CHAPTER 1

Toward a Theory of Scientific Organizations

Currently, the sociology of science is the fastest growing area in the sociology of knowledge. The relationship between the two has never been one of mutual support and recognition. Classical approaches in the sociology of knowledge generally held science to be exempt from sociological analysis. The Mannheimian approach to worldviews as weapons in the competitive struggles between social groups, the Marxist theory of ideology as "false consciousness," and phenomenological analyses of the mundane interpretive practices that actors engage in to construct everyday lifeworlds all maintained that there was a special rationality invested in science prohibiting sociological explanation. Mannheim claimed that the social substratum of science consisted of free-floating intellectuals who were not blinded by particular class interests; Marx was a firm believer in the objectivism of materialist science; and Schutz argued that the methodical pursuit of rational research freed science from the dogmatism of the natural attitude. The natural sciences particularly were assumed to follow neutral and objective methods for designing and testing theories that would eventually correspond to reality and were not influenced by external social factors. Even sociologists who stressed the historical peculiarity of modern Western science, such as Max Weber, argued that the rational core of science was nonsocial and that scientific knowledge could claim universal validity.

This privileged status of scientific knowledge reflects the sacred role science plays in the public discourse of modern society and culture. Ever since the Enlightenment equated science with societal progress and moral emancipation from tradition and superstition, science has come to be viewed as the paradigm for all rational practice. Science has replaced religion as the most authoritative

worldview, but shares with religion a charismatic remoteness from the profanities of everyday life and mundane reasoning. The label "scientific" lends special credibility and authority to knowledge claims and discursive practices, and so social groups try to mobilize science in support of their interests. Since the cultural capital invested in scientific knowledge is a highly valued commodity in the markets of public opinion and exchange, powerful social institutions, such as the professions, the state, political parties, or social movements, compete for the support of science as a way to gain legitimacy and acceptance. In a sense, science has become the religion of modernity; instrumental reason and technical objectivity are the core of its cult.

Realist epistemologies have systematized the worship of science as the model and metaphor for the sacred values of truth, impartiality, and objectivity. The eminently religious character of this worship is evident in the dualistic structure of epistemological realism. According to realism, there are "internal" and "external" domains in science (Hooker 1987:206ff.).[1] Realism intends to hold the internal and rational core of science exempt from sociological analysis. While the external domain consists of all those contingent social and psychological pressures that might influence variables such as researchers' choice of problems and institutional affiliation, the internal or rational domain of science is assumed to react to epistemic pressures only. That is, realist philosophies draw sharp distinctions between "internal" and "external" aspects of science, between "rational" and "social" factors, and between "contexts of discovery" and "contexts of justification." Traditional philosophy of science is a prescriptive model of how good science should operate, and good science reacts only to the epistemic pressures emanating from reality and logic. The distinctions between external and internal aspects of science resemble closely the distinctions religions draw between the sacred and the profane, and so I shall develop further Bloor's (1976) point that in worshipping the sacred side of science, philosophy of science actually worships our most fundamental and cherished social institutions.

As opposed to the traditional sociology of knowledge and philosophy of science, Mertonian sociology of science[2] does approach science as a mundane social institution with its own norms, reward and peer inspection systems, and institutionalized roles. However,

due to its normativist and functionalist biases, the Mertonian framework is largely insensitive to the actual internal workings of science. To some extent, the Mertonian paradigm still subscribes to the philosophical distinctions between internal and external aspects of scientific production and concerns itself mostly with those normative pressures operating in scientific communities that institutionalize the extension of certified knowledge and keep the deviance arising from conflict and competition at bay through social control. The Mertonians have primarily been concerned with the behavioral and institutional aspects of *scientists*, not so much with the contents and everyday operation of *science*. In this sense, the Mertonian paradigm still holds the core of science exempt from sociological inspection (King 1971:15).[3]

The most innovative and stimulating research in the sociology of science has been done by the constructivist "sociology of scientific knowledge."[4] Two classical studies have carved out the central problematic of this approach: Ludwik Fleck's *Genesis and Development of a Scientific Fact* (1935/1979), and Thomas Kuhn's *The Structure of Scientific Revolutions* (1970).[5] The sociology of scientific knowledge addresses the actual internal workings and contents of science. It disenchants the quasi-religious illusions about rational science that traditional epistemology and, to a lesser extent, the Mertonian school have been cultivating for so long. The new sociology of science abandons the internal/external distinctions and approaches science as an ordinary social field where interacting and negotiating scientists construct natural reality in a surprisingly mundane fashion. The sociology of scientific knowledge opens the black box of scientific rationality and inspects the actual internal dynamics of science-in-the-making. Nothing mysterious and culturally unique is found to happen in science once Pandora's box is opened.

