of putting it.

One of the first styles to be studied systematically is the authoritarian or dogmatic personality (Adorno et al., 1950; Rokeach, 1960; Goldstein and Blackman, 1978, chaps. 1–2). Its main characteristics are (1) intolerance of ambiguity and (2) closed thinking—that is, rejection of ideas that differ from one's own. The dogmatic thinker has a set of clear and distinct categories, such as communism and democracy or market and planning or facts and values, which he imposes on ordinary fluid situations. These categories cut through the surface confusion and deception to reveal the clear underlying essentials. Usually one of a pair is good and the other is bad, or one is true and the other is false. There is no middle ground. Ideas that differ from one's own are either a confused variant of one's own truth, or they are false and deserving of scorn.

A closely related line of thought is Maslow's discussion of the pathology of cognition (1966, chaps. 3–4). He suggests that anxiety and defensiveness in some area can induce a researcher to be too controlled, rigorous, precise, neat, orderly, quantified. The need for certainty or reassurance in some area can lead to premature generalizations, rigid maintenance of a hypothesis against seemingly negative evidence, denial of doubt or ambiguity. The needs to conform, to appear powerful, to fight authority, and so forth, can produce other pathologies of thought. In contrast, the healthy or creative scientist balances caution and boldness, control and looseness, sobriety and playfulness, precision and ambiguity. The healthy scientist has the right degree of self-esteem, doubt, respect for authority, rigor, reasonableness, persistence, and so on. What is the right degree? If you're healthy, you'll know.

The discussions cited above treat cognitive style as characteristic of immature, poor scientists. Healthy or mature scientists also differ in cognitive style (Maslow, 1966, chaps. 8–11). Mature people are not all the same, after all. On the long path toward maturity, we carry our past with us, converting weaknesses to strengths, extending or refining old coping mechanisms, transferring old ways of relating to new situations. Whether or not anyone ever fully matures, the result is cognitive style.

The best and most thorough treatment of cognitive styles in "good" science is Mitroff and Kilmann, Methodological Approaches to Social Science (1978). Mitroff and Kilmann construct a two-dimensional space, based on concepts of Carl Jung, and show how several other treatments of cognitive style can be reduced to the Jungian dimensions. Instead of rushing through an abstract account of the two dimensions, I shall present one slice through them. For a more complete discussion of the dimensions the reader should consult the original text, which is itself quite condensed and abstract.

The slice to be presented is taken from Hudson (1966). Hudson studied a group of schoolboys aged fifteen to seventeen and induced two opposite types which he called convergers and divergers. These are actually dimensions rather than points on a scale, since a person could be high or low on each one. Also, the score defining each one is multidimensional.

Convergers approach a problem by distinguishing its component parts and decomposing each part into subparts. Then they study each subpart separately, seeking clarity in the small in order to put together a larger clarity step by step. Their creativity consists in being able to disassemble a complex, messy situation into clearly distinguishable parts and in avoiding distracting side issues and slippery analogies while analyzing a part. They prefer to collect impersonal, precise data about the part they are studying, and to draw implications from these data a step at a time. The preferred logic is mathematical rather than dialectical.

This style is most effective with well-defined problems, so convergers select such problems if possible and avoid vague, ill-defined situations. They are also uneasy with conflict and controversy, and either avoid studying such unclear topics or try to abstract a simplified version of them for study. One tactic is to construct a simpler version of some hopelessly complex situation and model that, then try to apply the model to the complex situation step by step, complicating it when differences appear. Emotional situations are particularly hopeless, and convergers prefer to assign such things to art rather than to science. Science, they feel, should deal with facts and laws, not feelings.

Divergers approach a problem by placing it in context, expecting the context to provide lines of study into the problem. For instance, they might try to understand a published article by reading previous articles by the same author so they can trace themes and concepts moving into the article. They also read the citations to find the sources of other ideas and to locate contrasts and controversies. This enables them to understand the article in contrast or opposition. Having located the controversy or
issue that is the setting for this article, they study that by finding its context, the traditions of thought that went into it, the lines of publication that come together in the current situation.

In addition to locating larger and larger contexts in this fashion, divergers also locate multiple contexts. A diverger studying an international diplomatic statement, for example, the 1981 U.S. cancellation of the embargo on agricultural exports to the Soviet Union, initiated in 1979 in response to the Soviet invasion of Afghanistan, will first locate the international diplomatic contexts—previous relations between the countries, relations to allies over time, potential alliances and realignments—since the diplomatic statement may have been directed to any and all of these contexts. Then s/he locates the domestic politics contexts: the bureaucratic alignments, since the proposed statement is a move by one or more departments against or toward other departments; the party positions, since the statement is a pre-election maneuver or an attempt to establish a record to run on later; and executive-congressional relations. There is also the personal history and personality of the diplomat, which helps one interpret the nuances of his statement—as the diplomats in the other country well know. Next is the international trade context, since the grain embargo and its lifting affects other grain exporters, who are U.S. allies; and this must fit into the context of the international and domestic economic situation, which in turn impinges on government fiscal and tax policy. Bringing in the economy also forces one to bring in the lobbyists, who emphasize parts of the economic situation to the administration and thence to the diplomats; and so on.

