Rhetoric, narrative and argument in Bruno Latour's Science in Action

Abstract:

Why does Latour's Science in Action, which approaches science rhetorically, highlighting the persuasive and political work that must be done to establish a scientific or technological fact, not examine its own rhetoric? Latour acknowledges he is making use of the the persuasive techniques he attributes to science, but to submit his own techniques to the demythologizing scrutiny he directs at philosophy of science would undercut his primary goal of recruiting readers to his colors. The paper scrutinises three central figures in *Science in Action*: science as war, as network, and as Janus-faced, and examines its disciplinary and biographical context.

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> Latour must be read reflexively. He discusses Pasteur's recruitment of unlikely allies, and we should investigate how he summons us to his colours. (Simon Schaffer, review essay on The Pasteurization of France (1991, 179.))

In this paper, I look at the rhetoric of Bruno Latour's Science in Action (1987). Although it is a relatively small part of Latour's prodigious output, and his views have changed in important ways since it was published, it remains the most detailed and lengthy exposition of his approach to the study of science and the only book that he has written in English, without a co-author. As Science in Action offers its readers a deeply rhetorical vision of the natural and social sciences, highlighting the persuasive and political work that must be done before a scientific or technological fact can be taken for granted, one might expect that it would examine its own rhetoric. To see why Latour does not do so, one must start by considering the methods that he advocates there.

Latour's writing about science in action takes it for granted that science is a textual enterprise, and that narrative is an essential moment of the work of science. He gives great attention to the ways in which narrative is employed in the service of the hegemony of technoscience, and its myriad uses in the networks of power that constitute science in the making. What, then, is the role of story-telling in Latour's telling of his own story? Like the scientists he describes, he aims to "summon us to his colours" by convincing us that we need what he has to offer. But Latour's account of science is deeply at odds with the beliefs of most scientists about the nature of science, for he aims to demythologize the authority of science without undermining his own authority. The disparity between Latour's enthusiasm for examining how others recruit, and his scanty story about how he recruits his readers, is the guiding thread in the discussion that follows.

Most scientists, and most philosophers of science, draw a sharp distinction between the "context of discovery," by which they mean the specific historical circumstances in which a scientific result is first established and disseminated, and the "context of justification," the epistemological assessment of the reasons we have for regarding those results as accurate, or correct. It follows from this distinction, as it is ordinarily understood, that while the practice of scientists may be of historical and sociological interest, it is not directly relevant to the appraisal of current scientific achievements.

Latour starts by denying the significance of this distinction. His first rule of method**,** the guiding principle of his book, is set out in the final section of the introduction in the following terms:

> (3) The first rule of method
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> We will enter facts and machines while they are in the making; we will carry with us no preconceptions of what constitutes knowledge; we will watch the closure of the black boxes and be careful to distinguish between two contradictory explanations of this closure, one uttered when it is finished, the other while it is being attempted. This will constitute our **first rule of method** and will make our voyage possible. (1987, pp. 13-15.)

"Facts," Latour argues, arise when readers are sufficiently convinced about something that there is no debate about it, and the processes which led to the successful persuasion of those readers have dropped out of view (1979/1986, p. 76; 1987, ch. 1.) In the opening pages of Science in Action, Latour compares facts to the cybernetician's black box—the term is the first phrase in the book highlighted in bold text, a device Latour uses to indicate the introduction of technical terms that are important for his argument. A black box, Latour suggests, is anything whose makeup can be taken for granted for present purposes—whether it is a reliable piece of equipment, an established scientific

result, or a person with predictable habits. As a result we need only consider its input and output, and can "black-box" its construction.

At first sight, Latour appears to be reversing the priority traditionally accorded by philosophers of science to epistemological concerns about the logic of justification, in his preference for a focus on "science in action," the hurly-burly of lab life, the struggle for funding and fame, and the way settled results in science emerge out of that context. But by the time one reaches the final chapters of Science in Action, Latour's demythologizing attack on traditional understandings of the power and authority of science he has effectively given up the distinction altogether, as nothing more than a retrospective device that conceals the way facts are constructed by science in action.

Those of Latour's critics who take him to be a conventional relativist have contended that he has no consistent story to tell about the standpoint from which he tells a story of science as a struggle for military power, populated by competing forces engaged in trials of strength. He presents himself as writing an authoratitive textbook on the nature of scientific knowledge, but it turns out that his own account of knowledge means that he cannot have the authority he claims. Latour's relativism is considerably more sophisticated than this, however: he is quite ready to admit that he is making use of the the same persuasive techniques that he attributes to science.

