

edited by  
**Sylvère Lotringer**  
and **Sande Cohen**

**french theory in america**

**Routledge** New York London

6. Ibid., p. 8.
7. This refers to an oft-cited review by a critic in the *London Times* who also described Documenta X as "a tragedy being enacted in the small German town of Kassel." Daniel Birnbaum, "Little d, Big X. Documenta X: The *Artforum* Questionnaire," *Artforum* 36 (September 1997), p. 106.
8. Sound bite offered by Ulrick Looock, director of the Kunsthalle Bern; *ibid.*, p. 108.
9. Catherine David, *Documenta X: The Book*. Cantz, 1997.
10. Richard Flood, cited in "Little d, Big X. Documenta X," p. 110.
11. Nancy Princenthal, "Documenta X," *Art / Text* 59 (September 1997), p. 32.
12. In 1998, Hirschhorn constructed an "altar" on the streets of Zurich to venerate the Austrian writer Ingeborg Bachmann, who died tragically by burning to death in her bed. She was known for her chain-smoking—a smoldering cigarette caused the fatal fire. Hirschhorn fashioned the central element of his piece—Ingeborg Bachmann's name—entirely of boxes of Marlboro cigarettes glued together. This sort of literalism is noted here to give a dose of credibility to this speculation about Hirschhorn's hypothetical engagement with abjection.
13. For further discussion of this strategy see Alison Gingeras, "Striving to be Stupid: A Conversation with Thomas Hirschhorn," *Art Press* 239 (October 1998), p. 20.
14. See Elie Dering, "Blackboxing in Theory: Deleuze versus Deleuze," in this volume.

## From Difference to Blackboxing: French Theory versus Science Studies' Metaphysics of Presence

MARIO BIAGIOLI

Few academics or academic disciplines in the United States have displayed a neutral stance toward French theory.<sup>1</sup> A few have taken it up with excitement, but more frequently it has been rejected without much detailed criticism, often citing its alleged pervasive and multiform dangers. In this respect, the position of science studies is interestingly atypical. Here we have a field that has adopted various blends of relativism in its methodological assumptions, is openly concerned with theoretical issues, and yet has only skirted French theory, using it as a tool for "special" problems but without allowing it to affect the canonical questions and assumptions at the roots of the field. This essay addresses this peculiar ambivalence.

### The Peculiarity of Science Studies

Science studies is a loosely structured field that looks at science and technology from different perspectives (history, sociology, anthropology, as well as gender and literary studies).<sup>2</sup> Its active engagement with theoretical and methodological issues has been mostly the result of necessity, not virtue. In an attempt to take over the traditional epistemological questions of philosophy of science (while answering them differently), science studies practitioners have had to develop theoretical skills and sometimes reinvent themselves as analysts of the sociocultural and gendered production of scientific knowledge. Because many of the practitioners' original disciplines did not provide those skills as part of their standard graduate training, the range of interpretive positions that science studies has come to adopt is quite broad and has a certain self-taught, opportunistic quality to it. The absence of a theoretical canon has made the field hard to map, but it has also given it a good deal of unself-conscious freedom to experiment. Its adventurous theoretical bricolages may have raised a few philosophers' eyebrows, but have gained science studies an interested audience among more eclectic and cross-disciplinary fields in the humanities and social sciences.

Given such a genealogy, one would expect science studies to be a prime candidate for a productive engagement with French theory. Instead, the conversation has been either spotty or limited to those science-related topics addressed by French theorists themselves. Foucault's analyses of medicine, madness, sexuality, taxonomical systems, and biopower have provided the busiest area of conversation between science studies and French theory because of the topical overlap between the two fields.<sup>3</sup> But, with a few exceptions, Foucault's discussion of the power-knowledge nexus has been the source of ubiquitous citation, not sustained or detailed elaboration.<sup>4</sup> Semiotics makes cameo appearances in science studies, while some of Michel Serres's ideas (e.g., the "natural contract") have come into the field through the work of Bruno Latour but have not spread much beyond that.<sup>5</sup> Deleuze and Guattari are a background presence in the work of a handful of particularly theory-oriented authors, while Derrida's work (mostly his notion of "writing") has been taken up in, and limited to, a few specialized analyses of scientific inscriptions (with the exception of Rheinberger's notion of "experimental systems").<sup>6</sup> French feminism has had little direct influence on gender studies of science, and references to Lacan can be counted on one hand.<sup>7</sup> Similarly, Baudrillard's work or Lyotard's discussion of science in his *Postmodern Condition* have had almost no detectable effect on the field.

