Only recently have we begun to think of the discipline as an historically specific form. We have, since Mannheim (1936) and the beginnings of sociology of knowledge, recognized the possibility that knowledge may be constituted on the basis of ideological positions or other interests. We have also been aware of the ways that certain social structures, such as the research university or professionalism, have organized knowledge-production. But it was Foucault who first called attention to the discipline as a “system of control in the production of discourse” (Foucault 1972: 224) and to discipline as a larger set of strategies and techniques of control that have come to dominate much of modern life (Foucault 1978). To extend this Foucauldian analysis, GRIP (Group for Research into the Institutionalization and Professionalization of literary studies) held a conference on disciplinarity. The essays in this forum were developed from papers presented at “Disciplinarity: Formations, Rhetorics, Histories,” held at the University of Minnesota, April 20–23, 1989.1

1. The conference was the seventh annual meeting of GRIP. For other papers from the conference, see Messer-Davidow and Shumway (1990); and Richard D. Wolff, Stephen Resnik, and Jack Amariglio, “Division and Difference in the ‘Discipline’ of Economics” (1990).
Yet, as these essays testify, Foucauldian analysis is by no means the only approach available for the study of disciplinarity. Indeed, none of the essays here depends upon Foucauldian analysis, nor do they share a single conception of what it is that distinguishes disciplinarity. Just as they select different disciplines as cases, they also focus on different aspects of disciplinarity. Thus the essays presented here do not provide a unified or comprehensive treatment of the subject. What they do provide are diverse perspectives on the way modern disciplines control the organization and production of knowledge.

The etymology of “discipline,” rather than defining the term, reveals the historical proliferation of its meanings. The term “derives from an Indo-European root . . . for both the Greek pedagogic term didasko (teach) and the Latin (di)disco (learn); and disciplina itself already has in classical Latin the double sense of knowledge (knowledge-system) and power (discipline of the child, military discipline)” (Hoskin and Macve 1986: 107). In the English language, “discipline” was used in Chaucer’s time to refer to branches of knowledge, especially to medicine, law, and theology, the “higher faculties” of the new university. According to the Oxford English Dictionary, “discipline” pertained to the disciple or scholar, while “doctrine” was the property of the doctor or teacher. As a result, “discipline” has been associated with practice or exercise and “doctrine” with abstract theory. Given this opposition, we can see why “discipline” might have been chosen to describe the new science based on empirical methods and claiming objectivity. To call a field a “discipline” is to suggest that it is not dependent on mere doctrine and that its authority does not derive from the writings of an individual or a school, but rather from generally accepted methods and truths. “Discipline” also referred to the “rule” of monasteries and later to the methods of training used in armies and schools. The concatenation of these two senses suggests that to be trained in a branch of knowledge is to be disciplined and ultimately to attain discipline, which is believed to be the quality of self-mastery.

The various connotations of “discipline” have until recently been entirely positive; to call a branch of knowledge a discipline was to imply that it was rigorous and legitimate. The name did not reveal that knowledge was produced by regulating or controlling knowledge-producers, nor that the training of disciples produced the general acceptance of disciplinary methods and truths. The persistence of the term “discipline” has also masked the historical specificity of the organization and production of knowledge. The branches of knowledge themselves, as well as what “a branch of knowledge” even means, have changed radically since the classical era. Knowledge has long been divided into branches—for instance, the classical division of philosophy (logic, ethics, and physics), and the medieval trivium (grammar,
rhetoric, and dialectic) and quadrivium (arithmetic, geometry, astronomy, and music) (Kristeller 1951). The division of knowledge into these seven liberal arts was hegemonic before the thirteenth century and was perpetuated in the structure of the curricula of the late medieval universities. “The entire faculty structure, especially of the northern ecclesiastical university, was based on the belief that the seven liberal arts were the foundation of all higher knowledge. . . . Rudimentary and perfunctory as the teaching of many of these subjects had been, the subjects themselves were invested with a divine aura as providing the indispensable prerequisite to the study of theology, the summit of all knowledge” (Leff 1975 [1968]: 118). But if the faculty structure continued to be based on the seven liberal arts and “pious hopes” about them continued to be articulated, nevertheless by the thirteenth century they had become irrelevant to what was actually taught in the universities, a new learning dominated by dialectic and philosophy rather than logic, grammar, and rhetoric (ibid.: 118–19). In the medieval university, disciplines came to be identified with lists of books, so much so that music was taught at Oxford using Boethius’s *De re musica*, and even medicine was taught purely at the level of theory (Rashdall 1936). Since academic disciplines relied upon the method of scholastic disputation, different books were all that distinguished them from each other.

The Scholastic curriculum lasted well into the seventeenth century. The notebook of John Cole, an English student from the middle of that century, reveals how the curriculum divided the subjects that were taught on a model derived from Aquinas: “The disciplines which dispose man toward understanding of things are two-fold”: directive and objective. The former was art and the latter, science. The arts were concerned with “the ordering of activity, or the right conception of things to be made, whether these were liberal, involving solely the mind, for example, logic, the ‘making’ of demonstrations, or servile . . . which were . . . of two kinds: practical and fine, according as they were directed toward the production of the useful . . . or . . . the beautiful” (Costello 1958: 37–38). While the arts dealt with action, the sciences were concerned with knowledge. Cole lists two arts and four sciences. The arts included reasoning (or logic) and communication, which was divided into grammar, rhetoric, and poetry. The sciences were theology, jurisprudence, medicine, and philosophy. The last was subdivided into metaphysics, physics (or natural philosophy), mathematics, and ethics. Under mathematics are listed arithmetic, geometry, astronomy, music, and optics. Although physics and metaphysics were separate branches of philosophy, seventeenth-century physics, as taught at Cambridge, was heavily imbued with metaphysics. Observation and experiment were not excluded, but understanding
lay in knowing the first principles, not the particulars (ibid.: 83–84). Thus the name “natural philosophy” correctly designated the study of nature as a part of philosophy and, further, of a philosophy that was most fundamentally metaphysical.

