Bruno Latour is, with the possible exception of Thomas Kuhn, the most widely influential student of science and society of the last fifty years. Like Kuhn, his most influential writings are more philosophical than scientific. His writing style, focus, and concepts address the interests of a wide audience and have gained him credibility across the intellectual landscape. He has been concerned throughout his career with the nature of “the social,” but as a philosophical and ethnomethodological idea rather than a (social) scientific concept. The difference between social theory driven by ethnomethodology and sociological theory may help explain the wide influence of Latour’s theories. Briefly, ethnomethodology describes the accounts people give of their own lives and sense-making activities; it does not share the theoretical or methodological toolkits, scientific and causal perspectives on and assumptions about social life characteristic of sociology as a scientific enterprise. That difference is at the same time a source of tension between Latour’s “sociology of associations” and classically driven sociology. Latour views “the social” within sociology as too general, too encompassing, and a “garbage” or residual category.

Latour was born into an elite family of wine growers in Beaune, Burgundy (close to Dijon, France) in 1947. His earliest studies in Dijon focused on theology and philosophy. He did a dissertation on the Catholic writer Charles Péguy. Latour received his doctorate in philosophy from the University of Tours in 1975. The French philosopher Michel Serres was an
early influence on Latour. Latour was drawn to Serres’ opposition to a privileged metalanguage for science, and his idea of translating between accounts. Serres’ (1983) use of Hermes (and more recently angels) as a model for the messenger who translates between accounts and domains also appealed to Latour. Latour’s mature social theory is greatly indebted to Serres.

Latour was stationed in Africa during his military service. Under the guidance of anthropologist Marc Augé, Latour carried out a study of colonialism, race, and industrial relations in Côte d’Ivoire. In 1975, with the aid of an invitation from his long time neighbor in Dijon, the neuroendocrinologist (and future Nobelist) Roger Guillemin, Latour moved to La Jolla California to begin his celebrated laboratory ethnography at the Salk Institute. The study was carried out in collaboration with sociologist Steve Woolgar and funded by Fulbright and NATO fellowships. Latour taught at the École des Mines de Paris’ Centre de Sociologie de l’innovation from 1982 to 2006. He is currently Professor in the Centre de sociologie des organizations and vice president for research at the Institut d’études politiques de Paris.

Latour and Woolgar’s pioneering laboratory ethnography Laboratory Life: The Social Construction of Scientific Facts (1979) helped launch the field of science and technology studies, already by then a developing research arena still trying to find its way onto the academic stage after a decade of research, publications, and meetings. This book (reissued in 1986) contained an agenda that unfolded into a career that has taken Latour far beyond science studies. That career has followed to some extent at least the path to becoming a dominant French philosopher schematized in sociologist Michelle Lamont’s (1987) analysis of the career of Jacques Derrida. Briefly, some of the factors Lamont mentions that apply to Latour are: strong theoretical trademark; diffusion potential based on being ambiguous and adaptable; addressing fundamental questions and transcending classical works; and diffusion by prestigious scholars and journals.
Reviewers consistently describe his works as provocative and important, radically original, witty, stylistically dazzling, and bold in their approach to problems everyone has become embroiled in. These problems have been generated in the contexts of structuralism, postmodernism, grammatology, narrative, and social and cultural critiques of history and theory.

In an intellectual world characterized by widespread skepticism about the status of sociology as a science (a skepticism that has a foothold even within the sociological community), Latour’s criticism of the scientific claims of traditional sociology has been readily embraced and his status enhanced. His antipathy to sociology and to causal science has driven him away from sociology and anthropology (in spite of his self-definitions) and toward philosophy.

Latour is a founding member of the Society for Social Studies of Science (1975), a former president of the society (2004-2005), and a recipient of the society’s J.D. Bernal Prize for distinguished contributions to the field (1992). Latour presented some initial findings from the Salk study at the first meeting of the society (November 1976) in a paper titled “Including Citation Counting in the System of Actions of Scientific Papers.” There are already hints in this paper of an actor-network theory (ANT), Latour’s major contribution to social theory (e.g., as explicated most recently in Latour, 2005). The development of ANT is also the work of Michel Callon and John Law (e.g., Callon and Law, 1988, 1989).

THE SOCIAL AND INTELLECTUAL CONTEXTS

By the time Laboratory Life was published, Karin Knorr-Cetina, Steve Woolgar, Doug McKegney, Sal Restivo (with Michael Zenzen), and a few others were already engaged in field studies of science. Sharon Traweek, who would become one of the most prominent anthropologists of science, was already working outside of this network at the SLAC national accelerator laboratory in Stanford, California. By the early 1980s, the work of the ethnographers
had revolutionized our understanding of scientific practice. In combination with the studies undertaken by Harry Collins, Trevor Pinch, David Edge, Michael Mulkay, David Bloor, Donald MacKenzie, Steve Shapin and others (primarily representing the Edinburgh and Bath schools) on replication, discourse, mathematics, and social histories of science, the ethnographies of science produced a new narrative in answer to the question “What is science?”

Latour is one of the most prominent guides to our liminal times. The liminality of our era reaches to most of the fundamental categories and classifications that have guided human cultures for millennia in some cases and for the last few hundred years in the case of industrial societies. This liminality is driving some of the most significant and influential intellectual movements of our era. Nature-society, human-machine, male-female, person-fetus, and life-death are among the powerful dualisms that have become dramatically problematic. The very idea of science (along with those “good” terms rationality, truth, and objectivity) has been embraced by this liminality that threatens to engulf all of our values, goals, and gods.