The principal problems the author of Science in Action faces are not logical, but rhetorical. They have to do with the way he slips from claiming that he is treating ready-made science and science in action even-handedly to ultimately privileging science in action. The textbook character of Science in Action, the listing of methods and principles, the lengthy exposition of supporting case-studies, the charts, tables and citations from the relevant authorities; all these are the rhetorical devices of authority, of ready-made science. And this is necessary, for Latour does not want to present his way of reading science as just one of many competing alternatives, but as the best. In an interview with T. Hugh Crawford in 1990, published in 1993 in the first volume Configurations, a journal

on science, literature and society, Latour, a member of the journal's editorial board, invoked the name of the century's most famous scientist in characterizing the nature of his work: "Like Einstein's relativity, it is bizarre at first, but then it is completely simple" (Crawford, 1993, 264.) Like the scientists he describes, or Machiavelli's Prince, Latour aims to convince his audience by whatever means seem most likely to assure success. In order to create the appearance of authority, the rhetorical means by which authority is achieved must not be subjected to close examination. What, then, are the metaphors that structure his account of his own methods, and the scientist's?

Donna Haraway, whose own work on technoscience has many affinities with Latour's—a term both she and Latour have adopted in order to avoid an artificial separation between science and technology—has recently contended that war and heroic action are the structuring principles in the narrative of science and the discourse of the science studies scholar presented in Latour's seminal work. On this reading, the rhetorical stance of the author and the rhetorical strategies that he attributes to scientists are the same; both are engaged in a ruthless war, a war whose history is usually written by the victors.

> This powerful tropic system is like quicksand. Science in Action works by relentless, recursive mimesis. The story told is the same story. The object of study and the method of study mime each other. The analyst and the analysand all do the same thing, and the reader is sucked into the game. It is the only game imagined. The goal of the book is "penetrating science from the outside, following controversies and accompanying scientists up to the end, being slowly led out of science in the making." (Latour, 1987, 15.) The reader is taught how to resist both the scientist's and the false science studies scholars' recruiting pitches. The prize is not getting stuck in the maze but exiting the maze of technoscience a victor, with the strongest story. (Haraway, 1997, 34.)

While there is considerable truth to Haraway's observations about the shortcomings of Latour's machismo, there is much more to Latour's story-telling practices, or his use of narrative, than the tropic system and "relentless, recursive mimesis" Haraway identifies. The warlike bombast of the stories he tells should not be taken at face value.

Latour draws on a wide variety of traditional tropes and genres in his work. At least two other tropes are equally, if not more, important in the overall structure of Science in Action. One of these is the figure of science as a network; the other figure that plays a leading role in Science in Action is Janus. Janus, the two-faced Roman god of war and peace, is at first sight, an unlikely patron for a book that presents itself as a textbook on "how to follow scientists and engineers through society" (subtitle.) The book is structured by six principles and seven rules of method, listed in an appendix; each chapter is constructed around one principle and its associated rule of method. There are also dialogues, charts, diagrams, and cartoons. But there is a performative contradiction between the finalized, authorative character of the textbook and the emphasis on following the dialogue, debate and uncertainty of science in the making, and this knot is captured most clearly in the figure of Janus. As we shall see, his own way of writing has a similarly two-faced character. He simultaneously plays within the conventions of the genre he has adopted, and transgresses its boundaries in ways that bring its narrative character, and its underlying presuppositions, to the fore.

Latour invokes Janus in the opening pages of Science in Action, asking us to give equal time to two very different ways of looking at science: the perspective of the researcher in the midst of a scientific controversy, Latour's "science in action," and the perspective of someone for whom the controversy has been settled, "ready made science." Thus he opens the introductory chapter of the book, entitled "Opening Pandora's Black Box", with three paragraph-length "scenes" (1987, 1.) Set in (a) 1951, (b) 1980, (c) 1985, they describe times and places where (a) the structure of DNA was a matter of controversy, (b) the Eclipse MV/8000 computer was being built (c) an MV/8000 was used

to display a three-dimensional image of DNA. Shortly after time (a), the structure of DNA is blackboxed, with Crick and Watson's discovery that it was a double helix; shortly after time (b), the Eclipse is blackboxed and put on the market.