The emergence of modern disciplines in the nineteenth century was made possible by the development of new institutions and practices in the seventeenth and eighteenth centuries. Perhaps the most important innovation was the formation of scientific societies. The founding of societies marked a break in the history of knowledge-division. The Royal Society and the Académie des Sciences did not devote themselves to one of the Scholastic divisions of knowledge, but to the study of all nature, which included not only the old category of physics (or natural philosophy), but mathematics as well. The investigation of this new domain was to be accomplished by a new method—not scholastic disputation but “experimental philosophy.” Its commitment to this method allowed the Académie des Sciences to exclude from membership Cartesians and Jesuits, who were not regarded as open-minded seekers after truth (Hunter 1981: 37; Hahn 1971: 15). The new science continued to be called “natural philosophy,” but, in distinguishing knowledge of nature from other kinds of knowledge, it established the possibility for future specialization. From the mid-seventeenth to the end of the eighteenth century, the sciences we now differentiate as physics, chemistry, and biology emerged as branches of natural philosophy. Already in the eighteenth century, the phenomenon of specialization was recognized by Scottish philosophers, such as Adam Smith, who welcomed the greater productivity it permitted (Higham 1979: 6). Modern disciplines, however, came into being only with the breakup of natural philosophy into independent natural sciences at the end of the eighteenth century. Moral philosophy broke up somewhat later into the social sciences. “The humanities” is a twentieth-century term of convenience for those disciplines excluded from the natural and social sciences. While modern philosophy was defined by what was removed from it in the creation of the sciences, the other modern humanities emerged first in the form of classical philology, which produced history, modern languages, and even art history as descendants.

In the seventeenth century, scientific activity in such organizations as the Royal Society and the Académie des Sciences was “institutionalized, but not professionalized” (Frank 1975 [1974]: 82). Thus it would be a mistake to read into these early institutions later social functions. Membership selection is a case in point. The societies excluded women, usually on the categorical grounds of sex, and generally did not draw members from those classes who did not have access to a liberal education. Thus, while the circumstances of gender and class
served as de facto restrictions on membership, the societies did not restrict membership by examination or other formal guarantees of competence. While the Académie des Sciences relied on informal measures of competence to exclude amateurs, the Royal Society included many members who were amateurs or enthusiasts (Stimson 1948). Moreover, in the seventeenth century the societies did not monopolize the practice of science within their countries (not all working scientists belonged to them), nor did they have control over science in the universities. Appointments to the universities remained political, and the new science was not particularly welcome there. The early learned societies also failed to provide research direction and support. While the Royal Society had ambitions to act as a research institution, it was never able to gain the endowment necessary to support its own research program, and its informal character interfered with efforts at collaborative research (Hunter 1981: 43). The Royal Society remained a means by which individuals working in relative isolation could exchange and disseminate their work. For all of these reasons, the early societies cannot be considered disciplinary organizations.

By the eighteenth century, the Académie des Sciences had assumed a place in the French governmental bureaucracy that allowed it to have increasing control over science in France, to determine, for example, who was a scientist. The more open character of British intellectual life meant that the Royal Society would never have such official control. But insofar as this society was able to consolidate a network, it could function as an intellectual gatekeeper (see, e.g., Mendelsohn 1977). This power was not used merely to eliminate charlatans or bad scientists, but also to exclude women. Previously, women had participated in the aristocratic traditions of learning that flourished around the Renaissance courts and later in the amateur traditions, the salons and country circles, field excursions and home laboratories, of England and France. But the “first legitimation of the new science,” Londa Schiebinger notes, “also coincides with the formal exclusion of women from science. With the founding of the academy system in Europe, a general pattern for women’s place in science begins to emerge: as the prestige of an activity increases, the participation of women in that activity decreases” (Schiebinger 1989: 20). The Royal Society, founded in 1662, did not admit a woman until 1945, and the Académie des Sciences, founded in 1666, did not admit women until 1979. Women fared no better in the universities, where they were not even “formally admitted . . . until the 1860s in Switzerland, the 1870s in England, the 1880s in France, and the 1900s in Germany” (Schiebinger 1987: 316).

2. See, for instance, Reynolds (1920); Kelso (1956); Lougee (1976); Labalme (1980).
The power of these institutions became international so that, “by the end of the eighteenth century, a network of academies stretching from Saint Petersburg to Dublin, Stockholm to Palermo had consolidated Europe’s intelligentsia in what one historian called a ‘unified Republic of Letters’” (Schiebinger 1989: 20). Access to the fine arts was similarly limited by the art academies. Under the circumstances, women could only become scientists, scholars, and artists by other means: in the eighteenth century, Caroline Herschel learned astronomy from her brother, and Angelica Kauffman and Elizabeth Vigée-Lebrun were instructed in painting by their fathers. Early institutions restricted the access of an entire sex.

Kauffman and Vigée-Lebrun were among the very few women to be elected, respectively, to the English and Parisian art academies, and they were considered exceptions because they worked in the “male” genres of history and portrait painting. Most eighteenth-century women, barred from training in the art academies, did not learn life drawing from nude models, run ateliers, and gain patronage (see Nochlin 1972). Those who received private instruction produced miniatures, illustrations, embroideries, and china-paintings. Thus the art academies did more than exclude women; they bifurcated “art” into credentialed/uncredentialed artists and high/decorative arts. In these ways, the societies and universities managed, by the nineteenth century, to monopolize the production and dissemination of knowledge.