Traditional dichotomies have given way to complexities, non-linearities, and chaotic, fractal, and multi-logical ways of thinking, speaking, and seeing. We have encountered new phenomena across time and space on and off the planet; engaged new ideas, experiences, and values from east to west and north to south (politically, economically, and culturally); and we have endured enormous leaps in our knowledge about how the world around us works. The result is that we have been forced into new epistemological and ontological territories. It is important not to ignore the cultural inertia that sustains classical dichotomies. That inertia is fuel for caution when reading Latour’s criticisms and challenges. Nonetheless it is difficult to ignore the signs of worldview and paradigm shifts and essential tensions that are widely visible features of our everyday and professional lives.
Our liminal era is producing hybrid ideas and concepts and monstrous entities on a new scale. One day we are accosted by cyborgs, the next day by robosapiens; cloned sheep march with “natural” cows and horses; mice are patented; some women sell their eggs, some men donate their sperm. No one has exploited this situation on the public and professional stages better than Latour. His prominence has tended to obscure the innovative contributions in this arena by feminist social theorists, beginning with Mary Daly in the 1970s and including scholars such as Donna Haraway, Gloria Andalzua, and Susan Leigh Star. Latour, nonetheless, has been a leader in exploring new ways of reworking our systems of categories and classifications. He has been among the leaders documenting the changes in worldview our emerging human ecologies are calling forth. Such efforts, now as in all liminal eras, necessarily strike us as awkward, counterintuitive, and obscure to different degrees. Latour’s particular mix of counter-intuitives, even where his critics consider him wrongheaded and misguided, has the virtue of drawing our attention to the limits of our reigning categories and classifications. In a world of hybrids, monsters, and uncertainties it should not surprise us that Latour has produced theories and concepts that are themselves hybrids, monsters, and embodiments of uncertainty.

Latour can appear on the one hand as a charlatan and on the other as a creative strategist in the midst of uncertainties and complexities. Like science in one of his best known graphics, he is Janus-faced. One face knows, the other face does not know yet. This image gives us “science and technology” on the one hand and technoscience on the other hand. Perhaps to understand Latour we must look both ways – forward and backward in time. His advice is to recognize that we need to shift our activities and viewpoints just as science, nature, and actors/actants in general shift theirs. Here we have the foundation for a strategy that avoids dichotomies old and new as we move through time and space. Latour is not dogmatically
opposed to dichotomies *per se*, only to those that are uninteresting and obstruct our research (Gane, 2004: 79).

Prior to the emergence of science and technology studies (variously science studies, technology studies, social studies of science and technology, and the new sociology of science) in the late 1960s, the question “What is science?” was primarily addressed by scientists themselves, philosophers and historians of science, and science writers. Sociologists of science also studied science but they were not seen by scientists as encroaching on their jurisdiction because (1) they championed science and viewed themselves as scientists, (2) they studied the sciences scientists themselves viewed as embodying the best of what science could offer the world, especially physics, and (3) they did not claim any analytical purchase on the content of science.

The new sociologists of science associated with science studies included social critics of science (such as Restivo, Ravetz, H. and S. Rose, and Levidow) but for the most part still adopted an uncritical to worshipful orientation to science (in the works, for example, of Bloor and H. Collins). But it was their claim that they were now prepared to study the actual content of science that would eventually upset some scientists and many philosophers and historians of science. Latour became the whipping boy for these defenders of the faith because he was so outspoken, and so widely and wildly visible through his writings, lectures, and interviews. Underneath the attacks on Latour and science studies was a fervent resistance to sociology as a science, discipline, and profession and a widespread ignorance about the nature and findings of sociology. The so-called “science warriors” who initiated the science wars (see, for example and notably, Gross and Levitt, 1994) read postmodernism in general and the idea that science was socially constructed as meaning that science was arbitrary, not objective, and more fiction
than truth. The science wars of the 1990s brought some scientists and philosophers into open conflict with social scientists and humanities scholars over the nature of science. Briefly, the conflict pitted “realists” (who believed in an objective “reality out there”) against “social constructionists” whom they assumed (incorrectly) to be making relativist claims and challenging the validity of science (for the details of this controversy, see Restivo and Croissant, 2007: 225ff.). Latour, while trying in different ways to mollify the scientists and philosophers, has at the same time joined them in opposing a certain kind of sociology or perhaps sociology per se. Latour does not want to alienate the scientists who are his “subjects” and an important segment of the audience he wants to cultivate. One of the things he shares with many physical and natural scientists is a skepticism about the value and even the possibility of a scientific sociology.

THE MAJOR WORKS

_Laboratory Life_, Latour’s best known work (co-authored with Woolgar), was part of a movement that reworked our conventional ideas about how science works. The book is discussed in more detail further on. In _Science in Action_ (1988), Latour offered readers a systematic rendering of this reworking, and showed us how to think anew about science and technology. He tied together the major achievements of science studies, namely the emphasis on practice, construction, the central role of inscription, and the institutional context of modern science. Latour turned these achievements into a general theory of science as a network building activity. With this book, we are a few steps closer to articulating the actor-network theory adumbrated in _Laboratory Life_. Latour’s next book, _The Pasteurization of France_ (1988), contributed to the development of actor-network theory by demonstrating that it takes more than a great man to produce and ground a discovery.
Louis Pasteur (1822-1895) is best known to the general public for inventing pasteurization. He contributed to reducing deaths due to puerperal fever, and developed the first rabies vaccine. Latour demonstrates that Pasteur’s success in developing pasteurization was dependent on a network of forces that included public hygiene actors, physicians, and government interests. Pasteur’s triumph – substantively and methodologically – was the outcome of competing forces and interests within a specific historical context. Pasteur’s success (as an actant in a network of actants) in relation to other microbiologists was dependent on mobilizing various elements of the French public from farmers and industrialists to scientists and politicians.

Latour argues that society and scientific facts co-create each other simultaneously. Two Latourian axioms begin to come into sharper focus in this book. One is that it doesn’t make sense to think in terms of “science and society;” the other is that there is no way to “reduce” the case of Pasteur to disciplinary sociology. But where Latour and his acolytes see the triumph of “irreductionism” over sociological reductionism, some critics see just another example of sociological analysis (e.g., see especially Restivo, 2005, but also Star and Griesemer, 1989; Amstersdamska, 1990). The problem here turns once again on the assumptions ethnomethodologists make about the world compared to the assumptions of scientific sociologists. Latour does not admit social facts amenable in the manner of Durkheim to scientific qua theoretical analysis. Therefore, Latour views sociology as reducing social life to scientific explanations; thus his alternative notion of “irreductionism.” If we think of this sort of strategy in relation to physics instead of sociology, what could be made of an ethnomethodological physics? The very idea is self-destructive. If one assumes the reality of a physical world, the analysis of that world in terms of disciplinary physics is not reductionist. If
one assumes the reality of a social world, the analysis of that world in terms of disciplinary sociology is not reductionist. Latour does not admit such a social reality, so in his terms scientific sociology is by definition reductionist. (see the discussion on the debate between Latour and Bloor below).