For the most part, the sociology of scientific knowledge has followed the interpretive and microinteractionist turn sociology underwent in the late sixties and seventies. Most notably, ethnomethodological analyses of interactions-in-contexts are frequently called upon to reconstruct the microdynamics of everyday scientific production. Social studies of science reveal that the ethnomethodological modes of producing social order are congruent with the modes of producing natural order. In fact, social and

natural order coemerge. Following ethnomethodology, the sociology of scientific knowledge reveals that natural facticity, like social facticity, is socially accomplished and not given as an external reality that simply awaits "discovery." Facticity is always a cultural artifact. Consequently, the interactions and negotiations between scientists are shown to resemble very closely the practices of accounting for reality studied by ethnomethodologists in mundane settings. In fact, scientists at the workplace look more like Garfinkelian sense-makers than Parsonsian, Popperian, or Lakatosian rule-followers.

The influence of interpretive microsociology on social studies of science is also visible in the case study approach to science. Sociologists of scientific knowledge go into the laboratory as participant observers; they conduct in-depth interviews with scientists about incidents of controversial science; they analyze the indexical discursive practices scientists engage in to account for their actions; and they follow scientists in their attempts to enroll other agents in order to create strong networks of support. As a result, the evidence gathered by microsocial studies of science is often of a rather unsystematic and impressionistic kind. Thick descriptions of individual events and settings have largely driven out comparative studies of science as an organization. However, this interpretive microfocus was probably called for to overcome the excessive institutionalism of the Mertonian school and to start addressing the internal workings of science-in-the-making.

This innovative approach to science explains why the constructivist agenda receives prominent attention in this book. I shall draw rather extensively on this school in my discussions of the microdynamics of scientific construction because this is where the great strength of constructivism lies. Constructivism casts a light on the everyday processes of manufacturing scientific knowledge and hence is indispensable for any analysis claiming to uncover the internal workings and actual contents of science. With the constructivists I believe that realist philosophy misrepresents the actual reality of science, and like the constructivists I hold that science is "socially constructed."[6]

However, the constructivist agenda suffers from several severe shortcomings. Partly due to its orientation to interpretive microsociology, constructivism shares the astructural biases of actor-

centered approaches to social structure (Hagendijk 1990).[7] The "sociological" is too often reduced to interactions between people. The overriding concern with the interpretive microproduction of natural reality has forgotten the structural and organizational conditions of scientific production. The focus on interactions, conversations, and texts has led the sociology of scientific knowledge to paint an overtly discursive and idealistic picture of science. Science is presented as a form of mundane sense making that employs ordinary interactive and conversational devices to manufacture socially accepted knowledge claims. What is left out in this rather harmonious and cozy account are the material and structural contexts of scientific production, such as the distribution of intellectual property, the level of concentration in the physical means of intellectual work, stratification, or disciplinary variations in the ways of doing research.[8]

The micro-macro problem reoccurs in social studies of science as their inability or unwillingness to offer a social theory of science. It turns out that social studies of science are not all that "social" and remain much closer to realist philosophy than its proponents realize. The philosophical "science is rational" is often simply replaced by the sociological "science is social."[9] Most case studies of individual events or laboratories have a purely descriptive and narrative status. Lacking a comparative perspective on the contexts of scientific production, social studies of science fail to lead up to an explanation of how science is socially constructed, and why this construction differs between, say, high-energy physics and social theory. In social studies of science, nothing can be explained because nothing is allowed to vary, or because interpretive descriptivism creates hypervariability without a common metric that would allow for comparisons. As a result, many case studies of science exhaust themselves in dogmatic reiterations of commonplaces: that realist epistemology misses actual scientific practice, that social factors must be taken into account, and that science is socially produced. The sociology of scientific knowledge is still preoccupied with epistemological critique and, more recently, with its own textual practices. This has prevented the field from developing its own Strong Program, that is, a social theory of scientific knowledge. To show that science is socially constructed is not the result but the beginning of any strong sociology of science, for the impor-

tant task remains to explain why construction varies from science to science and over time.