Such an ambivalence toward French theory, I argue, cannot be understood by framing it as a purely intellectual problem, something reducible to its pros and cons or to the possible relevance of its questions to science studies. This approach would risk reifying "pros," "cons," and "relevance" rather than consider these notions as products of the academic systems in which they are used. In place of an analysis of the philosophical rewards and problems of French theory considered as a body of texts, I take a more mundane look at the conditions of possibility of exchange between French theory and science studies as they develop when a body of texts produced in a specific intellectual and institutional context comes into contact with a field that has constituted itself in a different disciplinary economy—an economy that frames notions of reward, style, cross-disciplinary kinship, audience, and forms of academic reproduction.

For instance, what are the conditions under which certain fields can borrow freely and extensively from others (e.g., history from anthropology, film studies from psychoanalysis, literature from gender studies) and yet maintain or even expand their traditional niches in academia? And under what conditions, instead, are seemingly comparable borrowings (like the kinds of notions and approaches science studies could import from French theory) resisted as possible threats to a field's "identity"? What is it about the academic system of disciplines that frames what counts as a useful or relevant import and what, instead, as trivial or even dangerous? What can the relationship between science studies and French theory tell us about what

"discipline" means in France and the United States and the role that notion plays in the economy of those two academic systems?

I begin by focusing on science studies' genealogy, its relationship to its subject matter, its borrowings from and relationship to neighboring disciplines, and the metaphysics of presence it has developed during its formation as a discipline. (By metaphysics of presence—a notion I will describe more extensively—I mean a set of assumptions about the socially constructed robustness of scientific knowledge). I will then try to show how such a metaphysics (with roots that are simultaneously philosophical and economic/institutional) has framed its interaction with French theory. I conclude by discussing the different academic ecologies of Anglophone science studies and French theory, as well as considering examples of the shapes that science studies has taken and could take outside of the U.S. academic system and its disciplinary economy.

### Conservative Radicals and Their Metaphysics

Predictably, part of science studies' genealogy derives from research questions and models and the methodological canons that have sustained them. Much of the field's agenda has been framed by (or in response to) the issues and categories laid down decades ago by Fleck, Kuhn, and Feyerabend.<sup>8</sup> This heritage has placed an almost exclusive emphasis on the belief systems of groups (paradigms, thought collectives, research schools, etc.) and related issues. How is the production of knowledge and consensus tied to the social structure, hierarchies, forms of communication, and boundaries of these groups? How could the knowledge and practices of these groups be made to travel outside of them and become "universal"? How do controversies across groups reach closure and claims become "blackboxed"? Even gender issues (quite alien to the original agendas of the "founding fathers" of the field) have often been analyzed in terms of the beliefs and gender assumptions of groups, and how these framed their claims about nature, as well as the inclusion or exclusion of women from these communities.<sup>9</sup>

Traditionally the focus has been on the various group-based processes through which scientific claims (assumed to be inherently unstable, contestable, and underdetermined) are eventually made to stick, or on variations on this theme such as how "weak" claims are sometimes turned into "strong" ones.<sup>10</sup> In the same conceptual space I would also place discussions of how science (which is made up of many different subcultures operating according to different assumptions, methods, and practices) manages nevertheless to develop a pragmatic "uniry," a kind of behavioral consensus that allows it to produce knowledge claims in the absence of a unified intellectual agreement about those very claims.<sup>11</sup>

Not surprisingly, sociology, social anthropology (in the United Kingdom), and cultural anthropology and ethnomethodology (in the United States) have provided much of science studies' toolbox. For the same reasons, Pierre Bourdieu's sociology of fields, and especially his concept of "habitus" have found several users in science studies (making him the most cited French thinker, after Michel Foucault). But, more frequently, it is Wittgenstein's later work that has provided the philosophical framework for science studies' group-oriented approaches.<sup>12</sup> ("Actor network theory" is an important exception that I'll discuss later.) But whether one talks about architecturally circumscribed spaces like laboratories, or about less physically bound entities such as schools of thought or disciplines, it often seems possible to find and talk about "language games." Debates about tacit knowledge, skill, and embodied knowledge can also be connected, in a more or less direct line, to Wittgenstein's discussion of what it means to "follow a rule."<sup>13</sup>