In *We Have Never Been Modern* (1993), Latour continues to confound the taken for granted boundaries that separates humans and things, the physical and the social sciences, and the sciences and the humanities. Where hybrids abound, the myth of modernity totters. This book is a way station on Latour’s path to an increasingly systematic and well-articulated actor-network theory. If modernity is a myth so must be one of its defining features, the idea of progress.

Latour’s *Aramis* (1996) is a cautionary tale about technology and progress. Reviewers have described the book as “quirky” and filled with “stylistic excrescences” on the one hand, and as “eminently readable” and “strange and deep” on the other. Latour tells the story of a robotic transit system the French government designed for Paris during the 1970s and early 1980s. He tells this story by inventing a new genre, “scientifiction.” The book interweaves fictional and real characters into a Rashomonesque tapestry designed to demonstrate once again the limits of sociology and the promise of actor-network theory. This book more than anything else Latour has written demonstrates his impressive capacity to mobilize wit, style, concepts, perspectives, bibliographies, empirical facts, and theoretical resources to create challenging hybrid theories. Even those who do not agree with his understanding of the sociological enterprise must conjure with an approach to reality which adopts sociology as one and only one of the resources to be brought to bear on a question or problem. It is difficult to argue with this approach, one that Nietzsche (1887/1956: 255) long ago anticipated when he argued that the more eyes one brings
to a situation the more objective the viewing will be. The heterogeneity of sociology itself affords us many different sociological eyes with which to view social reality. Symbolic interactionism in particular offers an important approach to the social that is in some ways competitive with ANT and in others a resource for ANT.

*Pandora’s Box* (1999) is purportedly Latour’s reply to a scientist friend’s question: “Do you believe in reality?” Latour mobilizes ANT in defense of the reality of science studies and the flawed nature of his friend’s question. He begins by rehearsing the contributions of science studies to our understanding of science and reality. This is followed by case studies in which Latour’s dictum “follow the scientists” is the research imperative. Latour’s account of science studies is designed to answer skeptics and critics of the field. From the very beginning, science studies has been about documenting in ethnographic detail and with Geertzian “thick descriptions” (Geertz, 1994) the details of scientific practice. Latour, more than most of his colleagues, is concerned to bring into sharp relief the ways in which technology and science, the material and the human merge as our pictures of reality emerge, evolve, transform, and stabilize. Where and why given all this complexity did the idea that there is a reality we can fathom that is independent of our humanity come from? Latour wants to be the champion of ordinary people who are submissive to and intimidated by the warring claimants to “the facts” and ultimate truths.

*Politics of Nature: How to Bring the Sciences into Democracy* (2004) evidences the culmination of Latour’s evolution from philosopher to sociologist and anthropologist to über-philosopher and naked metaphysician. Even if and where his metaphysics is in some self-contradictory sense “empirical,” it is a philosophy more idealistic than realistic. The title and subtitle sizzle with the promise of saving us from ourselves, or more correctly from our
categories and classifications, and from political tendencies driven by outmoded worldviews. In spite of his penchant for the empirical, his profound understanding of science and technology as interwoven practices, and his decades of critically dismantling our conventional ideas about science and society, in the end he has divorced himself from real problems, practical solutions, and the guidelines of the ecological-sociological imagination. We nonetheless are forced to engage with the limits of our taken for granted categories and classifications, and this is why the book is worth out attention.

Latour, who has traditionally shown little concern for a normative politics, now turns to a politics rooted in Plato’s allegory of the Cave. He argues that that myth has given the West its ideas about science and society and the concept of the philosopher-scientist. Unlike the rest of humanity who only have access to the Cave’s shadows, the philosopher-scientist can travel between the world of truth and the world of shadows, the social world. The myth of the Cave becomes a new starting point for an old idea in science studies: we have to distinguish the myth of Science from the actual practices of the sciences. Latour argues further that we have to on similar grounds distinguish the power politics of the Cave from politics in action. These distinctions flow from and reinforce the Latourian project of blurring the distinction between nature and society and between things and humans. The point of his argument is that we should reach our views on reality, the external world, and nature not by way of the travels and tales of scientists moving between the worlds of truth and the social world but rather through a representative “due process.” In place of an assembly of things and an assembly of humans, Latour proposes a new constitutional politics in which there are no special envoys and no barrier to go over and come back from. The sciences and one could say “the politics” are no longer concerned respectively with nature and with interests. Scientists and politicians now work
together to construct our view of reality. Curiously (but in a way that is consistent with the ethnomethodological program), the due process that gives us reality with representation leads us to all of the old forms – a reality out there, subjects and objects, humans, a cosmos – we constructed under the old constitution.

In *Reassembling the Social* (2007), Latour mobilizes all of his resources to mount a focused challenge to reigning ideas about society and “the social.” He reiterates his claim that following Durkheim’s imperative to explain social facts with social facts means “explaining” stable things in terms of other stable things. However useful this methodology may have been historically it is now obstructing and obscuring our ability to understand social life. The old theories and methods left out too many “things” or “facts” that enter into the social domain. These other things and facts cannot be taken into account if we think of the social as a kind of thing, a level of reified material reality. To understand scientists, we must follow them and document all the connections they make and engage; to understand society, we must follow it everywhere it goes and document all the connections it makes and engages. The new social science must focus on the process of assembling the social without prejudging what is and what is not social. Here, then, is the introduction to actor-network theory that many of Latour’s admirers and critics have been waiting for. Here is Latour assembling actor-network theory.

THE THEORY

Working out actor-network theory has leveraged Latour’s development of an alternative sociology, a sociology of associations opposed to a sociology of the social. Latour views this distinction as parallel to pre-relativistic physics (conventional sociology with its Durkheimian roots) versus relativistic physics (the sociology of associations, grounded in ethnomethodology, material-semiotics, and most recently in the work of Durkheim’s contemporary Gabriel Tarde).
The main methodological principle emerging out of Latour’s studies and made explicit in *Science in Action* (1987) was “follow the scientists and engineers.” This was the portal that led to ANT.