The complete absence of any theoretical and comparative apparatus is also responsible for the unsplendid isolation of the field. Ironically, in pursuing the sociological dimensions of scientific work, the sociology of scientific knowledge has moved further and further away from the main body of sociology and, even more distressing, from the sociology of knowledge. The real test of any sociology of science that claims nothing special is happening in science and that science is ordinary social activity must be whether or not it is able to relate its findings to a general sociology of knowledge. But the present isolation of the field implicitly reinforces the special and extraordinary status of scientific knowledge celebrated by orthodox realism. A strong and truly social theory of science, however, regards science as a particular case of knowledge production in general and offers an explanation for the differences and similarities between mundane and scientific reasoning. Although the elaboration of such a general theory is beyond the scope of the present book, I hope to make some suggestions as to how such a theory might look.

The constructivist agenda, then, has opened up a sociological way of analyzing science-in-the-making, but has failed to develop a general social theory of science. I shall draw upon constructivism in my treatment of the microproduction of natural reality in chapters 2 and 3, but will go beyond constructivism in my attempt to design a comparative and explanatory theory of scientific production. It will be shown that constructivism does not describe the nature of science *per se*, but one form of scientific production. Constructivism can then be explained as a special case of the theory of scientific production.

There is, however, one brand of constructivism that is not widely received among mainstream constructivists, and which comes much closer to the theory developed here. This constructivism also insists that scientific knowledge is socially constructed, but it manages to avoid the philosophical pitfalls of relativism and the sociological shortcomings of interpretive idealism. Instead, "social construction" is seen more in Durkheimian and materialistic terms, which reveal science as a conflictual and stratified struggle over organizational and symbolic property (see Restivo and

Collins 1982; Collins and Restivo 1983a, 1983b). This more structural kind of constructivism takes the social-construction metaphor of scientific knowledge not to mean that all is interpretation, and that all interpretation is relative and contingent, but rather that science is much like politics and social conflict. This view is more aware of the organizational and material conditions of scientific work, and hence has guided the elaboration of my own theory.[10]

This theory, which I shall call the "theory of scientific organizations," is based upon two major traditions in social thought: the neo-Durkheimian sociology of groups and group cognitions, and the technological paradigm in organizational research. Science is approached as a particular work organization whose technologies and social structures determine the ways in which groups of scientists do their research. I believe this strategy will overcome the current isolation of the sociology of scientific knowledge and will prove useful in linking the sociology of science to the sociology of knowledge. At the same time, this theory will place the micro-production of natural reality in the wider material and organizational contexts of scientific communities, and so will attempt to bridge the micro-macro gap that separates the Mertonian paradigm from social studies of science.

There are two important works that have begun to develop an organizational theory of science, and that have influenced my own thinking a great deal: Randall Collins's *Conflict Sociology* (1975:470–523), and Richard Whitley's *The Intellectual and Social Organization of the Sciences* (1984). While both works go a long way toward developing such a theory, I believe both fall short of fully realizing its potential for explaining the cognitive *contents* of science. Despite these important efforts, there still is a widespread belief that explaining matters of content is the privilege and exclusive domain of interpretive microstudies. The equations between macro = institutional norms and micro = contents of science are misleading. Among other things, the theory of scientific organizations can explain why some fields produce solid facts while others engage in informal conversation. It explains why some sciences look more like literature, and why certain fields are rather cumulative and mature while others are very self-critical and discursive. This theory also explains why some fields constantly

inspect their classical origins and philosophical foundations while others move self-assuredly on the path of increasing knowledge. It offers an explanation of why some sciences alternate between normal and revolutionary phases whereas others produce constant yet invisible innovations. The theory of scientific organizations can explain why some fields are very reflexive and pluralistic while others engage in a more empiricist style. And it explains this in just the same way as it would explain differences in the cognitive styles of mundane social groups and organizations. Just as in science, there are more "relativistic" social groups and organizations, such as universities, that allow for more internal debate and openness, and there are more "empiricist" groups and organizations, such as welfare bureaucracies, that are very prejudiced and dogmatic. I believe the strength of the theory of scientific organizations lies in this ability to explain a wide variety of mundane and scientific cognitive styles with the same conceptual apparatus.