Science studies' comfortable reliance on Wittgenstein for its philosophical needs has dampened its appetite for alternatives. Its comfort level has only been enhanced by the fact that Wittgenstein is one of the few modern philosophers left largely unscathed by recent critical work—work that, instead, has inflicted substantial damage to Freud, Marx, the structuralists, and other major modern providers of comprehensive interpretive frameworks. More important, Wittgenstein's authority has been left unchallenged by both Anglophone (often analytical) philosophers as well as by French theory (which has either left Wittgenstein undeconstructed or, as in Lyotard's case, has relied on it).<sup>14</sup> Science studies, therefore, hasn't had to worry about having bet on a dead (or deconstructed) horse, nor has it had to defend itself from or convert to French theory.

But perhaps the main reason behind the scant interaction between science studies and French theory is that the field (contrary to the interpretation by some of its recent critics, who have branded it "postmodernist" because of its relativistic methodological assumptions) has developed a peculiar metaphysics of presence—a position that puts it at odds with several French theorists. Science studies does not attach presence to the kind of nature-related categories used by scientists or by older philosophy of science (or to the speech-like notions Derrida has discussed in the case of Western metaphysics), but rather to social structures, institutions, cultural values, networks, laboratories, trading zones, social and monetary resources, skill, tacit knowledge, and ubiquitous "practices."<sup>15</sup>

These categories instantiate "presence" in the sense that it is through them that science studies explains how scientific claims—claims it presents as inherently unstable—are eventually made robust and stable. But while such a metaphysics of presence is about what constitutes scientific knowledge, its mundane roots are in the academic economy in which science studies has developed and tried to establish itself. It is rooted in its relationship with the disciplines it studies and those it bor-

rows and tries to differentiate itself from in an attempt to develop its "authorial voice."

Science studies' intellectual agenda still focuses on the age-old question of how one gets from many competing claims down to one. What differentiates it from previous philosophical traditions that have addressed these issues since the pre-Socratics is that the trajectory from "opinion" to "truth" is now usually explained as the effect of sociocultural entities (though several of them are arguably opaque or even trespassing into the occult). In its most radical expressions, science studies has turned upside down received views on the production of scientific knowledge, which presented nature itself as the ultimate arbiter of knowledge. But while placing society and not nature in the arbiter's seat, these radical revisionist moves have maintained a causal epistemological framework—one that is ultimately rooted in the nature-society dichotomy.<sup>16</sup> In some ways, social constructivism has done to realist philosophy of science what Marx did to Hegel. But like Marx who could not get away from the pull of dialectics, social constructivism has not been able to part with causality. In the end, it has simply inverted the direction of causality from nature to society.

Science studies in fact simultaneously criticizes and maintains the traditional dichotomy between nature and society, and the explanatory mental habits engendered by it. Although the field is vocal about the need to question the separation between nature and society, it usually ends up reinforcing such a dichotomy by casting its analyses as attempts to "bridge" it rather than to do away with a conceptualization of the problem that would then cast bridges as the natural solution. Only a handful of authors have seriously departed from this framework.<sup>17</sup> Most commonly, science studies still tries to bridge the gap between science and nature through a range of murky topoi such as mediation, clotting, influence, interaction, embodiment, cultural meanings, embeddedness, resonance, and through a host of optical metaphors (reflection, refraction, diffraction). That the field seems to turn a deaf ear to the opacity of these notions indicates, I believe, the extent of its commitment to a modernist, explanatory, causal ethos. Its earnest but short-lived engagement with the issues raised by the "writing culture" debate in ethnography—an engagement that has been mostly about putting the cat back in the bag as quickly as possible—is, I believe, another symptom of the field's reluctance to give up its disciplinary identity as a social "science."<sup>18</sup>