The early societies initiated the development of the techniques and strategies that were necessary for the disciplinary organization of knowledge. An example is the use of publication as the primary means of authorizing new knowledge. As originally conceived, the Royal Society would meet regularly to witness the discovery of new knowledge by experiment, but these meetings declined steadily over the first twenty years of the Society’s existence (Hunter 1981: 42). In place of demonstrating experiments, the Society began to represent them, first in correspondence and then in one of the earliest scientific journals, *Philosophical Transactions* (Bazerman 1988). The function of publication, however, changed over time. At first, the *Transactions* served as a means by which the experiments performed before the members of the Society were recorded. Later, the journal became the means whereby the results of experiments and observations performed or recorded in private were presented to the public. As a result of this change, those who published the journal assumed a gatekeeper role, both because the integrity of scientific information obtained in private now became questionable and needed authorization and because they controlled the means to authorize and disseminate it. As the journal’s function changed, so did its audience. Originally, the readership
was wide and critics of scientific findings included nonscientists, such as nobles and clerics. During the eighteenth century, as membership in the Society became restricted to working scientists, the readership increasingly became limited to this group. Thus the innovations that were initiated in the seventeenth century prepared the way for the later development of modern disciplines.

However, other institutions were required to produce in the early nineteenth century what has been called a “‘second’ scientific revolution . . . marked by the eclipse of the generalized learned society and the rise of more specialized institutions, and by the concurrent establishment of professional standards for individual scientific disciplines” (Hahn 1971: 275). The social transformation that made these new disciplines possible was the development of new educational institutions and new forms of scientific education. The research universities in Germany and the Grandes Ecoles in France not only gave steady employment and financial security to scientists, but also encouraged them to identify with others of their speciality, rather than with scientists as a group. These events were delayed in the United States until the late nineteenth century when research universities, derived from the German model, replaced the old colleges as the dominant form of higher education (see Geiger 1986; Veysey 1965). Research universities both enabled the professionalization of knowledge-production and depended on various forms of professional organization to join scholars who were geographically dispersed. Professional associations representing particular disciplines—the Modern Language Association, the American Economics Association, etc.—made communication possible and served as arenas where interinstitutional leadership could emerge. The journals they sponsored furnished the means by which scholars’ work could be evaluated relative to the discipline, and even the fact of publication in these journals indicated a positive evaluation. This apparatus provided the mechanism of evaluation that was necessary if disciplines were to exercise authority over their own ranks.

Just as civil regulation established the cognitive exclusiveness of lawyers and physicians in their respective domains (see Larson 1977), the university enabled disciplinary practitioners to achieve cognitive exclusiveness over their regions of the academic world. These practitioners relied not on licensing but on credentialing; they controlled the apparatus for training future practitioners and admitting them to their ranks. As an historian of research universities observes, “A discipline is, above all, a community based on inquiry and centered on competent investigators. It consists of individuals who associated in order to facilitate intercommunication and to establish some degree of authority over the standards of that inquiry” (Geiger 1986:
Moreover, the organizational structure of the research university developed so as to permit disciplinary practitioners to exercise this authority. Departments were in principle given control over hiring, tenure, and curriculum. But this relative autonomy from the central administration did not leave departments free to pursue any course. Rather their authority was shared with national and international institutions that embodied the disciplines—professional associations that sponsored disciplinary activities and promulgated disciplinary values, journals that published disciplinary and specialized research, funding agencies that supported disciplinary research and teaching, and peer reviewers who evaluated disciplinary work.

The organization of universities into departments ostensibly defined by discipline has led some to believe that “a discipline is at bottom nothing more than an administrative category” (Jencks and Riesman 1968: 523) that groups certain specialties together for reasons of historical accident and administrative convenience. This interpretation conflicts with two facts—that disciplines are social forms that are not contained by single universities; and that disciplinary practitioners, who consider themselves to be members of disciplinary communities, engage in a differentiating activity called “boundary-work.” Boundary-work entails the development of explicit arguments to justify particular divisions of knowledge and of the social strategies to prevail in them. As Thomas F. Gieryn observes, “The intellectual ecosystem has with time been carved up into ‘separate’ institutional and professional niches through continuing processes of boundary-work designed to achieve an apparent differentiation of goals, methods, capabilities and substantive expertise” (Gieryn 1983: 783). Gieryn is right about the organization of academic knowledge, but his organic metaphor tends to mask the motives and strategies of those who, at least in recent times, have carved it up. The metaphors most commonly used in discussing this knowledge are geographical ones—for instance, “territories,” “fields,” and “frontiers,” which disciplinary practitioners “annex,” “map,” and “explore” (Becher 1989: 36). Tony Becher’s interviews with several practitioners, for example, “produced clear and detailed conceptual maps” of the disciplines and their relations:

Economics was said to have one common frontier with mathematics and another with political science; some trade relations with history and sociology; and a lesser measure of shared ground with psychology, philosophy and the law. Biology was portrayed as being bounded on the one side by mathematics and the physical sciences (especially physics, chemistry and physical geography) and on the other by the human sciences (in particular by psychology, anthropology and human geography). (Ibid.)
Boundary-work is performed for various purposes. When the point is to establish or protect a discipline, boundaries mark it as a territory to be possessed by its owners, not appropriated by others, and they indicate the relations it may have with other disciplines. But these same boundaries may be redefined if the discipline is attempting to expand into new territory. When the point is to regulate disciplinary practitioners, boundary-work determines which methods and theories are included, which should be excluded, and which may be imported.