Already in the first chapter of *Laboratory Life*, Latour (with Woolgar) begins to dismantle the very idea of the social. Their concern with ‘the social’ is different, they stress, from that of traditional (notably Mertonian) sociologists of science. It has become increasingly clear that Latour’s understanding of ‘the social’ is not just different from that of sociologists of science but that of sociologists in general. The focus on ‘the social’ in *Laboratory Life* emphasizes the construction of ‘sense’ in science, rather than the sorts of variables the Mertonians addressed (such as norms, rewards, and competition).

What, then, are the socially available procedures for constructing ordered accounts out of practices, discourses, and environments that appear initially to be chaotic? Some of Latour’s colleagues discuss this in terms of constructing facts out of contingencies. Latour has mounted a formidable attack on cultural patterns of practice and discourse, categories and classifications that have concretized over centuries and resist our efforts to learn anew, to adapt to new situations, and to strategize politically in the wake of new political and ecological imperatives. It is important to recognize that while the laboratory scientists are constructing order out of disorder, Latour and Woolgar are constructing an orderly account out of the initial appearance of disorder in the laboratory. Later, Latour (1988: 161) would say that one order is being created out of other orders. On the surface, the effort to make this approach seem like something innovative is belied by how closely it imitates classical ethnographies. At least some sociologists of the social appear to operate essentially as Latourian sociologists of associations but without losing the Durkheimian sense of the social.
It is instructive here to review accounts of anthropologists engaging a culture for the first time. Raymond Firth’s (1936) introduction to *We, the Tikopia*, for example, clearly describes a process of creating order out of disorder or out of other orders. Such accounts demonstrate that Latour’s effort to create a new sociology has continuities with classical ethnography. The Firth example is one of many more one could point to that demonstrate that Latour’s sociology has been a part of classical and modern sociology all along. What *is* innovative is the idea that the account given by Latour and Woolgar is not privileged over the accounts given by the scientists themselves in terms of giving us access to a sociology of science. Even here, however, we hear echoes of ethnomethodology and anticipations of the new ethnography and the commitment to making anthropology the science of giving a voice to the Other. *Laboratory Life* plants the seeds of an assault on Durkheimian sociology and of the future science wars.

The marks of ethnomethodology pervade this account, and postmodern French philosophy (notably the works of de Certeau and Serres) underwrites the emphasis Latour and Woolgar place on science as the production of ‘fictions,’ connoting here literature and writing accounts and not falsehoods. After all the exegesis and critical evaluation is completed, it will turn out that Latour and Woolgar have made an invaluable contribution to the sociology of science, independent of the distinction between the social and associations. That is, they have neither denied the ‘out-thereness’ of reality, nor the existence of facts; but they have stressed and empirically demonstrated that facts and realities are social accomplishments, the result of the practical, discursive work of scientists. On this point, they are at one with their post-Mertonian colleagues (including Bloor, Knorr-Cetina, R. Collins, Leigh Star, and Restivo), all of whom however distance themselves from Latour’s claims about the demise of the social.
In 1986, Latour and Woolgar published the second edition of *Laboratory Life*. They added a postscript and eliminated the word “social” from the subtitle. In a section on “The Demise of the Social,” Latour and Woolgar carry out the promise of their original study to understand how scientists themselves distinguish between “social” and “technical” factors. The idea of the social was useful to the Mertonians in their development of the concept of science as a social institution. It was equally useful to the Edinburgh School in its development of a sociology of scientific knowledge (SSK), constructed on the foundation of Bloor’s “strong programme.” Latour and Woolgar now claim that “the social” is no longer useful.

Perhaps the single most important focus for critics of ANT is that it seems to assign agency to nonhumans. ANT has been described by its founders as a material-semiotic method that maps relations between things and between concepts simultaneously. This means that the interactions we can observe in a bank, for example, are not just the interactions between people, but rather the network of interactions involving people, their ideas and concepts, and technologies. It is not clear why, when ANT is described in this way, it is any different from the way anthropologists view culture holistically in terms of the network of relations between socifacts, mentifacts, and artifacts (to use David Bidney’s [1967] categories). This is a good place to recall the work of Ludwik Fleck (1935/1979) who anticipated so much of Latour, not to mention Fleck’s contributions to Kuhn’s (1962) thinking. The anthropologist Mary Douglas (1986) has succinctly explicated the significance of Fleck and Durkheim for any sociology of knowledge.

Latour identifies more closely with anthropology than to sociology. The foil he makes of sociology from this position is somewhat forced. Opposing sociology from the anthropological perspective is based on a distinction without a difference, a matter of professional, disciplinary,
and historical contingencies. His identification with anthropology as an interpretive discipline (as opposed to sociology-as-science) probably allows him to mobilize more humanities scholars, and anti-quantitative STS scholars and social scientists.

Let’s look again at the example of the bank. In ANT terms, the bank is a network that can under certain conditions be treated as a unity, as an actor/actant. ANT stresses that networks are transient, constantly engaged in making and re-making themselves. Our relationships and our networks constantly have to be reconstituted, re-produced. Again, it is not clear what this idea achieves that hasn’t already been achieved by sociologists like Harold Garfinkel and Erving Goffman. These two exemplars might readily be dismissed because they are idiosyncratic in the context of mainstream sociology. But we could as easily demonstrate the point with Weber and the sociologists who have followed in his wake, Merton no less than R. Collins.

BASIC CONCEPTS IN ACTOR-NETWORK THEORY

ANT’s focus is on actants. The term “actant” appears in the work of Lucien Tesnière as early as 1959. It is also associated with the works of Greimas (1966) and Kristeva (1967). Latour introduced the term ‘actant’ into science studies to avoid speaking of ‘actors’ acting or systems behaving. It is characteristically difficult to pin Latour down on definitions which seem to flow from him like zen koans (cf. Zammito, 2004: 189). Giving him the benefit of the doubt, we can argue that this is just what is necessary in order to capture something about a world of great complexity and uncertainty that seems constantly to be outrunning our efforts to stabilize it conceptually. Latour’s critics see shallow maneuvering, comic effects, and attention-getting strategies in his work. It doesn’t help the matter when Latour himself refers to his work as a joke, and tweaks his readers with ambiguities and contradictions. We, however, have to consider
whether he has hit on a strategy that has at least temporary relevance for a period in which worldviews are undergoing stress and change (cf. Restivo, 1985: 129-156). It is important to keep in mind that the heritage of the laboratory studies has been to keep the focus on matters that are not yet settled, not yet closed, and still mired in different degrees of controversy.