The stabilization of the unstable or the production of some unity out of disunity has become the field's central challenge. Science studies has reproduced, *mutatis mutandis*, the traditional distinction between the "context of discovery" and the "context of justification" and has dealt almost exclusively with the latter. Like logical empiricists who focused on the conditions under which scientific knowledge could be accepted as true while paying little attention to how new

scientific claims emerged, science studies has analyzed how claims could be socially "blackboxed" and controversies be brought to closure, and not to how claims and beliefs keep emerging and move in different directions. While science studies is about the production of scientific knowledge, "production" tends to denote the survival of specific knowledge claims, not the proliferation of their differences. Science studies treats difference, play, and drift only as its starting point but *not* as its final result, or as processes that need analysis in and of themselves. It assumes difference as the raw material for its analyses but then moves quickly to what it takes to be the more important stuff: the social processes through which difference is reduced, if only temporarily, to one. As a corollary, I suspect that science studies' interest in Foucault (by far the field's most cited exponent of French theory) may have been based on a selective reading of some of his notions (episteme, institutions, discipline) as objects that *constrain* (rather than produce) knowledge.

### Relativistic Experts and Their Voices

I am not suggesting that science studies should reverse its interpretive priorities and dedicate itself to the joyous celebration of the proliferation of epistemological differences. That would simply flip (not undo) the polarity of the field's intellectual framework. What I want to point out, instead, is the strong *directionality* the field imputes to the knowledge-making process, and that such directionality separates the beginning and the end of such a process not only chronologically but *analytically*. The "beginning" and the "end" become two different kinds of beasts. Not only do they attract different levels of attention, but also become the objects of different kinds of discourses and analytical constructs.

Furthermore, different values, different levels of professional reward have been attached to the study of the beginning and the end of the knowledge-making process. While most of my colleagues would argue that their analyses of the closure of scientific controversies do not entail any value judgment on the claims that happened to "win" or "lose" those controversies, science studies *as a field* does attach value to closure (or cognate categories like legitimation, acceptance, canonization, etc.). More precisely, while science rewards the *results* of closure (that is, claims that are accepted by the community), science studies prizes the *explanation of the process* through which closure was achieved (or of why closure did not take place). In sum, science studies' metaphysics of presence is also related to notions of disciplinary reward and "value."

The tensions produced by this metaphysics is most clear in science studies' acrobatic defenses or qualifications of its relativistic stances. The image of the field as made up of irresponsible relativists that has emerged in the recent "sci-

ence wars" is something that has followed its practitioners at least since Kuhn's *Structure of Scientific Revolutions* (1962). The critics' main line has been that at best science studies flirts with the belief that "anything goes" or that at worst it fully endorses that position (or related ones, such as that scientific claims stick just because of the power of the sociopolitical interests behind them). Science studies practitioners see these critiques as misrepresentations. They respond by saying that, yes, they do believe scientific claims are underdetermined by evidence, that replication is affected by an endless logical regressus, that the choice of methodological assumptions is by no means univocal, and that communication across theoretical divides or across different experimental practices is inherently problematic. But they also say that, no, this does not amount to "anything goes." On the contrary, only a very *small* number of scientific claims do stick, and that they do so as the result of a lot of work, negotiations, and constraints.

Science studies, then, is not about arbitrariness but about how arbitrariness is made to end; it is about how consensus is established, if only temporarily. If the critics see relativism as the beginning and the end of science studies' program, its practitioners see the underdetermined nature of scientific claims only as the starting point of their work—work that then focuses almost exclusively on the intricate ways in which claims are made contingently robust despite their ever-contestable underpinnings.

While the critics equate constructivism with relativism, science studies (especially the sociology of scientific knowledge) displays a surprising scientific ethos. Relativism is cast as a "scientific" methodology: unless you start from relativistic assumptions you will never understand how science *really* works. One starts as a relativist but ends up a social "scientist." One starts with assumptions about the inherent instability of scientific knowledge, but ends up with a (social) master narrative about how it becomes stable. In the end, we have turned ourselves into *relativistic experts*. The predicament is, I believe, telling.

The tension between relativism and normativity is particularly evident in the sociology of scientific knowledge, but it can also be found, *mutatis mutandis*, in other branches of science studies. As a result of their training in history, science historians have heard and reproduced (at least on an official level) the credo that history is about open-endedness (not determinism or normativity) and about description (not explanation). However, once they moved into science studies, historians felt that the descriptive ethos of their original discipline put them at risk of becoming epistemologically subordinated by the explanatory stances of their subject matter (science) and of their fellow travelers (sociologists and philosophers). This predicament was new to them because, in their previous incarnation as historians of social events and processes (or even as intellectual historians), they were not members of an epistemologically ambitious discipline.