Disciplines differ with regard to the permeability of their boundaries, and most scholars believe that the permeability of a discipline’s boundaries is related to the uniformity and coherence demanded of its practitioners. “Impermeable boundaries,” Becher stresses, “are in general a concomitant of tightly knit, convergent disciplinary communities and an indicator of the stability and coherence of the intellectual fields they inhabit. Permeable boundaries are associated with loosely knit, divergent academic groups and signal a more fragmented, less stable and comparatively open-ended epistemological structure” (ibid.: 37–38). Physics and economics serve as instances of internally convergent fields that maintain uniform ideas, methods, and standards, while geography and literary studies are often cited as examples of internally divergent fields that “readily absorb ideas and techniques from neighboring intellectual territories” (ibid.: 37).

Most scholars of the disciplines would agree with Gieryn that the sciences “have often come up winners in the long history of such boundary disputes” (Gieryn 1983: 783–84), but not entirely through their own efforts. Literary study is a case in point. Betty Jean Craige contends that with each development in modern science—from Newton to Darwin to Comte—literary study defined itself in opposition to science. Whereas science investigated materiality, sought universal laws, and produced “truth,” literary study examined the human spirit, appreciated unique works, and produced civilizing effects. In the United States, “the isolation of the discipline of literary study,” Craige writes, “came about through the confluence of many educational changes during the nineteenth century (Craige 1988: 59)—including the elective system and academic specialization, which brought new prominence to the sciences, and the increased democratization of higher education spurred by the establishment of land-grant universities—but also through its own inept boundary-work. Elizabeth Wilson reveals more about this ineptitude from the 1920s to the 1950s. While the social sciences were establishing their social utility through an alliance with the progressive education movement and statistics, literary study returned to the nineteenth-century values of humane learning and individual cultivation. “The response of . . . the majority of the
literary establishment,” Wilson believes, “was to retrench behind a
tradition of aristocracy” and consequently to lose ground during the
25). Gerald Graff, however, has described literary study as a discipline
wracked by internal dissension and swept along on waves of “scientiza-
tion”—first philology and research and later the New Criticism (Graff
1987: 55–80, 121–44). In either case, given the facility of the sciences
in defining themselves, the problem is not that literary study has failed
to make itself a science but that it has failed to make itself as valu-
able and powerful as a science. The sciences have come up winners,
according to Gieryn, not only because they have defined themselves as
scientific and other disciplines as nonscientific, thereby appropriating
the cognitive authority that Western cultures grant to interpreters of
nature and producers of truth, but also because they have managed
to command academic apparatus and resources. As one of his case
studies, Gieryn shows that the nineteenth-century Edinburgh anato-
mists managed to defeat the phrenologists by defining themselves as
scientific and the phrenologists as nonscientific and, more importantly,
by denying them access to professional forums, such as university
chairs, scientific societies, and lecture halls (1983: 788–89). Even more
dramatically, contemporary scientists have cashed in their cognitive
authority for copious material resources and power:

About $1 billion of tax revenue was provided last year [1982] to support
basic scientific research in American universities; “expert” scientists are
called before courts and government hearing rooms to provide putatively
truthful and reliable contexts for decision making; science education is an
integral part of modern curricula, opening employment opportunities for
scientists at almost every school and university. Scientists often win these
professional advantages in boundary disputes that result in the loss of
authority and resources by competing non-scientific intellectual activities.
(Ibid.: 784)

If the Edinburgh anatomists were gatekeepers who controlled poten-
tial colleagues’ access to disciplinary apparatus and resources, con-
temporary scientists are monopolists of the nation’s resources for aca-
demic research and development.

But this earlier kind of boundary-work is still evident today. For
instance, traditional scientists have responded to feminist critiques
of science by declaring them nonscientific and excluding them from
scientific publications and curricula. For their part, feminists are at-
ttempting to erode the boundary between science and nonscience by
claiming that the sciences are social. They have shown that gender
relations have informed the cognitive styles of early modern scientists
(see Keller 1985; Bordo 1987) and that social relations continue to per-
meate the so-called intellectual core of the sciences today (see Harding 1986; Longino 1990). If science is social practice, as feminists contend, then they can study it in the same ways they study other kinds of social practice. “Contrary to the dogmas of empiricism,” Sandra Harding asserts, “the same kinds of analytical categories are appropriate for understanding science and society. . . . The kinds of beliefs physics and chemistry tend to produce should be explained in the same ways that we explain the kinds of beliefs produced in anthropological, sociological, psychological, economic, political, and historical inquiry” (1986: 92). Such arguments comprise two moves—de-naturalizing scientific knowledge while validating the historical, critical, and psychoanalytic methods that are used to do so. Feminists have to validate these methods because scientists have been taught to discount them. Finally, having made the point that science is social practice, feminists then challenge the paradigmatic status of physics, arguing that its “objective” (intellectually detached, culturally unmarked, and morally disengaged) approach is unsuited to socially produced knowledge. Harding has declared that a “critical and self-reflective social science” is a better model than physics for the other sciences (ibid.: 44). Physics—self-contained, deterministic, and predictable because its objects are less complex than those of biology and the social sciences—is in this respect anomalous. But even physics, as Sharon Traweek’s (1988) anthropology of Japanese and U.S. particle-physics communities shows, is social practice. By understanding disciplines as social practice, we can relate the disciplinary organization of knowledge to other social practices. In this light, modern academic disciplines are revealed to be regulated and regulating elements of a larger disciplinary regime.