To be sure, the term "contents of science" is understood here in the more general sense of "cognitive styles" and "discursive practices." I do not claim to be able to explain why scientists X and Y prefer theory A over B at laboratory Z in the year of C. And I am not sure whether any sociology of science has successfully explained matters of content in just this very detailed way. But the theory of scientific organizations does explain variations in the cognitive styles or mental habits of scientific fields. It explains why some knowledge systems are more formalized and standardized than others, or why some scientists advocate a rather authoritarian and prejudiced view of their work while others subscribe to a more tolerant and relativistic *conscience collective*.

The theory of scientific organizations intends to integrate the constructivist microperspective on science with the Mertonian emphasis on the institutional mesocontexts of scientific production. The sociology of scientific knowledge is certainly justified in attacking the functionalist and normativist biases of the Mertonian school, and it is also correct in objecting that these biases conceal the contents and actual internal workings of science. But I think this critique has gone too far in abandoning *any* notion of community organization in science in favor of a radically situationist and actor-centered microposition. I shall try to recover the Merton-

ian perspective on systems of peer inspection and approval without losing sight of the internal dynamics of science-in-the-making. We are then able to explain the covariations between forms of community organization and cognitive styles in science.

Due to its comparative and theoretical orientation, the theory of scientific organizations can also detect some surprising similarities and differences between science and other professions. The proof of the frequent declarations about the ordinary and mundane character of science lies in the ability to explain what scientific workstyles have in common with those of other professions. Such questions cannot even be posed within a framework that exclusively deals with individual labs or episodes of controversial science. Thus, I shall analyze the patterns of stratification across various professions; a topic which, incidentally, microsocial studies of science completely ignore. The theory of scientific organizations also explains, for example, why the workstyles of hospital-based medical specialists are closer to those of research front scientists than to medical generalists with a solo practice, and why current deconstructionism equates science and sociology with literary criticism and rhetoric. From this perspective, we can address the issue of why gentlemen-amateur physics differs from modern physics, and why social theorists have more in common with poets than, say, with experimental small-group researchers.

The theory of scientific organizations also fulfills the "reflexivity postulate" advanced by the Strong Program in social studies of science. While the radical sociology of scientific knowledge, still captured and fascinated by philosophical problems, interprets "reflexivity" as the abstract possibility of knowledge, I take this postulate conservatively to mean that a sociology of science should include a sociology of sociology.[11] Hence, I shall apply the organizational framework to the current state of sociology and argue that its immature and discursive character is not due to its youth or complexity but its fragmented control structures. In particular, I suggest that the debates between interpretivism and positivism, or science and hermeneutics, are not really debates over methodology but over organizational politics.

I realize that "theory" in the way it is done here is not all that fashionable these days. The postpositivist and deconstructionist

movements have severely shaken our confidence in science, theory, and explanation. Literary criticism, hermeneutics, and rhetoric are the modest replacements for the grand occidental metanarratives of science and rationality. Hence, the theory of scientific organizations anticipates at least three major objections from constructivist practitioners. Since most sociologists of scientific knowledge subscribe to an interpretive or ethnographic case-study approach to science, there will be some criticism against theory *per se*. Interpretivism favors thick descriptions over general explanations, adopts members' perspectives rather than an outside observer's, and emphasizes local and historical uniqueness more than comparative analysis. Undoubtedly, such a perspective has its merits, for without detailed case studies we would have no access to the internal workings of science. However, without some kind of comparative yardstick, the sociology of scientific knowledge will end up adding case study upon case study, without any means of assessing their relative status and broader significance. The theory of scientific organizations does not, of course, intend to replace or even supersede interpretive case studies, but instead understands itself as a way of making sense of the huge amount of detailed findings that have already accumulated.

The second objection against the theory of scientific organizations comes from the recent network model of technoscientific artifacts.[12] This approach believes that a general sociological explanation of science is impossible or even undesirable. Instead, it recommends to follow scientists in their efforts to translate other agents' interests and enroll them in strong "actor-networks" of support. There is not a sociology on one side that explains a science on the other; there are only weak and strong associations made up of heterogeneous human and nonhuman forces. In this view, the sociologist should not try to explain science on the basis of some preexisting conceptual schema, such as social factors, that enter the game from outside. Rather, one should start with nothing and watch how technoscientific worlds are gradually built up through the work of the participating agents.