This new disciplinary predicament has pushed them toward the implicit adoption of science studies' metaphysics of presence and the many tensions that go with it. For instance, they have engaged in an awkward methodological dance aimed at making "description" sound more explanatory, but not so explanatory as to undermine their disciplinary identity as historians. In some cases this has been attempted by characterizing their work as belonging to the genre of Geertz's "thick description," Ginzburg's "microhistory," or Nietzschean-Foucauldian "genealogy"—a move that is not justified so much by the theoretical and methodological clarity of such genres (which, in fact, can be quite opaque), but because they seem to give a name to the demarcation that science historians wish to draw between themselves and "merely" descriptive historians. There are also a few historians of science who proudly endorse historicism, but they usually do so not to limit the epistemological authority of their work but, on the contrary, to amplify it by criticizing the scientism and reification at the basis of the authority of philosophy and sociology of science. The result of that move is not to give up explanatory ambitions, but to defend them at the local level while undermining other disciplines, grand narratives and the explanations they produce about those same events or methodological constructs. Presence is not done away with but is rather found in local historical contexts.<sup>19</sup>

### Disciplinary Economies

Science studies' metaphysics of presence and its scant engagement with French theory is largely a matter of how its academic genealogy framed its disciplinary kinship categories and authoritative ambitions—ambitions that cannot be severed from the logic of academic economy in which they are constituted. Science studies' epistemological stance is as hybridized as its academic predicament. The field is uneasily nested between the "two cultures," unclear about how to negotiate different kinship lines pulling it in different directions; it has emerged by rejecting previous views of the production of scientific knowledge while maintaining the same causal framework and epistemological questions of its predecessors; it brings new perspectives to old epistemological questions but imports most of its tools from other traditional disciplines (mostly social sciences) that had used them to pursue questions other than those of scientific knowledge; it tries to blend strong relativistic assumptions with an equally strong explanatory ethos; it questions the "naturalness" of science's authority but resists doing the same about its own claims; it can have a sympathetic understanding of what French theory is, but usually treats it only as a supplier of "special" tools for "special" problems because a more extensive conversation might end up destabilizing its academic position and authority.

I do not know whether, or to what extent, I mean these comments as a criticism or simply as a statement of science studies' predicament and genealogy. In fact, some of the factors behind science studies' metaphysics of presence can be quickly traced to the specific constraints deriving from its subject matter and to science's dominant role in academia.<sup>20</sup> As inscribed in its name, science studies is still about that thing called science, not about broader (but not necessarily less tricky) notions such as knowledge. Even if one does not think of science as an enterprise producing truth, one must acknowledge that it exists as a powerful and highly successful profession rooted in the production and reward of claims recognized by one's peers. The existence and especially the success of science (no matter what the reasons for such success) determine the subject of science studies as science studies. To a large extent, "presence" comes with the territory (or at least with *this* territory). And it is not clear to me how much science studies could move away from its subject-imposed subject without changing its name and moving into different academic niches.

Similar things could be said about science studies' commitment to an explanatory ethos. It does have costs, it does produce aporias (especially when it is coupled with relativistic assumptions about the indeterminacy of scientific claims), but then if aporias were death knells, academia would be a cemetery. Or, to put it differently, aporias may be the cost a discipline has to pay to develop and maintain its "symbolic capital" within an academic system in which rewards are ultimately tied to expertise (or expertise "effects") even though that expertise might be about aporias themselves.

One could also consider other crucial elements of the academic economy such as the "products" a discipline is expected to deliver. While we often tend to think of texts as such products, perhaps we should also focus on our role as Ph.D. producers. Academics are keenly aware of issues relating to the recruitment, funding, training, and placement of graduate students, but then for some reason we tend to bracket off those concerns and focus instead on our role as scholars and intellectuals. Once we admit that, at least in U.S. academia, the cost of a five-year graduate fellowship matches (and sometime exceeds) that of a condominium, it becomes apparent that a discipline is a peculiar industry and, as such, it would not survive for long if it failed to carefully package its students for successful placement in recognizable niches within the academic market. Science studies' metaphysics of presence, then, is also about the stabilization of academic market categories, that is, the production of disciplinary "value."