In nineteenth-century education, for example, “discipline” named not merely a division of knowledge, but also the habits that study produced in individuals. The rigor of study was sometimes understood by close analogy with the rigor of bodily exercise so that U.S. and British preparatory and undergraduate curricula dominated by the classical languages were justified on the grounds that they provided mental “discipline” (Veysey 1965: 21–25). In the teaching of classical languages, students were required to memorize large quantities of information, a process which was thought to train their minds for many other applications. Thus we may understand the teaching of classical languages to be a technique of discipline. Foucault details the other techniques and strategies which proliferated in the nineteenth century. They are based on simple instruments: “hierarchical observation, normalizing judgment and their combination in a procedure that is specific to it, the examination” (Foucault 1978: 170). The monitorial system, devised in the late eighteenth century by Joseph Lancaster in England and Andrew Bell in Madras, India, exemplifies the applica-
tion of hierarchical observation and normalizing judgment in education. The teacher trained the older children, or monitors, who then drilled the younger children. Everything and everyone had a precise place: pupils were classified by level of attainment in reading and arithmetic; each one had a number used to mark his or her coat nail, slate, and desk; the desks in their schoolroom were arranged geometrically so that the monitors could easily survey and assess the students; and the school day was apportioned periodically, often by the ringing of bells to mark the commencement of every task. Discipline in this sense is a distinct form of power which trains both body and soul, and, by systematically observing and distinguishing its subjects, makes individuals.

The training of disciples in modern academic disciplines involves a similar process, although perhaps more subtle methods of control than were used in nineteenth-century schools. In order to attain his or her terminal degree, the modern disciple is repeatedly ranked and evaluated on the basis of norms. Ranking and normalizing judgments are usually rendered with the aid of various examinations. Undergraduate and graduate education is obviously replete with examinations, which do not end with the dissertation defense. The “reviews” to which most faculty are regularly subjected are also forms of examination. The price of failing a retention or tenure “review” is the loss of one’s job. More important, however, is the publication process on which the results of this other procedure usually hinge. The “referring” of manuscripts not only limits what can be said to the confines of a discipline, but also serves as the principal means of rewarding or punishing researchers and as the basis for subsequent rewards or punishments.

Foucault also notes that academic disciplines contribute to the exercise of discipline in society at large. Many of the social sciences developed out of the needs of disciplinary institutions, such as prisons and asylums, which continue to be both objects of social science inquiry and consumers of the knowledge that is produced. All academic disciplines exercise control over the objects they study, but the meaning of this control changes when the objects investigated are people. Methodologically, experimental or empirical social science requires social regulation, no matter how disinterested the research goal may be. Such social scientists are required to control their human subjects for all those variables arising from the complexity of these subjects, the diversity of their social contexts, and the unwanted effects caused

3. See Bell (1797); Lancaster (1803); Barnard (1961); Curtis and Boulwood (1966). Foucault describes French innovations in educational discipline (1978: 156–57, 177–82).
by the mere presence of researchers. As the example of experimental psychology suggests, social scientists attempt to discover “objective truth” by reducing these variables, which they do by heavily regulating their subjects. They replace the subjects’ varied existences with a uniform one of their own making—usually a brief sojourn in the research laboratory, setting, or cohort—and employ “neutral” methods, such as experimentation, observation, and quantification, to decontextualize and homogenize the subjects (Squire 1989: 25–26, 43–49; Venn 1984: 119–52).

Not all forms of knowledge-production, however, are disciplinary ones, particularly if we extend the term “knowledge” to nonacademic endeavors. Folklore and street smarts are the work of different subjects under very different conditions; they are produced and passed on by collectives gathered for such occasions as quilting parties and gang meetings. Information technology and direct mail are often used to manipulate subjects, whether to consume products, protest issues, or contribute money. But the vast networks of terminals and the waves of letters that reach a geographically disparate but demographically targeted population represent a new kind of “institutionalization” whose effects we have yet to gauge. Among the many projects in academe that appear to be cross- or counter-disciplinary, we consider three: interdisciplinary research and teaching, innovation in the social sciences, and feminist transformation.

“Interdisciplinarity,” Julie Thompson Klein writes, is a term everyone uses but without a clear sense of what it means and how widespread it is (Klein 1990: 11–14). It designates a range of activities:

— borrowing across disciplines;
— engaging in collaborative problem-solving (e.g., a health-care team of university researchers, clinicians, corporate officials, agency staff, and citizens);
— bridge-building between disciplines that remain discrete (e.g., C. P. Snow and F. R. Leavis’s “two cultures” controversy and subsequent attempts to make connections between science and literature);
— developing synthetic theories that operate across disciplines (e.g., Marxism, structuralism, and general systems theory);
— and constituting new fields from overlapping areas of separate disciplines (e.g., psycholinguistics, criminology, Egyptology, and urban studies). (See Klein 1990)

Interdisciplinary activities have been variously institutionalized as, for instance, educational movements, colleges, undergraduate programs, area studies, research centers, think tanks, temporary teams, and study groups; and, like disciplinary activities, they depend on
the usual apparatus of publications, conferences, and societies. Some scholars engage in them because they hope to unify what they perceive to be fragmented knowledge and others because that knowledge does not allow them to solve “real-life” problems. Whether they do so “for synoptic or instrumental purposes,” Klein adds, “there is a common pattern of justification—that of ‘necessity’ or ‘complexity’” (ibid.: 44). But whatever the form and purpose of interdisciplinarity, it is an attempt to obviate the difficulties created by specialization and to change the disciplinary organization of knowledge.

Mattei Dogan and Robert Pahre believe, however, that specialization is a necessary phase of knowledge-production and do not approve of interdisciplinary research. To attempt to be synoptic, they argue, is, for most scholars, to be superficial; specialization gives much-needed focus. Rather than occurring through interdisciplinarity, innovation in the social sciences happens quite naturally in the “intersections” of disciplines through “the twin processes of fragmentation and hybridization” (Dogan and Pahre 1990: 83). As disciplines attract more scholars who produce more knowledge, they become more dense; and as they become more dense, they fragment into subfields, which themselves further fragment into specializations. This fragmentation occurs along several lines—subjects, methods, theories, epistemological assumptions, and ideologies—as suggested by the examples of physical and cultural anthropology, New Criticism and deconstruction, or sociobiology and molecular biology. When specializations become densely populated by scholars and supplied with knowledge, the scholars debate minor points that in an underpopulated field would be ignored; those interested in major points look elsewhere. Thus “density in the core opens up room for innovation at the margins of the field, on the frontiers,” in the intersections (ibid.: 32). An “intersection,” according to Dogan and Pahre, is an area where specialties from different disciplines overlap. Innovative scholars who work in the intersection seek to combine the fragments into a hybrid field. The twin processes of fragmentation and hybridization work like a kaleidoscope to reconfigure knowledge.