The critiques leveled against Latour often mischaracterize his position. Indeed, these are critiques that have been leveled against science studies researchers in general (notably in the science wars). Latour (1999: 299-300) claims explicitly that his critics are attacking someone with his name who defends all the absurdities he disputes: that science is socially constructed, that science is nothing more than discourse, that there is no “reality out there,” that “everything goes” in science, that science is conceptually empty, that the more ignorant you are the better; that everything is political; that subjectivity and objectivity are inter-mingled; and “that the mightiest, manliest, and hairiest scientist always wins provided he has enough ‘allies’ in high places.”

How is it that critics could make such a mistake? One answer is that Latour demands with the authority of the ethnographer that we rethink ideas about science that have gone unexamined; another is that science studies has invaded territories long held by more traditional disciplines; and finally, we can’t dismiss the possibility that the very idea of a sociology of science breaches powerful ideologies of science.

Already in his presentation at the first meeting of the Society for Social Studies of Science (1976), Latour is at work redefining things in the world of science so as to extend what it includes; he begins to draw the outlines of what he will later capture in the term “actant.” In his 4S paper, he defines “literature” as a continuum which includes drafts, corrected manuscripts, private and public preprints, oral presentations of papers, posters, abstracts, and the finally
published papers, reprints and copies. The very process of ethnography forces Latour to focus on the frontiers of science, watching unsettled science where we find chains of conflict, controversy, and modalities. What Latour sees in his Salk laboratory study is not facts plain and simple, but a continuous chain of activities.

In *The Pasteurization of France*, Latour mobilizes ANT in the interest of providing what amounts to a “thick description” (C. Geertz) of actants, of an actor-network. Society is not formed with the social alone; in this particular case, for example, we have to add the action of microbes. We cannot speak of something – science – done in laboratories and then speak of groups, classes, interests, and laws in a separate narrative. Instead we have to speak of actor-networks, and instead of thinking in terms of “forces” that cause this or that, we must think and speak of “weaknesses,” “entelechies,” “monads,” or more generally “actants.” Latour uses “actor,” “agent,” or “actant” without assuming actions or properties. They are “autonomous figures,” and they can be individuals or crowds, figurative or nonfigurative.

These ideas can be very confusing, but the main thing is to avoid using the term “actor” which is often limited to humans; the virtue of the term “actant” is that it can refer to humans and nonhumans. People and things have “spokespersons” in the assemblage of an ANT and Latour borrows the term “actant” from semiotics to describe what the spokesperson represents. He now can describe the power of the scientific laboratory in terms of the number of actants it can mobilize around its findings and interests. In general, then, power is a function of the number of allies “the laboratory” or anyone, or any network can shape and enroll – mobilize – to support its findings and interests in an agonistic arena. If Pasteur speaks for microbes, the Curies can be said to speak for plutonium, Cantor for transfinite numbers, Einstein for photons, and so on. Perhaps the simplest definition of actant is the one Latour offers in the glossary for *Politics of*
Nature; but he once again confuses his readers by pairing actor and actant as one entry. Actant applies to humans and nonhumans, he writes. This is followed by “an actor is any activity that modifies another activity in a trial.” Presumably, this is what he means by “actant,” the non-anthropomorphic sibling of “actor.” It helps to reflect on the use of the concept of actant in semiotics to reveal more clearly what Latour is trying to accomplish.

Originally, the concept of actant was invented to help readers of stories identify characters as one sort of actant or another: helper or opponent (the conflict axis); subject or object (the project axis); and sender or receiver (the communication axis). Characters could also be combinations of two or more actants. This framework offers a primitive narrative organization for a fairy tale. Something or someone is missing as the result of a villainous act. The subject lacks this object. The sender and the receiver contract to retrieve the missing object. The sender is high on the hierarchies of status, power, and privilege, which means the receiver incurs an obligation in this contract. The subject, with or without a helper, retrieves the object in combat with an anti-subject (opponent). This is known as “the test” (Hawkes, 1977; Tesnière, 1959). Latour (1987: 89-90) has translated this framework and imported it into science studies. The ‘things’ that stand behind the texts of science are like the heroes of our epics. In some stories, heroes defeat dragons and save maidens. In some stories, hero scientists “resist precipitation” or “triumph over bismuth.” The essence of the hero does not appear to us all at once but over time and retrospectively. What at one point is a list of actions eventually becomes clear as an essence.

Actants are characters, and they require spokespersons to turn them into actors (Akrich and Latour, 1992). By pairing humans and nonhumans, Latour makes it possible to assemble the greatest number of actants in a single world, an assembling carried out by the collective. The
result is that there is no longer any need “to defend the subject against reification, or to defend the object against social construction. Things no longer threaten subjects. Social construction no longer weakens objects” (Latour, 2004: 80-81). The creation of an actor-network is known as “translation.” Notice that Latour considers it useful to focus on a single actor (read “actant”) and to see translation from that actor’s perspective. The process of translation occurs in four stages. First a focal actor identifies and aligns itself with other actors who share its identities and interests. The focal actor sets itself as an “obligatory passage point” (OPP), and in this way renders itself “indispensable” (Callon, 1986). This is known as the problematizing stage. At the interessement stage, the focal actor is engaged in convincing others to accept its definition(s) of identities and interests. The stage in which the others accept the focal actor’s definition(s) is known as enrollment. The fourth stage, mobilization, solidifies the shape, form, and scope of the network.