These opening scenes allow him to contrast two different ways of looking at any technoscientific achievement: the perspective of those for whom it is controversial, and the perspective we have once the controversy has been closed. The first figure of the book is a cartoon illustration of Janus (Fig. I.1, 1987, 4). One face is labelled "Ready Made Science", Latour's term for the retrospective perspective on science; it looks backwards at the past, is older and bearded. The other face looks forward, is younger and clean-shaven, and is labelled "Science in the Making." There are seven more pictures of Janus distributed through the rest of the text; in each case, the god gives voice to two contradictory perspectives, on matters such as the nature of facts: "'Just get the facts straight!''Get rid of all the useless facts'" (Fig. I.2, 1987, 7); the nature of scientific and technological success (1987, Figs. I.3-5,1.1, 4.2) and the relationship between science, nature and society (1987, Fig 2.5, 3.6.) Indeed, these exchanges sum up some of the principal themes of the first four chapters of Science in Action. At the end of Part I, Latour suggests that the moral of his Janus-faced characterization of science is that we must be even-handed: we must recognize that scientists are relativists while engaged in science in the making, but realists once the controversies have been settled. On this account, the two voices are complementary, two different ways of approaching science, each of which needs to be understood (1987, 100.)

The problem of resolving the relationship between Janus' voices in Science in Action is not so much resolved as put aside in the final part of the book, where Janus is no longer invoked. Instead, it is here that many of the leading themes of what Latour would soon identify as his "actor-network" theory are presented. These include the rejection of a categorial distinction between the social and the natural, the idea that the universality of science is an illusory product of the extension of local networks, the denial that there is a

"Great Divide" between the pre-modern and the modern, and a fortiori, between the modern and the post-modern.

The ontologically crucial notion of an actant, a term which applies to anything that acts, regardless of whether it is a social actor in the usual sense, one of the objects studied by the natural sciences, or a cyborg that straddles those boundaries, is introduced in Part One of Science in Action. Instead of distinguishing between natural objects, the subject-matter of the natural sciences, and social entities, the subject-matter of the social sciences, he introduces the term "actant" to cover both social and natural entities, people and things. It is defined as "whoever and whatever is represented" (1987, 84); we are also told that its nature is not an essence, but whatever emerges from trials of strength (1987, 89.) But a reader of Science in Action is given only a hint of the role of actants in Part Two of The Pasteurization of France (Latour 1988), where they are said to be prior to the social and natural, which emerge out of the networks actants generate, or their importance in his subsequent work.

Networks only come to prominence in Part Three of Science in Action, "From Short to Longer Networks." This is the point at which Latour sets out his principal disagreement with most previous work in the sociology of scientific knowledge: he argues that the symmetry principles set out in Bloor's "Strong Programme" (1976, 1991)—and especially the principle that good science and bad science alike must be given a causal and naturalistic explanation—are vitiated by an unquestioning acceptance of a distinction between the natural and the social, and the sociological dogma that one must provide a social explanation of science. Latour argues that once one makes this move, one will come to see science as a collection of networks, the product of actants at work in trials of strength.

Just what the "actor-network" vision of science amounts to, and how we are to conceive of actants and networks, is only hinted at in the closing pages of Science in Action. However, in a series of articles written shortly after this, the "actor-network"

terminology and the rejection of a distinction between the natural and the social takes center-stage (Latour 1988a, 1992, 1993, 1995; Pickering 1992; cf. Callon 1986.)

Comenting on his recent exchange with Collins (Pickering 1992), Latour criticized the method of "alternation" that Collins had proposed: the sociologist of science, Collins claimed, must be able to alternate between the perspectives of the different parties in a scientific controversy, moving between a sympathetic understanding of the competing views. Collins also defended the utility and importance of retaining categorial distinctions between persons and things, and the social and natural sciences. In their reply to Collins Latour and Callon argue that Collins ignores the importance of their rejection of the social/natural distinction and resultant insistence on the "coproduction of society and nature. … "natures" and "societies" are secreted as by-products of this circulation of quasi-objects" (Pickering 1992, 349.)