Both kinds of knowledge-production, interdisciplinarity and fragmentation/hybridization, are what Klein calls integrative. But Klein’s notion of integrative work performed by agents contrasts with Dogan and Pahre’s notion of innovation resulting from processes. Paradoxically, given the titles of their books, Klein’s Interdisciplinarity emphasizes agent-initiated change, whereas Dogan and Pahre’s Creative Marginality features structure-initiated change. But both kinds of integrative knowledge-production may be contrasted, on the one hand, to multidisciplinarity, which “signifies the juxtaposition of disciplines” and “is essentially additive” (Klein 1990: 56), and, on the other hand, to feminist inquiry, a transformative project.
Most feminists believe that knowledges and the social structures where they are produced are mutually constitutive. Feminist knowledge, for instance, “is generated by the fact that women are both insiders and outsiders to the disciplines; the contradictions imposed by their status create a breach between their consciousness and their activity, generating critical dialogue and producing new sources of knowledge” (Anderson 1987: 239). Since they began to enter academe in the late 1960s, feminists have cultivated this outsider-within consciousness (see Collins 1991) because it allows them to work, critically and self-reflectively, in, across, and against the disciplines.

In the early 1970s, academic feminists realized the extent to which knowledge by and about women had been excluded from the disciplines, and they concluded, as Elizabeth Kamarck Minnich put it retrospectively, that whatever “is actively excluded from—or never makes it into—the curriculum is very likely to be forgotten” (Minnich 1990: 12). The first solution was to produce knowledge about women and add it to the curriculum through faculty-development projects that came to be known as “mainstreaming.” But Florence Howe and others rejected the “add-women-and-stir” approach to curricular revision, which they deemed reformist.4 Howe argued that mainstreaming “may be better than nothing,” but “it is not what I mean, however, when I describe the task ahead as transformational. I am talking about ‘changing the form of’—that is what transformation means—changing the form of the curriculum so as to include all the human race and not just a small segment of it. I am assuming . . . that research on women is changing the shape of the disciplines, and that the shape of courses based on such research will similarly be transformed” (Howe 1983: 107).

The difficulty of transformation can be seen in the contradictions that exist between feminist knowledge and the organizational structure of the universities that house this inquiry. Since disciplines first have to produce the objects they then proceed to investigate (Foucault 1972: 40–49; Canguilhem 1988: 10), reconstitution of the objects of inquiry would seem to be a minimal condition of transformation. Feminist inquiry has constituted a series of objects and has redefined itself successively around each one: women as a subjugated sex class and patriarchy as the system of male dominance; gender (the social character of women and men) denaturalized and detached from sex (the biological traits of women and men); sex-gender systems that organize various cultures; and interactive identities and oppressions, including those of race, class, sexuality, ethnicity, nationality, and gender. The reconstitution of feminist objects of inquiry is owing

4. “Add women and stir,” Charlotte Bunch’s memorable phrase, spread by word of mouth and has been widely quoted by feminists.
to the contributions of especially marginalized feminists and also to the awareness that the disciplines did not and must not contain feminism, intellectually or socially.

At first, the disciplines excluded knowledge about women and women as knowledge-producers. Now that feminists produce this knowledge, the disciplines refract the objects of inquiry. Sex-gender, for instance, is an object differently known and perhaps even a different object to be known when it appears through fictional representations, patterns of stratification and segregation in the work force, and developmental expression of chromosomes. Furthermore, the disciplines have, to some extent, disciplined the feminists who have entered them by obliging them to devote their energies to academic work and professional advancement to a degree that may be detrimental to movement activism. Thus, the problems arising from institutionalized knowledge-production are more complex for feminists than they are for those who do cross-disciplinary work that supports or modifies, rather than transforms, the existing social arrangements. Department-based disciplines, where human and financial resources flow, have power as sites of knowledge-production that is difficult to overcome, but discipline-based departments, which constitute their own objects of inquiry, produce knowledge that fragments feminist objects of inquiry. The more successful feminists are at tapping into the flow of departmental power and resources, the more likely they are to be disadvantaged by disciplinary limits. Moreover, the locating of feminist knowledge-production in the academy and the subsequent disciplining of feminists seem to be constituting feminist inquiry and activism as separate practices. The connection to activism is an old one, and most feminists understand the need to maintain it. Reflecting on the Mississippi Freedom schools of 1964 and on academic feminism of the 1970s, Howe wrote, “Looking back, however, I can see that a social movement is helpful, if not essential, to the process” of transformation (Howe 1975: 150). If the movement is no longer the context for knowledge-production, many now wonder, will feminism be able to remain a transformational project?

The study of knowledge yields few answers to questions of this sort because, when knowledge itself has been an object of study, it has seldom been studied as social practice. The oldest approach to knowledge, the philosophical project of epistemology, sought not to describe the production of knowledge but to show how we could know that what we know is true. Epistemology was not aligned with any particular form of knowledge, although its importance stems from the need to refute the skepticism which arose in response to the Enlightenment rejection of the old Scholastic means of authorizing beliefs. Epistemology is thus a form of apologetics. It has a vested interest in
denying whatever might place conditions on knowledge, so it does not concern itself with the social or the historical. From the perspective of epistemology, disciplinarity is a neutral term since the social form that knowledge takes does not bear on the question of its “truth.”