Divergers like to play these multiple contexts against each other, since each throws a different light on the diplomatic statement. The result will often be to reveal ambiguities and dilemmas in what initially seemed to be a simple move. Thus divergers are skeptical of seemingly clear and simple situations, including clear and simple logic, and look for the complexities hidden beneath the surface simplicity. They feel more at home in the obviously complex and ambiguous situations that convergers shun. Also unlike convergers, divergers accept and emphasize the emotional aspects of reality as integral to it; the diplomatic exchange is a response to, and expression of, hostilities, suspicions, hopes, illusions, loyalties, and despair (the U.S. farmers, who went broke in 1980).

Students don't like divergent teaching, because it leaves them floundering, not knowing what the right answer is.

How would a converger deal with the U.S. cancellation of the Soviet grain embargo? If he was unable to escape the assignment, the converger would begin by breaking the problem into its basic parts and then proceed a step at a time. The U.S. action was partly domestic and partly international; let us begin with the international aspect. The two main countries involved were the United States and the Soviet Union; others can be brought in later. The U.S. move, cancellation, was either a response to some Soviet move or a new move. Search for previous Soviet moves and U.S. references to them. If none, the U.S. move either anticipated some Soviet response or it did not—for instance, if the domestic situation was primary. Search for reports of anticipated Soviet responses. Search also for reactions to the actual Soviet response, to see whether it was expected or not. And so on. Any positive data at any of these steps require further analysis and further data to clarify each substep.

Mitroff and Kilmann emphasize that we should not assign individual scientists to particular types or points on the two dimensions. People are mixtures of various things, as we observed in the masculine-feminine context. Thus a political scientist might feel divergent when constructing a case study of a diplomatic exchange, and shift to convergent when constructing a mathematical model of the same case piece by piece. The two styles can interact, as pieces of the model clarify the influence of some context, and as plural contexts demand complication of the model. Other researchers might be neither especially convergent nor divergent, but have other cognitive styles altogether.

However, some researchers may have developed one side of an opposition much more than the other, and their style would include rejection of research problems that require the other cognitive process. For example, a person might cultivate empathetic processes so much that problems requiring impersonal, formal reasoning, mathematical thought, become puzzling and insoluble. Such people, like the extreme convergers and divergers, will limit themselves to certain problems and concepts. They will also have difficulty perceiving the opposite components of problems they do study—for example, mathematical components of interpersonal relationships.

Another dimension that is hidden away in the Mitroff and Kilmann space is the ability to perceive movement or process (call it M). A person high on M will, for example, perceive a theory developing over time in a series of publications, or perceive the gradually increasing mobility of world capital after 1950 in balance-of-payments tables. The impetus to
the development of theory will appear in the early publications as a pregnant metaphor or concept or model that must expand its range of applicability and work out its details; or a tension that pushes toward resolution through change of both poles. The impetus to the increased mobility of capital comes from familiar demand-supply contradictions: a postwar surplus of U.S. capital goods and a shortage of European capital goods, which turned of itself into a surplus pressing for outlet, accompanied by rising labor costs and shifts of finance capital, and so on.

The series of publications or trade tables are cross sections through the movement that reveal fluctuations and turning points; but the movement itself is already implicit in the tensions and contradictions of the initial situation.

A person low on M will be good at seeing distinctions and making classifications. Since everything is what it is and not something else, to perceive something clearly and distinctly is to perceive its difference from its context and from other things. Most differences are trivial, but the task of science is to find the important or relevant differences. The result is classes and categories and types: theoretical practice and practical practice, demand side and supply side, Chicken and Prisoner’s Dilemma, firstness, secondness, and thirdness, high and low M. After that, one can establish causal connections, or stages in a sequence, or the essential structure or specificity of each type.

For the low-M thinker, a theory is a set of categories, concepts, and propositions, while for the high-M thinker it is a history. Thus for one person Keynesian theory is the doctrine appearing in the General Theory, perhaps as stated more carefully and systematically by Klein or Samuelson; for another person Keynesian theory is the whole Keynesian tradition. Consider for example Grünbaum’s assertion (1984, chap. 7): if recent Freidians hold the same theory that Freud did, his objections to that theory apply to them too. But if their theory is different from Freud’s, they are not Freidians. For a low-M thinker this statement is clear and correct, while for a high-M thinker it is ridiculous.

For a high-M thinker, the proper logic is dialectic, the logic of becoming; for a low M, logic is symbolic logic. Hermeneutic theorists like Gadamer are high M; logical empiricists like Ayer and Carnap are low M. The interplay of these two ways of thinking about logic has produced a continuing series of misunderstandings and ambiguities and fruitful ideas. Thus low-M Marxists like Engels and Kautsky have reduced the dialectic

to exactly three laws of thought, or three kinds of science, or in Althuser’s case have tried to remove it from the theory entirely.

The purpose of Mitroff and Kilmann’s typology or space is not to classify particular scientists, but rather “to help us to see and to organize some of the complex patterns by which humans behave” (1978, p. 11). That is, they intend that their book should help us understand the particular cognitive style of some social scientist we are studying, including ourselves. That is also the aim of the present chapter.

CONCLUSION

The study of personality influences, the subjective side of science, helps us recognize how much we shape and construct what passes for objective knowledge. It may also help us accept or devise practices that do not fit some narrow definition of scientific method, and accept more of the pluralism of methods that actually exists. And since method expresses personality, it may help us accept parts of ourselves that we may have thought were irrelevant or even hindrances to good practice. In particular, many social scientists would do well to accept and use their “feminine” side more, at least in research if not in the aggressive and competitive public arena. To this end the study of the feminist writers would be helpful.