This is an intriguing argument, but I think it is premature to drop all attempts at a sociological explanation of science. In fact, the network perspective has developed its own generalizations, such as "enrollment" and "translation," that are now being ap-

plied to particular cases of science-building. The network concept itself is an eminently sociological one, and it would pose no conceptual difficulty whatever to admit nonhuman agents into such conventional sociological networks.[13] It seems to me that the dismissal of any sociological explanation of science is based on a narrow interpretation of the social which comprises only interactions between people. Due to their isolation from the main body of sociology and their interpretive microorientation, sociologists of scientific knowledge often have a very limited perception of the larger field. For example, the theory of scientific organizations not only includes interactions between people, but also such nonsocial determinants as technology, the material means of scientific production, levels of resource concentration, and the like. In any case, I think that the possibilities of a social explanation of science have by no means been exhausted, and it is therefore rather premature to drop such attempts altogether.

A third objection against the theory of scientific organizations will likely be raised by those practitioners who are very impressed by the postmodern deconstruction of Western metaphysics and its metanarratives of truth, presence, and rationality. In this view, the whole point of a sociology of scientific knowledge is that there is no such thing as an accurate representation of an external and objective reality. There is no meaningful distinction between the word and the world or between accounts and reality (Woolgar 1988a). Neither does natural science represent the reality of the physical world, or the sociology of scientific knowledge represent the actual reality of science. Hence, the attention shifts reflexively to the textual practices that create the appearance of such an independent reality. The sociology of scientific knowledge then turns into a special form of literary criticism that deconstructs its own narratives. The goal is to detect and avoid any rhetorical devices that create the dangerous illusion that the text is actually about something or, even worse, corresponds to some external reality. There is no such thing as a reality that could be represented by accurate descriptions, there are only texts that should reveal their own rhetorical tricks so as to allow for multiple readings and contingent interpretations. As a result, the "reflexive" branch in the sociology of scientific knowledge (Woolgar 1988b; Ashmore 1989) experiments with "New Literary Forms" such as play, dia-

logue, and irony that avoid any realist connotations and are structured multivocally to give voice to conflicting readings (Mulkay 1985). In this view, the worst thing to have is an empiricist monologue in which one author has the nerve to present her views as actually being about something other than itself.

I believe that deconstructionist reflexivity is yet one more dubious outcome of remaining overimpressed with epistemological problems such as truth, representation, and relativism.[14] Only if we remain fascinated with the philosophical problem of truth will our inability to solve it distract us from making points and statements about the world. Attention to the rhetorical and textual practices of science and sociology is very useful, but only when conducted as a discourse that is about something and has realist ambitions.[15] But if we deconstruct our own textual practices as not being about the *world*, we get stuck with our own *words* and become obsessed, just like realist epistemology, with the purity of our own discourse, instead of talking about science. Even novelists do not routinely deconstruct their own stories but do everything they can to draw readers into their narratives. I don't see any reason to be more fictional than fiction.

It seems to me that deconstructionists want to throw out the baby with the bathwater. I agree that there are no translocal foundations for knowledge, but practices simply don't need any foundations to be working well. The practice of parliamentary politics requires no foundation in a social contract, and the practice of procedural legislation needs no natural law doctrine. In the same way, science can do, and actually does, without epistemology. But to say that science needs and has no epistemological foundations does not imply that we should do literary criticism and textual deconstruction instead. If this is acceptable, then the rush away from realism and explanation into literary criticism and rhetorical deconstruction is itself not well founded. The antifoundationalist *pathos* impresses only those who naively believed that practices were indeed based on foundations. But physics proceeds largely unimpressed by deconstruction. Once we drop this belief in the necessity of foundations, deconstructionism is hardly more than a tempest in the teapot of pure philosophy.

What, then, is the epistemic status of my own account? From an abstract philosophical viewpoint, I do not claim any special

cognitive privileges for my analysis of science. From a philosophical, though not sociological,[16] perspective it is just another story about science that tries to muster all kinds of allies to present itself as a credible claim. I have chosen the empiricist monologue not because I believe in safe transcendental foundations for knowledge, but simply because I think it is a great way of making a point. As Latour (1988a) says, the problem of most texts, especially in our discipline, is that they are not believed enough, not that they are believed too much. Hence, I see no reason to deconstruct my own story through new literary devices that give voice to alternative interpretations and conflicting readings. *Others* will disagree and deconstruct and be reflexive anyway, so why should I do their job? In fact, it strikes me that the epistemic arrogance and omnipotence of reflexive and deconstructive accounts is even more offensive than the empiricist monologue. For the latter gives readers and critics at least a chance to speak for themselves, instead of silencing them by anticipating their responses in an allegedly multivocal discourse that is nevertheless constructed solipsistically.