In the twentieth century, the prestige of the science disciplines sparked a new discipline, philosophy of science, whose purpose was to interpret and justify their ideal status. Rather than the fear of skepticism, it was the desire to protect the purity of science from the corruption of pseudoscience that motivated this study. Thus boundary-work is its major activity. As Steve Fuller puts it in his essay below, philosophers seek “demarcation criteria that systematically discriminate the sciences from nonscientific (and especially pseudoscientific) forms of knowledge” and draw “the science/nonscience boundary largely in order to cast aspersions on the legitimacy of particular pretenders to the title of science” (p. 302). While they do not deny that other disciplines have some of the characteristics of the sciences, they argue that these disciplines lack the one requirement for scientific status: “truth.” Like epistemology, philosophy of science has a vested interest in not addressing the social and historical conditions of the sciences and in not developing a critique of discipline.

Sociology of knowledge, by contrast, has sought to explain the production of knowledge in terms of the interests such production has served. In its early years, sociology of knowledge remained largely concerned with ideas, rather than with the social organization of knowledge-production, and thus did not address disciplinarity. More recent projects in the sociology of knowledge, as exemplified by the work of Bruno Latour and Steve Woolgar (1986), investigate the actual practices of knowledge-producers. While these practices point to discipline, the new sociology of knowledge has not made it an explicit object of investigation. The failure to do so has been attributed to the synchronic, rather than diachronic, approach taken by sociologists of knowledge, but it probably arises from the fact that for them, as for others who concern themselves with academic knowledge, discipline remains a given.

As Fuller notes, below, “The ‘discipline’ is one of the few units of analysis that requires the cooperation of two rival historiographical approaches,” by which he means the internal, “devoted to charting the growth of knowledge in terms of the extension of rational methods to an ever larger domain of objects,” and the external, “devoted to charting the adaptability of knowledge to science’s ever-changing social arrangements” (p. 302). Yet, historians of knowledge have seldom exhibited such cooperation because their interests often lie in asserting the rationality of the knowledge they discuss or in demonstrating that irrationality results from that knowledge’s social position. Internal his-
Stories are usually written by the disciplinary practitioners themselves and use the terms of a discipline to recount its history, while external histories are more likely to be written by members of other disciplines and in terms other than the disciplinary ones. Moreover, the internal program "assumes an independent history of intellectual structures," whereas "the external program . . . views the social structures and the environment . . . not simply as contingent boundary conditions or as a complementary dimension of the development of the logical structures of thought but regards them as constitutive of these" (Van Den Daele 1977: 27). As these affiliations suggest, internal histories serve a legitimating function, while external ones may be critical. Wolf Lepenies and Peter Weingart identify several legitimating functions and intended audiences. Socializing histories, for example, "appear in the first chapters of standard text books" (Graham, Lepenies, and Weingart 1983: xvi); they introduce students to a discipline by presenting biographies of its great practitioners and accounts of landmark advances. Other histories serve to legitimate a discipline to those who support (foundations, government agencies), regulate (policymakers), or generally approve (the public) its research. The rhetorics employed in these histories differ greatly, as the contrast between technical reports and popularizations suggests. Legitimating histories also address academic audiences. They may be intended to justify a discipline to practitioners of more scientific disciplines. (It is an interesting fact that the more scientific disciplines are seldom justified to the less scientific ones.) They may function, radically or conservatively, in promoting or opposing a change, or they may simply celebrate disciplinary progress. But they are rarely intended to contribute to such progress since most historians of progress assume that the lesson to be learned from the past is the superiority of the present.

What distinguishes our approach from these kinds of studies is that we require historical and critical analysis of the disciplines, whereas they do not (see, e.g., Heckhausen 1972). We treat disciplines as historically discontinuous: they are knowledge-formations unlike those that have preceded them and may very well be unlike the knowledge-formations of the future. Not all studies of disciplinarity need be histories, but they do need to assume that knowledge is historically and socially contingent. The authors of the essays presented here seek to understand the internal lives of the disciplines they discuss without making the usual mistakes of focusing on the "progress" of knowledge or the social circumstances that have influenced it. They understand rhetoric, for example, as contributing to the distinctive knowledge any discipline produces since all disciplines, even those with highly mathematical methods, produce distinctive discourses. But they also understand rhetoric as one of the ways a discipline has social consequences.
Thus, these essays share some of the concerns of sociologies and histories of knowledge, although they are not themselves limited to these approaches. They borrow little, however, from philosophy of science and epistemology. To take disciplines as historical artifacts is to refuse to equate disciplinary knowledge with “truth.” This approach to disciplinarity leads away from the issues that have preoccupied philosophy of science and epistemology.

For Fuller, however, this approach means not a rejection of epistemology, but a redefinition of it. The outlines of his project are presented in his book, *Social Epistemology* (1988), where he argues for a normative philosophy of knowledge rooted in a descriptive sociology of actual knowledge-production. In the essay which concludes this forum on disciplinarity, Fuller takes up an issue which the other essays also address, “the embodiment of knowledge as a source of worldly power.” He contends that the disciplines which have held or claimed the designation of “science” have achieved their power by “rhetorically drawing our attention to the fact that scientific knowledge represents the world and away from the fact that it also intervenes in the world” (p. 301–2). Paradoxically, disciplines must demonstrate what Fuller calls “Baconian virtues”; that is, they produce knowledge that maintains the social order but seems independent of that end and, indeed, of the world itself. Sciences must give the impression of being undetermined by the world; if they fail, they encounter reflexive difficulties, which undermine their scientific status. Thus Fuller demonstrates that objectivism of a particular sort is entailed by the disciplinary organization of knowledge.