Six additional concepts help to flesh out the basic conceptual skeleton of ANT: inscription, irreversibility, punctualisation, depunctualisation, token, and technoscience. Inscription creates technologies designed to protect the interests of actors and networks (cryptography technologies are a transparent exemplar). Keeping in mind that interessement involves interrupting and ultimately triumphing over competing definitions, the idea of irreversibility refers to how likely it is to return to a situation in which alternative possibilities exist (Walsahm, 1997). As Hardy, Phillips and Clegg (2001: 538) note: “These strategies help to create convergence by locking actors into the network. The more fixed or stable it appears, the more ‘real’ and durable it becomes, and the less controversy and ambiguity are evident….The aim, then, is to put relations between actors into ‘black boxes’ where they become a matter of indifference – scientific ‘facts’, technical artifacts, modes of thought, habits, forces, objects.”
The laboratory studies, viewed from this perspective, describe the process of translation from macrocosm (larger “outside” world) to microcosm (the laboratory), from laboratory activity to laboratory inscriptions, and from the laboratory back to the outside world (Callon, Lascoumes, and Barthes, 2009).

Punctualisation refers to the fact that the components of complex systems, such as those of an automobile, are hidden from the view of the user. If a car breaks down, this can provoke the driver to recognize that the car is a collection of parts rather than just a vehicle that s/he can drive from place to place. This kind of awareness can also be kindled when parts of a network begin working in conflict with the network as a whole. This is referred to as “depunctualization.” Social order in general and the working automobile in the example above are achievements of the actants interacting within actor-networks. Such creations are known as “tokens” or “quasi-objects” and they get passed from actants/actors to actants/actors across actor-networks. The more tokens circulate through a network the more they get punctualized and reified; a decrease in the circulation of a token results in depunctualization and a decrease in reification.

Early on in Science in Action, Latour (1987: 29, 174-175) “forges” the word “technoscience” in order to avoid endlessly writing “science and technology.” Technoscience refers to “all the elements tied to the scientific contents no matter how dirty, unexpected or foreign they seem.” This leaves “science and technology” (in quotes) “to designate what is kept of technoscience once all the trials of responsibility have been settled”. We can see Latour’s ANT strategy at work here. The term ‘technoscience’ appears to have been introduced into philosophy by the Belgian philosopher Gilbert Hottois (1984) in the late 1970s. Hottois’ concept of technoscience was not tied to a social theory of science but rather to Percy Bridgeman’s
notion of operationalization. In both cases, the term technoscience is designed to broaden our notions of science and technology. Notice that for Latour, technoscience implies a stage as well as a new stability. It is a stage within which science and technology are composed of many different kinds of elements (or actants). Once the trials of responsibility are settled, we can once more distinguish science and technology (or in Latour’s more exact terms, “science and technology”).

Latour’s translations within science studies revolve around mobilizing the concept of the actant and ANT and result in a clear separation between ANT and the strong programme (hereafter SP) in the sociology of scientific knowledge. Latour’s principle antagonist is the author of SP, David Bloor. Latour’s theory and critique of “the social” achieves a dramatic focus in his “vehement” (Bloor, 1999: 81) criticism of the sociology of knowledge and of SP. Bloor (1999: 82) claims that Latour’s criticism of SP systematically misrepresents the programme and his alternative, “in so far as it is different, is unworkable.” Latour and Bloor differ on what to do about the “subject-object schema.” Latourian sociology simply rejects the schema. Bloor points out that there are many levels and interpretations of the schema, and that at least one is sociologically viable.

**Latour versus Bloor**

Latour criticizes sociology and SP for relying on “Society” to explain things. He is opposed to a Durkheimian sociology that explains social facts with social facts, and a SP that uses Society to explain Nature (Latour, 1992: 278). The issue for Latour is that sociology and SP do not take into account the ways in which non-social things and processes contribute to “Society,” that is, to the social organization of our lives. Latour adopts the term “anthropology” for a project that is non-sociological, non-reductionist, non-naturalistic, and non-causal and not
anything like the anthropological tradition that runs from Durkheim to Mary Douglas. That tradition is central to SP. Latour mistakenly assumes that the goal of SP is to use society to explain nature. The goal of SP is in fact to explain not nature but “shared beliefs about nature” (Bloor, 1999: 87). The debate between Bloor and Latour is not easily resolved. The reason in part is that differences in metaphysics (as Latour recognizes) and more broadly differences in worldview are at issue. We must in the end compare and contrast entire worldviews rather than little bits and pieces of epistemology, methodology, and ontology, a general strategy elegantly outlined by the philosopher of science Clifford Hooker (1975).

Latour wants to interrogate everything: science, nature, society, causality, and so on. Bloor wants to carry out interrogations on the grounds of the successful sciences. Parenthetically, notice that this could easily degenerate into a conflict between a Latourian antagonism to causal science and a Bloorian scientism. In his reply to Bloor, Latour raises the banner of anti-absolutism, the very banner Bloor waves in the face of those SP critics who understand relativism as the opposite of realism. Bloor has consistently stressed that relativism in SP is opposed to absolutism and even defined relativism as “distinterested research” (a classic philosophical and sociological definition of science; v. Barnes and Bloor, 1982: 47n).

Could it be that the result of this debate is to demonstrate that Bloor and Latour are at one on the nature of science and society? This would not be an unusual outcome. After all of his efforts to distance himself from Lakatos in Against Method (1975), Feyerabend joins him. And all radical appearances to the contrary, the more Kuhn explains Kuhn the clearer he makes it that he is more a traditional internalist historian of science than a sociologist of knowledge. The outcome of all of Latour’s interrogations is that he lands on Bloor’s territory. When we have interrogated all of the old forms – the subject/object schema, external reality, society, and nature using Latour’s
(2004) proposed new constitution and parliament, in the wake of the due process he demands, all of the old forms will be back. Latour’s slogan is “No reality without representation.” If all the forms are the same before and after representation, before and after due process, what has Latour added to our discourse except the Strong Programme and SSK? Bloor, indeed, claims just this, that Latour (1999: 113) has given us SSK “couched in a fancy vocabulary”

CRITIQUE AND OPPOSITION

Four main questions have occupied Latour’s acolytes and critics – is he a constructionist (social or otherwise); is he a relativist; does he grant machines and material objects agency; and what discipline can he be assigned to? Latour himself claims that he is not a social constructionist; he is a relativist only in the same way, he says, that Einstein is a relativist; he is frustratingly ambivalent about the agency of objects; and if not a dominant French philosopher he is at least an über-denker. Forced to discipline him, I would choose philosophy over sociology or anthropology. Let’s look more closely at Latour and social constructionism.