> Since… we wish to attack scientists' hegemony on the definition of nature, we have never wished to accept the essential source of their power: that is the very distribution between what is natural and what is social and the fixed allocation of ontological status that goes with it. We have never been interested in giving a social definition of anything, but we want to explain society, of which the things, facts and artifacts, are major components…. Our general symmetry principle is thus not to alternate between natural realism and social realism but to obtain nature and society as twin results of another activity, one that is more interesting for us. We call it network building, or collective things, or quasi-objects, or trials of force; … and others call it skill, forms of life, material practice (Pickering 1992, 348.)

In a contemporaneous discussion of his differences with Collins, Latour sums up their disagreement in terms which, strikingly, are more applicable to Science in Action, for it was that book had advocated alternation between the naturalism of the older Janus and the social realism of the younger Janus:

> BL [Bruno Latour]: …You can't alternate between social realism and naturalism, and then throw in some semiotics or discourse analysis. You must do the three together; then immediately you realize that in science studies we have, all along, been studying phenomena that have the characteristics of being narrative, collective, and outside. They are quasi-objects; they are not of our own making. We build them collectively, and they are narrated. That is it: real, narrated, social. It is perfectly simple. Now if these characteristics are put together, you have the trace—the trajectory in a network.
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> THC [T. Hugh Crawford]: To tie this to primary arguments you have made, the first rule of method in Science in Action is that you follow the scientists as they are working. In a sense that is what you are discussing with the actor-network theory: you do not wait until the object or the meaning is completely constructed, but instead follow it through whatever network it happens to be tracing
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> BL: Yes, but the whole of Science in Action is a tame version of the argument, tamed for sociologists. Each proposition is slightly weaker because of that, so it is a simplification. It does say, let us follow the actors—but of course then you have to ask, what is following the actors? That is not too clear and begins to show the weakness of all my rules there. (Crawford 1993, 263-4.)

Matters become still more complex when one considers Latour's relationship to the field of sociology. Here one might expect that he would make common cause with those in science and technology studies and the sociology of scientific knowledge who have also stressed the importance of science in action for an understanding of science and technology. But although he does claim that his book summarizes the methods of those "few people, either trained as scientists or not, who open the black boxes so that outsiders may have a glimpse at it" (1987, 15), and draws heavily on their work, on balance Science in Action "pose[s] as [a] searing critique of existing sociology of scientific knowledge" (Schaffer 1991, 181). and the extent of its indebtedness to previous work will not be

apparent to an uninformed reader. Why is this the case? To answer this question, we must consider the broader trajectory of Latour's career.

Despite Science in Action's systematic and method-oriented approach, it is in many ways a transitional work, roughly midway between his earlier fieldwork in the laboratory, where he made a contribution to the social study of science, and the articulation of his later "actor-network" theory. When it was published in 1987, he had been an associate professor of sociology at the School of Mines in Paris since 1982; that year he was made a director of research at the École des Hautes Études en Science Sociales; the following year he took up a long-term part-time in Science Studies at UC San Diego (Callebaut 1993, 107.) Professionally socialized as a sociologist of science, and at the point at which he received substantial professional acknowledgement, he instead began to position himself as a theorist whose work was not hemmed in by any disciplinary field, both by addressing a wider audience and by being exceptionally critical of the accepted orthodoxy within the sociology of scientific knowledge.

Latour's first book, Laboratory Life: The Social Construction of Scientific Facts (1979), co-authored with Steve Woolgar, a British sociologist of science, was based on fieldwork Latour did at Roger Guillemin's laboratory at the Salk Institute in San Diego during 1975-77; it was published by Sage in 1979. The book attracted a good deal of attention, and remains frequently cited as an early and influential example of sociology of science based on fieldwork in the laboratory. Latour's collaboration with Woolgar on Laboratory Life began after they met at a sociology of science meeting in 1975. Latour's initial contribution to the co-authored book was his fieldwork, while Woolgar's lay in his knowledge of the field of science studies. Laboratory Life presents itself as a contribution to the sociology of science, respectfully citing the work of leading figures in the field in its opening pages. Methodologically, its chief novelty consisted in its "anthropological" approach, a field study whose methods were modelled on those used by cultural anthropologists in "primitive" cultures. Instead of attempting to become an insider in the

culture of the laboratory, Latour retained a certain distance, regarding the beliefs of the scientists as just one more datum to be explained.