By dropping its explanatory commitments and developing closer ties to French theory, science studies would perhaps free itself from some intellectual and disciplinary constraints, but it might also change its field of authority, thereby diminishing the likelihood of its academic reproduction. By taking that direction,

science studies would probably move closer to the predicament of a field like cultural studies (with all the pros and cons that move might entail). There are indications that, in some instances, this is already happening. Science studies work most explicitly informed by French theory has indeed moved toward literature and rhetoric departments or toward cross-disciplinary programs in the humanities; but by doing so it has also reduced its chances at reproducing itself as “science studies.”<sup>21</sup>

### From Boundaries to Networks

The tensions of science studies’ predicament can be exemplified by the peculiar reception of Bruno Latour’s work, which in some ways occupies a hybrid position between science studies and French theory.

As it does with French theory, traditional science studies sees Latour as a very important source of “suggestions,” of ideas “one should think about,” or of a range of “proposals” one should pick and choose from. At the same time, because it has an undeniable family resemblance with science studies, his work cannot be marginalized as French theory has been. He is perhaps the most cited author within science studies, but, at the same time, the field is far from being “Latouricized.”

Latour is one of the few authors who have explicitly rejected science studies’ metaphysics of presence based on the nature-society dichotomy—a position that aligns him with some of French theory.<sup>22</sup> And, like French theory, his work is hard to categorize in terms of disciplinary boundaries. Although it developed from empirical ethnographies of scientific laboratories, the questions it has since pursued go well beyond those of traditional science studies to the point of questioning the relevance of the fields’ disciplinary identity.<sup>23</sup> Then, like French theory, Latour is French. This means that his work, while strongly pitched for the Anglophone market, has developed in an academic system and in disciplinary frameworks that are much closer to those of French theory than of traditional science studies. His literary style, genres, and publication venues are much closer to those of Parisian *maîtres à penser* than to American or British academic authors.<sup>24</sup> At the same, Latour is no friend of French theory. With the exception of the work of Michel Serres, he has had few kind words for French theory, which he has lumped in with postmodernism and then usually dismissed with little engagement.<sup>25</sup> While he does not place himself in the modernist camp, he also rejects the label *postmodernist* and instead defines his work as “amodern.” Then, unlike French theory (but like traditional science studies) he is still strongly committed to an explanatory ethos (though one that differs from that of the traditional social sciences).

Latour’s innovation hinges on his (and Michel Callon’s) notion of “translation”—a notion that is quite distinct from sociocultural “mediation” (and related categories), which most of science studies sees as constitutive of scientific knowledge.<sup>26</sup> Translation is meant quite literally as trans-lation, that is, as spatial displacement, not as a process through which “meaning” is negotiated or reproduced. According to Latour, claims do not have sociocultural meanings, nor are they blackboxed through political, social, or professional “power.” His approach is constructivist but differs radically from social constructivism in that social structures and dynamics (such as “power”) are not seen as providing the containing and stabilizing framework for unstable claims. Both claims about nature and social objects are seen as symmetrically constructed through a series of spatial translations, through a long chain of displacements and re-alignments of hybrid entities that are neither natural nor social. The strength of scientific claims is the result of their “spatial” features—the density and length of the chains of translations of which they are part. Laboratories play a crucial role in Latour’s model, but not as socially powerful institutions whose walls serve to separate expert knowledge and its legitimate practitioners from other forms of knowledge that are constructed as unscientific precisely by not being admitted in. Laboratories do not produce knowledge through socioepistemological gate-keeping, but precisely by breaking down the separation between nature and society through the construction of long, fine-textured, and dense chains of translations of which the laboratory becomes a “node.”<sup>27</sup>

While Latour’s emphasis on hybrids and displacements would seem to send him in the direction of French theory, he parts ways with the postmodernists because he does not wish his hybrids to “drift.”<sup>28</sup> Like traditional science studies, Latour still relates knowledge to control, not proliferation. Latour’s hybrids do proliferate (and the more they proliferate the better it is), but they are also “lined up” through translations and networks. He abandons science studies’ social metaphysics of presence, but then immediately introduces a network-based ontology—an ontology that is neither about nature nor society but is an ontology nevertheless. In this sense, his version of “amodernity” is close to modernism.