CHAPTER OUTLINE

In the second chapter, I discern some common underlying themes in the sociology of scientific knowledge. Since the new field owes more to Kuhn and epistemological critique than to Marx, Mannheim, or Merton, I first deal with the philosophical background of social studies of science. Epistemological critique deconstructs the representational metaphor of true scientific knowledge corresponding to reality and, in this way, prepares a *sociological* analysis of science's internal workings. Realist epistemology believed that science reacted to epistemic pressures only, and showing that science reacts to *social* pressures precisely requires the critical conversion of epistemic into social pressures. This conversion is accomplished by epistemological critique.

Although epistemological critique was valuable in opening up a sociology of scientific knowledge, it now tends to stand in the way of a social theory of science. In particular, the endless debates on relativism and epistemic reflexivity have led the field to spend too much energy on irresolvable philosophical metaproblems. In fact, the recent experiments with New Literary Forms threaten to

transform the field into a special branch of deconstructionist literary criticism. Relativism and reflexivity are still treated as *philosophical* issues, but once we stop doing philosophy, relativism turns out to be a pseudoproblem, and reflexivity simply means that a sociology of science is self-exemplifying.

The third chapter is especially addressed to nonspecialists in the science field. It will review the main empirical findings produced by social studies of science and put them into a comprehensive framework. This framework draws on Latour's network theory of fact production. As opposed to other constructivist agendas, Latour's analysis lends itself to comparative theory, despite his own reservations against theory and explanation. I shall argue that most of the rather disorganized and fragmented research results in social studies of science can be understood as leading up to a theory of fact production. Scientists use material and symbolic resources to induce other scientists to accept their statements as premises for further research, which gradually turns statements into facts. The important difference between mundane and scientific knowledge is that the latter can draw upon more and more powerful material and symbolic resources that raise the costs for objecting to scientific statements. I believe this is the structural reason for the authoritative status of science: there is too much capital invested in science to successfully challenge it.

The fourth chapter moves toward a general theory of scientific organizations. Microsocial studies of science are typically narrative and nonexplanatory case studies of individual events in science. They fail to lead up to a general theory of scientific production because nothing is allowed to vary. Discursive practices, the local interactions between scientists, and the "social factors" that close controversies between conflicting scientific groups are all implicitly treated as constants. Ironically, the claim to have discovered the social "nature" of science echoes the philosopher's claim to have revealed the rational "nature" of science. But if scientific practices are treated as constants, nothing can be explained because nothing is allowed to vary. There is no *a priori* reason to assume that all science is the same and that scientific practices do not change over time.

Once we treat scientific practices as variables rather than constants, however, we can explain the findings by social studies of

science as resulting from the structural arrangements of scientific communities. The Mertonian mesofocus on patterns of community organization is recovered, but without its normativist and functionalist biases. The theory of scientific organizations explains some of the most significant findings in social studies of science, such as the idiosyncratic structure of local scientific production, the conversion of statements into facts, and the dynamics of scientific change. For each case it will be shown that constructivism does not describe the social nature of science *per se*, but one extreme pole of what should be treated as a continuum of scientific practice. That is, it is demonstrated how scientific practices covary with the social-structural arrangements of scientific communities.

To strengthen the case for a theory of scientific organizations, the fifth chapter reviews some classical studies in the technological tradition in organizational research. The theory of scientific organizations has the strong implication that the crucial organizational dynamics do not vary between scientific and nonscientific organizations, and this implication will be verified. Similar task technologies generate similar structures and practices, whether in science or other organizations. This chapter will also identify the crucial variables in all technological approaches to organization, and will suggest a comprehensive contingency model of organizational structure serving as the starting point for the theory of scientific organizations.

The sixth chapter puts the sciences into a comparative perspective. As a profession, science shares certain characteristics with other professions, such as medicine, law, and art. It will be shown how an organizational cum neo-Durkheimian theory of professional production explains the patterns of stratification in various professional fields. Such a theory also explains differences in the workstyles of various professions by underlying differences in the structural organization of professional groupings. In fact, this theory even explains why there is so much postmodern skepticism about the possibility of general theory in sociology. Postmodernism likens sociology and social theory to literature because these two professional work organizations have similarly loose and informal control structures, and they also institutionalize comparatively low levels of professional autonomy and resource concentration.