Latour’s early education and training in philosophy and theology and his continuing exercise of philosophical analytical and discursive strategies in his research and writing underpin his defense of metaphysical narratives. His view of how the world works and what it “is” bears some resemblance to the views of the late physicist David Bohm. Bohm was at least as sensitive as Latour to the volatility and dynamics of the material world as well as the world of humans, their languages, and their cultures. Bohm’s science is strikingly consistent with Latour’s metaphysics (Restivo, 1985: 121-125).

As we approached the second millennium, the flux of categories and classifications and the proliferation of hybrids and monsters, increasingly came to dominate our everyday lives and the horizons of humanity. These are times that require great courage and imagination to engage,
so it is not surprising that only a few thinkers like Latour come to the fore. But his work and his ideas are strengthened by the fact that a thinker like Bohm, stressing science as opposed to metaphysics, has seen contemporary liminal dynamics through the very lenses that Latour is seeking to change. Bohm (1976) even championed a verb based language as one way of coordinating language and reality.

Does Latour consider himself a relativist? Yes and no. Is his sociology more metaphysics than science? Yes and no. Do machines have agency? Yes and no. It’s easy to view Latour as a sort of zen master, prying open black boxes, challenging the taken-for-granted, shaking us out of our complacency about everyday language – by drowning us in language games designed to enlighten us. There is method behind what sometimes can appear to be zen sociology or less charitably, just a little joking. When the philosopher Graham Harman described Latour as an empirical metaphysician, Latour countered by stressing that while philosophy and metaphysics are significant aspects of his research program the main thrust of his approach is to engage in empirical research. He accepted with good humor someone’s description of him as a “serial re-describer.” Latour’s approach has roots in ethnomethodology, arguably a methodology of translation (translating phenomena into the language of ethnomethodology) but certainly not a scientific methodology. Nonetheless, Latour’s book *Politics of Nature* is nothing if not an exercise in unadulterated metaphysics. He addresses issues ranging from science and philosophy to world politics, ecology, and the body. He is always “trespassing” onto the territories of other scholars and specialties. This is, on the one hand, characteristic of the general theorist, and especially of the philosopher. On the other, this opens Latour up to attacks on many different fronts.
The difference between the sociologists of the social and the sociologists of association is a red herring. Latour claims that the former can be called on to study stable social orders, but that the latter are needed to study and understand social orders in process. But there is no reason to make such a distinction unless one is ready to defend the claim that in any field of study you need two different sciences to study statics and dynamics. For the Durkheimian “sociologists of the social,” the other issue is that if we make “everything” social, we lose sight of the unique qualities that obtain in the social interactions of humans – the ties that bind: belongingness, community, solidarity, emotional coupling. Indeed, when he chooses Tarde over Durkheim as his starting point, he reveals why his social theory does not rise to the level of a scientific sociology. At the same time, he ignores the influence of Tarde on pragmatism and Chicago school sociology. Tarde is a more subtle sociologist than I can demonstrate here, but see Tarde (1899/2009).

Tarde locates the origin of social changes in the “individual” and the “single mind” (cited in Latour, 2005: 15). For Durkheim, society precedes the individual; the individual is a social unit, a social fact. Humans come onto the evolutionary scene not as individuals who then at some Hobbesian point choose to come together socially by way, for example, of a social contract. Rather, humans emerge everywhere, always, and already social. Latour’s preference for Gabriel Tarde over Emile Durkheim amounts to an attack on the sociological imagination (C. Wright Mills), the sociological cogito (Randall Collins), social constructionism (Sal Restivo, Karin Knorr-Cetina), and the form of symbolic interactionism inspired by Anselm Strauss. In Latour’s defense, it is important to note that the sociology of associations stresses the in-betweenness of things classically held to be inside individuals. Emotions, for example, are in-between, relational; so is consciousness. When humans and objects interact, relational
phenomena emerge. There are forms of emotion that characterizes our relations with the shells we pick up as we stroll along a beach, and with the computers and robots we interact with. The more interactive and the more humanoid the object the more salient the emotional relationship will be. In this sense, then, the concept of actants and networks of actants can be enlightening. The problem is not to lose sight of the unique nature of the relationships between human beings, and the roots of a certain privileged form of being human in those relationships. We are different kinds of humans interacting with each other and interacting with objects.

Latour is a formidable social theorist, but this does not automatically make his work sociological. His criticism of the sociologists of the social ignores the fact that sociologists as different as George Lundberg, Nicolai Bukharin, Howard Becker, and Randall Collins have addressed the very issues and problems Latour claims require ANT. Like many philosophers, he is prepared to claim jurisdiction over a discipline (sociology in this case) and to idiosyncratically define the nature and subject matter of the field. If sociology has to be reconfigured, so be it. Latour has tried to do this without understanding first what it is that sociologists do. He has abandoned social constructivism/ ionism without persuading many of his science studies colleagues that he has discovered an alternative to the constituting activities of social relations. And that, after all, is what social construction means: we have no recourse outside of our interactions – our social humanity - for constituting the world (Restivo, 2005).  Latour has underestimated the diversity of contemporary “sociology.” This has trapped him in a caricature of a universe he doesn’t inhabit and sees only from afar. It is important to continuously keep in mind that the issues here turn on whether we accept ethnomethodology as a mode or school of sociology or rather as an opponent of or alternative to sociology.
David Berreby (1994) conjured the image of a boxing match when he titled an essay, “And now [one can almost hear the implied “in this corner”], overcoming all binary oppositions…that damned elusive Bruno Latour.” Latour’s elusiveness is due in great part to the increasingly philosophical voice he has adopted combined with the wider and wider scope of the issues he has taken on. His philosophy, once empirically grounded, has moved onto a metaphysical plane divorced from the social and political realities of everyday life. If he started his career with the promise of helping to fashion a Copernican revolution in the sociology of science, he has evolved into a thinker who reminds us more of Rousseau or Hobbes. This helps explain his ready dismissal of the perspectives and findings of the social sciences. His plan for bringing the sciences into “democracy” is more Platonic and transcendental than empirical. He writes more and more in a tradition that pays serious attention to bats, armadillos, aliens, and idealized humans in trying to achieve insights about real humans and the human condition. His ideas about democracy, for example, owe more to the Platonic philosophical imagination than to empirical political economy.