In sum, science studies broke up the small containment vessel that traditional philosophy had built around science, and expanded the definition of science so as to include its sociocultural dimensions. But while replacing a small vessel with a larger one, it did not question the notion and role of vessel itself. The socioepistemological box that science studies constructed around science doubled as a disciplinary box for the field—a box that framed science studies’ academic predicament and identity. Therefore, Latour’s replacement of the nature-society dichotomy with networks of translations is not seen as just another approach to science studies but as a model that risks dissolving the field’s boundaries by sub-

stituting them with interpretive networks that cut across many (perhaps all) traditional disciplinary divides. And it is not clear what kind of disciplinary boxes could accommodate the work produced through that kind of analysis. In some ways, Latour's work is like Leviticus's "abominations" studied by Mary Douglas. It doesn't have scales, but it doesn't have legs, either. And science studies can't figure out if it can eat it.

### From Author Function to Discipline Function

These preoccupations are perhaps more important in the United States than in France, where, although the academic system is more rigid and less open to cross-disciplinary work than here (to the extent that fields like gender or cultural studies are given almost no academic space) there is also a sizable extrauniversity market for intellectual productions. Much of what we call French theory has developed and circulated in this market—a market that seems less concerned with disciplinary affiliations (at least in the case of those authors who manage to gain sufficient fame to operate in it).

The presence and influence of a peculiar institution such as the Collège de France greatly contributes to such an extrauniversity, author-based economy of intellectual products. As a highly prestigious and elitist institution that operates outside and above the normal academic system, the Collège can occasionally canonize remarkably hybrid authors like Barthes and Foucault (and allow them to define the titles and descriptions of their chairs). Because it embodies the state's authority—an authority that would not mean much in the U.S. academic system but is remarkably effective in France—the Collège can lend legitimacy to the idea that intellectual work can be almost discipline free (when done by appropriately anointed *maîtres à penser*).

These differences extend to the modalities of cultural and disciplinary reproduction. Unlike the university system, in which disciplines gain authority and reproduce themselves by placing an increasing number of their students within that very system, authors who operate in the French nonuniversity sphere tend to reproduce *themselves* (not so much their disciplines). They are more like "stars" (albeit of smallish and self-selected communities) rather than "founding fathers" of specific disciplines.

Perhaps one could say that this section of the French intellectual market is more author based, while in the United States disciplines and fields seem to dominate intellectual taxonomies and hierarchies of academic value. We even feel the need to create a discipline-like term such as "French theory" to designate some-

thing that was not seen as a discipline, field, or school in its country of origin. The coining of the term "Yale school" may reflect these dynamics. It seems, then, that in the United States the "discipline function" has taken over many dimensions of the "author function."

These two different economies of intellectual capital are inscribed in the different styles of the texts they produce. U.S. academic productions tend to follow disciplinary styles (types of narrative, footnote apparatus, standards of evidence, etc.) and are usually published by academic presses. French theory, having been articulated, published, and rewarded in a different system, tends to develop styles that are neither those of U.S. academia nor those of American academic authors who have crossed over into commercial publishing (and because of the different structure of the two intellectual fields, such a crossing carries very different connotations in the two countries).

French theory seems simultaneously too difficult and technical to be aimed at more popular, nonacademic audiences (hence the frequent critique of it as "jargon"), and too little "disciplined" to fit easily into U.S. academic taxonomies. This taxonomical puzzle informs the tendency, among sympathetic U.S. academics, to see them as providing "interesting" suggestions and ideas but not discipline-based claims and positions that the corresponding U.S. fields should feel compelled to engage with. More specifically, because academic disciplines tend to be attached to subject matters that are often defined in terms of their material features (specific natural processes, geographical areas, historical periods, kinds of texts, or categories of practices and professions), academics have a hard time figuring out what French theory's "subject matter" might be. Failing to do that, they often end up reading French theory as providing "methods," or to cast its authors in the category of "cultural commentators."

That French theory has been called contradictory names in the United States (from too radical, nihilistic, and flaky, to dangerous or even potentially Nazi friendly) suggests that we are facing a true translation problem—not translation between languages but between different intellectual economies, different "forms of life." It is also interesting that some U.S. academics seem to react to French theory the way people react to controversial artwork. Not only do some love it while others hate it, but the expression of these reactions often exceeds the vocabulary of traditional academic judgments. Unable to fit French theory into U.S. academic taxonomies, its authors are placed into the ultimate category of academic "otherness": the artist. Unable to translate the "quality" of these authors' work into categories such as expertise or knowledge, or to compare them with "the best in their field" (because it's not clear what that field