The seventh chapter introduces the fully developed theory of scientific organizations. This theory explains variations in the cognitive modes and workstyles between various scientific fields by underlying variations in the forms of organizational control over scientific production. This theory explains why some scientific fields are more scientific and mature than others, why some fields change more through innovation and cumulation rather than fragmentation or migration, and also explains why some scientific fields are organized more bureaucratically than others.

The final chapter applies the general theory of scientific organizations to one of the most persistent debates in sociology and social theory. This is the debate between "interpretive" and "normative" paradigms, between "qualitative" and "quantitative" research, or between science and hermeneutics. This debate is usually conceived of as an ontological and epistemological debate over the foundations of the social sciences, but I suggest that this debate is a conflict over organizational politics and structure. The interpretive paradigm, or hermeneutics, arises as a problem in organizations with low levels of reputational autonomy and professionalization of control. Hermeneutics is what we obtain if we democratize and decentralize the control structures of scientific work organizations. Such fields produce conversation rather than facts, they worry a lot about their metaphysical presuppositions, and they also engage in constant reinterpretations of their history and authoritative classical scriptures.

Unlike many works in the sociology of science, which are targeted more exclusively on a fairly narrow specialist audience, the present argument intends to bring the sociology of science back to the core concerns of our discipline. One of these core concerns has always been the relationship between social structure, material organization, and modes of cognition. Weber's comparative sociology of world religions, Marx's materialist theory of the mind, and Durkheim's investigations into social imagery are classical attempts at explaining thought sociologically. The theory of scientific organizations belongs more comfortably in this tradition than in the specialist sociology of scientific knowledge, which is generally more narrative, interpretive, and astructural. In this sense, the theory proposed here is naturally "cumulative." "Cumulation" does not mean the realist hope in gradually getting to know the

Truth, but the intention to identify core problems in a variety of areas, and addressing them with the same basic theoretical tools. In sociology, it is much more common to argue from a rather narrow perspective, and to make no persistent efforts at integrating one's own views with other approaches. This has contributed greatly to the current fragmentation in our field. The theory of scientific organizations suggests ways in which this fragmentation might be overcome. It does so not by rigidly prescribing one "central notion," but by offering a general model of knowledge production. Within this model, it is possible to see what such various fields as the sociology of knowledge, the sociology of professions, the sociology of science, organizational theory, and the sociology of sociology have in common.

The theory suggested here is really a social theory of knowledge, not just of science. Science is one special, if very powerful, case of knowledge production. Explaining science reveals the hegemonic alliance of power and knowledge (Aronowitz 1988:3–34; Fisher 1990), or how knowledge comes to be more-than-knowledge. As Hooker (1987:195–206) observes, science has become an extremely powerful force shaping every area of modern life with an authority that is largely unchallenged and justified by its allegedly superior rationality and objectivity. The sociology of science must question these dangerous illusions. They are dangerous because they allow power to hide behind knowledge and Truth. Science has disenchanted cosmology and religion only to enchant itself as the religion of pure and disinterested rationality. If we don't want to surrender to the "experts" and their science, it is essential that we understand the nature of science's power. This power is *social* in its origin, not epistemic. That is, it can be granted or withdrawn, for it is not based on the intrinsic superiority of science as cognition. This means that the differences between scientific and mundane knowledge are not epistemic but social. The power of science is the power of its organization and resources, not its method.

Underestimating the power of science is as dangerous as overestimating it, or attributing it to the pure forces of rationality. There is a strong tendency in some sociologies of science to reduce science to texts and rhetoric, and then to deconstruct the objectivist narratives of science. The enormous power of science as an

organization is left out in this cozy picture. And the allegedly critical attitude toward science underlying this approach is as harmless as deconstruction itself, for it falsely regards science as nothing more than a special form of writing. The authority of science is not simply grounded in its texts, but rather in its organization. The critique of science as power is not greatly advanced by criticizing science as text only.

The important consequence is that science deserves no more and no different *type* of respect than other powerful organizations. People are often willing to surrender to the experts because they are in awe of the privileged rationality of their science. So we accept the experts telling us what to eat, who we are, how we should live, and how to raise our children. Foucault calls this totalitarian regime of Reason "biopower," Adorno and Horkheimer call it "instrumental reason," Habermas calls it the "colonization of the lifeworld." Once we stop being too impressed by the experts and their science, and once we realize that their power is simply that of their organization, we can begin to loosen their tight grips on our lives.