Laboratory Life, like Science in Action, takes Barnes (1974) and Bloor's (1976/1991) work on the "strong programme" in the sociology of science as its methodological point of departure. Unlike most previous work in the sociology of knowledge, which had treated scientific knowledge as categorically different from other knowledge, and for that reason, not amenable to a thoroughgoing sociological analysis, Barnes and Bloor claimed that the sociology of knowledge can investigate and explain the content and nature of scientific knowledge. A crucial move that underwrites this annexation of the theory of knowledge by sociology is their naturalistic conception of knowledge: they take the term to apply to whatever is collectively endorsed, and aim to give a causal account of it. The methodology of the strong programme, together with a focus on "controversy studies"—looking closely at all sides of a wide variety of scientific disputes—are characteristic of most work in the sociology of science in the ten-year period preceding the publication of Science in Action—and are also central commitments in the opening chapters of that book.

The clearest indicator of Latour's denial the distinctively social character of his work was the publication of a second edition of Laboratory Life by Princeton University Press in 1986, unchanged except for the addition of a short preface and postscript, a table of contents, index, additional references, and the dropping of the word "social" from the subtitle, which now read "The Construction of Scientific Facts." In their postscript, the authors justify this change by claiming that their use of the term "social" in the book was ironic, given their explicit disavowal of "social factors" in the first chapter. "Social" only had significance as part of a binary opposition with the non-social; "by demonstrating its pervasive applicability, the social study of science has rendered "social" devoid of any meaning" (1986, 281.) In other words, if everything is social, then nothing is social. But if "nothing is social" is just another way of saying that "everything is social", then it is not

so clear that Latour and Woolgar could succeed in denying their social orientation by no longer talking about it. For a less cryptic defence of their claim that their book was no longer about "the social construction of scientific facts" but "the construction of scientific facts" one has to turn elsewhere.

Schaffer's review essay on The Pasteurization of France (Latour 1988) shows how that book underwent a comparable, but less visible, distancing from the discipline of sociology when it was translated from Les Microbes (Latour [1984]). By closely comparing the text of the two books, Schaffer found that the authorial voice, and the audience the book addressed, had changed substantially in a crucial place (Schaffer 1991, 182-3.) The earlier introduction to the French version frames Latour's study of Pasteur's science and influence as a contribution to the social sciences, comparing his present exercise in the interpretation of Pasteur's role in French science to his earlier work in the microsociology of science. In the closing words of the English version of his introduction, published four years later, and so presumably written at the same time as Science in Action, or shortly after, Latour drops the talk of microsociology, instead comparing himself to a laboratory scientist, and introducing a new terminology of "networks of association" and "linkages" (Latour 1988, 12.) Schaffer aptly observes that the change in vocabulary, and the reader addressed are in keeping with Latour's precept that the good scientist shifts allies when opportune. By presenting himself as a scientist, and not a social scientist, Latour addresses a wider audience and allies himself with a more prestigious profession. But simply claiming to be a scientist, and to take on the rhetorical strategies he attributes to scientists, are not enough. Latour also had to make good on his claim to be doing something genuinely different from other sociologists of scientific knowledge.

Part Two, "Irreductions," is an aphoristic, anecdotal, and philosophically systematic presentation of his thought, a Machiavellian and Spinozistic Tractatus Scientifico-Philosophicus (Latour 1988, 4.7.8, p. 234.) It offers some of the most fertile ground in Latour's oeuvre for Haraway's reading: the first chapter, "From weakness to

potency" begins with the thesis that there are only trials of strength, only forces, or in Latour's preferred term, "actant," and "Whatever resists trials is real." (1988, 158-9.) But in keeping with the Spinozistic character of the piece, it is the monad, or actant, that is central, and the networks that the actants make up plays a relatively small role in the argument. More recently, Latour has suggested that this approach does not provide sufficient protection against reestablishing a metalanguage, of finding in it an "apology for unified power," akin to the danger of reading Nietzsche as offering a unified metaphysics on which everything is produced by the will to power. He has also maintained that his notion of a network is closely related to Deleuze's metaphor of the rhizome, a criss-crossing network of roots without a taproot or unifying structure (Crawford 1993, 264, 263.)

How are we to make sense of this welter of ways of approaching science? My suggestion here has been that if we approach them as the "recruitment of allies," rather than as setting out a theory, their narrative and argument will become much clearer. To demythologize the demythologizer is not to deny the significance of his ideas; that topic must be left for another occasion.

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