His elusiveness is as much a function of his unquestionably elegant literacy and logic as it is to his cleverness, wit, and playfulness. He is without question an engineer of brilliant insights on science and society. Yet he seems to put some of his readers into an adoring trance with his neologisms, doodles, and wit. On the other hand, his critics describe him as obscure and self-indulgent. He wants a reasoned dialogue with his reader but he wonders whether he is raising questions for himself and himself alone (Latour, 2004: 6). It is always worth navigating Latour’s counterintuitives but the dangers are everywhere – solipsistic meanderings, clichés, pithy confucianisms (e.g., “today’s enemy is tomorrow’s ally”). It is the game of plurals that is his *forte*. Where once it was obvious that we had science and nature, he leads us toward sciences
Science studies has given us the sciences as social practices and discourses. This empirically grounded view of the sciences is indisputable. The further lesson that Latour draws from science studies, that sociology’s conventional toolkit must be eliminated, is far less convincing. The disagreements on this point and on the social construction conjecture with Bloor, Restivo, Knorr-Cetina, and others do not trouble Latour. He advocates something akin to Richard Rorty’s (1981) imperative to keep the conversation going. For Latour, there is nothing sinister or dangerous about these disagreements; they are merely differences to be played out on the field of “politics.” The pluralism in Latour’s theory, to the extent that it is salient, does not rise to the level of the pluralist theories developed by the feminists, black and queer social theorists, and women and minority voices in Brazil, Africa, India, and elsewhere outside the Euro-American sphere.

This then continues the conversation of the Western philosophers, and does so politely under the rules of a gentlemen’s club. It is, however, hard to ignore the vocabulary of warfare that marks Latour’s rhetoric. If this rhetoric was derived from sociological theory, from conflict theory for example, it might be more persuasive. In Latour’s hands, this rhetoric is just another strategy for mobilizing adherents and a philosophical undertaking rather than an empirically grounded political economy.

His most recent philosophy cum metaphysics – empirical or not, science-like or not - is more like the story-telling he advised as an alternative to explanation early in his career. At the end of the day, it will fall to Latour’s readers to balance the applause of his most adoring
acolytes against the damnations of his most volatile critics. This won’t be easy because Latour transforms ANT as he goes. In Latour (1999: 1), four things do not work with ANT: actor, network, theory, and the hyphen; in Latour (2005:9), ANT fits Latour’s project “very well.” Are networks, indexed in Latour (2005), the key idea, or is circulation, not indexed in that same volume, more to the point (Latour, 1999: 19)? If, however, the balance is true, and readers weigh the pros and cons fairly, they might readily and reasonably conclude that Latour is a social theorist to conjure with, a social theorist to think with, and one of the most learned and eloquent guides to our time and place as the twenty first century unfolds.

Acknowledgements. I would like to thank the following readers for their kudos and cautions, criticisms and suggestions on this essay during various stages of its preparation: Randall Collins (Pennsylvania), Jennifer Croissant (Arizona). Abby Kinchy (RPI), Michelle Lamont (Harvard), Julia Loughlin (Syracuse), Jerry Ravetz (Oxford), and Leigh Star (Pittsburgh).

Bibliography

The Writings of Bruno Latour

Books


**Articles and Chapters**


Bibliographic Essay: Reading Latour
One of the best ways to engage Latour’s ideas is to follow him through one of his many interviews. Gane’s (2004) interview allows Latour to articulate his views on sociology. See also Crawford’s (1993) interview with Latour on the eve of the outbreak of the science wars; and his review of *We Have Never Been Modern* (Crawford, 1994). For an in depth reading of Latour as a social theorist, see Zammito (2004). Zammito contextualizes Latourian social theory at the near end of the post-positivism study of science that has Willard Quine at the far end. Zammito characterizes Latour in relation to some of his more prominent colleagues in science studies including Haraway, Longino, and Pickering. Zammito tends to side with Latour in the Latour-Bloor debate. Perhaps the best introduction to Latour as a philosopher can be found in Harman (2009). Latour himself can be as good a guide to his ideas as his champions and translators. For one example, see his essay in *WIRED* (Latour, 2003). For those readers coming to Latour for the first time, Dave Harris (2005) of University College Plymouth has written an accessible guide to Latour’s *Science in Action* and the basic ideas of Actor-Network Theory. One of the earliest critiques of ANT arose in the context of the development of the concept of boundary objects in the works of Susan Leigh Star and her colleagues (Star and Greisemer, 1989; Star, 1989; Bowker and Star, 1999). For a Marxist perspective on ANT, see Rudy and Gareau (2005). The symposium they introduce highlights the most persistent criticism of Latour and ANT, the failure to address issues of social justice, inequality, and power. Among Latour’s colleagues in STS, Restivo (2005) and Amsterdamska (1990) have written essays that criticize him on the same grounds, even in the wake of his increasing focus on democracy and world peace. In her review of *Science in Action*, Olga Amsterdamska criticizes Latour for what she claims is a “might makes right” approach to understanding science, a constant rhetoric of warfare, and a confused understanding of reality; Restivo’s review of *Politics of Nature* focuses
on Latour’s turn to philosophy and the poverty of his sociology. For a recent defense of a “post-
ANT” ANT, see Gad and Jensen (2010). Gad and Jensen come close to arguing that ANT is
little more than a distillation of how to do science based on what we’ve learned from the
ethnographers of science about scientific practice but without causality and with a deification of
complexity and uncertainty.

Further reading

and Human Values 15, 4 (Autumn), pp. 495-504.


Pp. 21-47 in (eds.) Martin Hollis and Steven Lukes, Rationality and Relativism. Basil Blackwell,
Oxford.


Transaction Publishers, Edison NJ.


Philadelphia.


Press, Cambridge MA.