Evelyn Fox Keller’s essay provides what might be read as an extension of Fuller’s point. In discussing two books—by Muller, a biologist, and by Schrödinger, a physicist—that present visions of the incipient field of molecular biology, Keller shows that the book which had more influence was the one that did not present an interventionist agenda. While the effect of molecular biology is intervention on a scale previously unimaginable, the project of molecular biology was successfully promoted, not in terms of the possibility of “molding the evolutionary future of the human race,” but rather as a quest for “unified, all-embracing knowledge” (p. 235). Keller’s point, however, is that the authority of physics explains the greater influence of Schrödinger’s book, and the authority of modern physics derived precisely from its interventionist power.

Keller also shows how the practice of biology changed under the influence of physics. Physicists brought to the new biology, not particular methods or techniques, but rather an agenda, an attitude, and a language. For the insoluble problem of biology (“the mystery of life”), they substituted a problem that could be solved (“the physical basis
of genetic information”) and then claimed to have solved the former mystery. Biologists were attracted because this agenda resonated with their ambitions and the association with physics provided them with the authority to pull it off. Indeed, the outcome was the transformation of biology from a discipline that described organisms to one that experimented with genetic materials. As this new disciplinary object was constituted, “the older biology (and many of the older biologists) became objects of disdain; they had lacked, above all, an understanding of scientific progress” (p. 239). As a result, biology has increasingly abandoned—and failed to take up—forms of research other than the molecular. One conclusion we can draw from Keller’s essay is that disciplines take particular forms of development as the result of reciprocal processes that defy “internal” and “external” categorization.

Keller has written her essay “to illustrate the conjoint workings of intentionality and consequentiality, of social and material forms of power” (p. 229). Theodore Porter addresses this same matter in his discussion of the growth of quantification in French government bureaucracy of the nineteenth century. By quantifying, French engineers claimed for themselves an important expertise—the ability to determine the objective value of projects that had widely different utility for various segments of society. While numbers seemed to provide the means for transcending the “idiosyncrasies of the various scientists” (meaning their identities and social relations), as well as the socio-political problem of conflicting interests, Porter also points out that this rhetoric of quantification was a response to democratization because such an elaborate justification would have been unnecessary to a regime possessed of absolute power. Moreover, the appeal of quantification—it suggested the democratic values of openness, honesty, and freedom from corruption—allowed it to be used as an instrument of political power. Thus, Porter argues, the “massive growth of quantification shows that the identity of knowledge and power announced by Bacon is more nearly accurate than we might suppose in a world where politics appears irrational, and science (especially quantitative social science) abstract and otherworldly” (p. 245).

Quantification is also a central issue for David Sylvan. He is concerned, not with rhetoric that justifies a discipline to the public, but with the hegemony of the rhetoric of quantification within the discipline of political science. Disciplinary boundaries are also an issue for Sylvan, but he understands them in a somewhat different way from Fuller. Qualitative methods, he argues, “act as the limits of the discipline, defining both semiotically (through certain buzz words like ‘soft’ or ‘subjective’) and praxiologically just what it is that most political scientists want not to do” (p. 273). Sylvan’s essay attempts to explain why the boundary of political science came to be established here and
not elsewhere. If Fuller were to address the problem, he might argue that the identification of quantification with science is an instance of boundary-work that demarcates the sciences from nonsciences. Sylvan’s explanation, however, is that the quantitative turn in political science occurred more or less as a result of historical accident. The influence of Charles Merriam and the political science department he chaired at the University of Chicago explains the dominance of quantitative methods. This specific history is contrasted with that of the sociology department at Chicago, whose influence led sociology to include strong representation of both quantitative and qualitative methods. Sylvan’s discussion of Harold Lasswell suggests that political science might have taken a different turn if Lasswell had pursued his original, qualitative agenda. Although Sylvan finally cannot explain why the history of political science took its precise course, he is clear about the effect of that course: its “one-sidedness means that political science has become more than ever a disciplining discipline” (p. 282).

Quantification, which is often assumed to be the most rational aspect of science, appears magical in Donald McCloskey’s essay. He is also concerned with the rhetoric of quantification, intervention, and control. McCloskey discusses magic and economics as two forms of practice that seek to deal with the problem of scarcity. But where magic assumes that we can wish scarcity away, economics—or, in McCloskey’s view, good economics—knows that we can only live with it. Economics should be understood as poetry, rather than as magic: it may seem to be all spells and incantations, but good economics knows that names are not things. Instead of telling us how to produce something out of nothing, “it says: you can’t get that” (p. 297). This view of economics seems precisely to deny its Baconian virtue, its interventionist value: “the point is to know history, not to change it” (p. 298). Economics, or any other science, can become dangerous when it forgets itself and becomes magic. Thus both Marxism’s old claims to prediction and the Nazis’ attempt to “lay down the future” at Auschwitz are examples of magical thinking. In this sense, McCloskey’s argument is consistent with the poststructuralist critique of totalizing projects, and its warning against the dream of prediction and control would have to be applied to the ambitions of physics and molecular biology as well. However, the effects of his argument are ambiguous. While denying the practical efficacy of a discipline may seem to be a radical move, it may be an example of the rhetoric Fuller describes: a claim to represent the world, rather than to affect it.

The essays assembled here do not present a monolithic conception of disciplinarity. They differ in where they locate the essential features of disciplines—in public rhetoric or internal communication, in historical necessity or historical contingency. Yet all of the contribu-
tors agree that disciplines are historical products and that they are rhetorically constituted. Their emphasis on history and rhetoric contrasts with the long neglect of these dimensions by epistemologists and philosophers of science. But these essays hardly represent the full range of issues which studies of disciplinarity might explore. They remain more concerned with the discursive, the logical, and the ideal than with the material and institutional. The disparate character of these studies suggests that no paradigm currently reigns in the study of disciplines—and that the incipient discipline of disciplinarity remains as yet undisciplined.

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