STS C200, Week 9: Actor-Network Theory

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1 Science in Action / Bruno Latour[3]

Latour presents a framework for studying the making of science. As the title suggests, there is an emphasis on examining science as it is being developed, rather than ready-made science. Unlike *Laboratory Life*, this examination is mostly post-hoc and is not shy to bring up scientific controvery – in stark contrast to last week's reading.

The basis of Latour's method is the identification and opening of the *black box*. A black box encloses a scientific fact that has reached closure; later users of the fact need not and will not be familiar with the 'inner workings' of the fact: its potentially controversial history, the arduous process of its discovery, or the academic networks that hold it in its place.

Chapter 1 The journey begins in the innermost part of the scientific network, focusing on actions taken by scientists towards developing their fact. Science is framed as a powerful and unique rhetorical tool, and this discussion therefore is mostly on the idealistic (i.e., non-materialistic) level.

The factuality of a scientific statement is determined by its use in subsequent statements. These subsequent statements are *modalities* that modify (or qualify) the original statement. A *positive modality* leads the reader away from the conditions in which the statement was conceived, thus closing the black box and making the statement appear more factual (e.g., "The structure of the protein is *A-B-C-D*"). A *negative modality* reminds the reader of the conditions in which the statement was conceived, opening the black box and pushing the statement away from closure and factuality (e.g., "In 1984, Prof. *X* claimed that the structure of the protein is *A-B-C-D*").

Facts are, therefore, established in a collective process of uses in modalities. Technical texts are the rhetorical hammer of science, whose weight is in its association with 'friends': references to previous works and citations by future works. "The power of rhetoric lies in making the dissenter feel lonely" [3, p. 44]. In what reads like a manuscript of military theory, Latour describes the positioning tactics of scientists in constructing a triumphant paper.

This viewpoint is obviously relativistic. However, it leaves not much room for an analysis of scientific facts based on their *contents*. The focus is on how other statements open or close the black box, but this regime does not even touch the black box itself.

Chapter 2 Latour sends us back to the laboratory, this time not as anthropologists or historians [4], but as dissenters of a scientific fact. The dissenter puts the laboratory and the fact it produced through *trials of strength*. The defending scientist plays the role of a *spokesperson*, showing how the instruments (cf. inscription devices [4]) speak for the fact. The dissenter tries to weaken this connection.

Recalling that reality is that which is too costly to change [4], the dissenter ultimately mounts an extreme (and extremely costly) attack: the construction of a counter-laboratory. In the counter-laboratory, the dissenter attempts to reconstruct (or refute) the fact entirely. The black box is obliterated.

These increasingly challenging trials show us that as controversy ramps up, the number of instruments grows, as each instrument decomposes into several. Indeed, the dissenter first politely asks their host (the

principal investigator) to point towards the instrument that yielded certain readings. Then, they ask for evidence that the instrument functions correctly. Ultimately, the dissenter sets up their own laboratory, in which each instrument is (or can be) opened up and inspected.

The process of scientific discovery has scientists taking "new objects" (such as a suspected biological substance or mechanism) and, based on the answers these new objects inscribe, defining these objects as "things". In other words, scientists speak for "things", allies that they themselves create.

When the scientist and the thing pass all trials of strength, the thing becomes *reality*,[1] and joins the scientists as a most powerful ally.

Latour seems to claim that this yields a hybrid realist-relativist approach, in which nature (or reality) is used to justify already-settled controversies, but nature is also the result of yet-to-be-settled controversies. Therefore, the outcome (nature) can never be used to explain why and how a controversy has been settled.

As was the case in *Laboratory Life*, this seems to me like a textbook relativist approach (per Bloor's definition). Furthermore, I found his dismissal of a purely-relativist outlook not very well supported. The argument can be summarized by his criticism of scholars of science: "Either they went on being relativists even about the settled parts of science – which made them look ludicrous; or they continued being realists even about the warm uncertain parts – and they made fools of themselves" [3, p. 100]. Ignoring the *ad populum* (is looking ludicrous such a bad thing?), the thesis of this very book is that science is to be studied by viewing its "warm uncertain parts", so the "settled parts" should not concern us as much. What, then, is the problem with being entirely relativist?[2]

Chapter 3 The methods so far present three possible outcomes for dissent: give up, be convinced, or dispute the fact in the laboratory. This gives the impression that the scientist with the stronger paper shall win. This is, of course, rarely the case.

This chapter examines the broader scientific network. To transform an idea into reality, the scientist needs to enrol others in the construction of the fact, and control (or at least predict) their behavior so that the fact built is indeed the fact intended.

Latour then describes the strategies and tactics of how this task is accomplished. Interestingly, he claims that these negotiation strategies are applied not just to fellow scientists and interest-holders, but also to nature itself. The negotiations are performed by all elements of a *machine*, human and non-human. This machine is the black box that is formed when many elements are made to act as one.

This is a main point of the text: technical and social associations should be analyzed with the same toolset. No "type" of association is inherently superior to the other; what matters is only the strength of the association.

In a notable act of reflexivity, Latour acknowledges that social scientists are scientists themselves. Just as nature cannot be used to explain the emergence of a fact (which is Bloor's symmetry), Latour says that society cannot be used either, because society too is a result of the resolution of controversies.

This chapter crystalizes Latour's differences from the STS we have encountered so far. Latour's view that society cannot be the basis of explanation puts him in opposition to the Strong Programme of Bloor, and his lack of distinction between technical and social associations differentiates his approach from that of the Social Construction of Technology of Bijker and Pinch [1].

Chapter 4 In the spirit of blurring the boundary between nature and science, and given the previous chapter that focused on people who are auxiliary but necessary for the scientific effort, Latour concludes that even people who are not sitting at the lab bench also participate in the scientific process; yes, that includes shaping the content of a scientific fact. Similarly to the previous chapter, what matters is not whether an actor is internal or external to the laboratory; only the strength of the connection matters.

At this point I refer back to Bourdieu [2] and my question of whether scientific capital interchangeable with other forms of capital? The conflict then was over the definition of scientific capital: is it the power to shape the content of scientific facts (the 'narrow' definition), or is it the power to shape the scientific effort

[1] In this work, reality is defined as "that which resists trials of strength".

[2] I am merely trying to challenge Latour by emulating Bloor's thought. My personal views are not relevant here.

at large (the 'broad' definition)? With Latour, the conflict is resolved and the question is answered in the positive: a government agency that offers a grant directing science in a certain direction ('broad' scientific capital) does necessarily also shape the content of science ('narrow' scientific capital).

Chapter 5 The efforts so far culminated in a massive increase in the number of elements that determine the fate of a claim. From nature alone, insider and outsider human elements were added, as well as non-human objects. These elements are expensive, and therefore technoscience is made in an exclusive environment: a powerful and tight-knit system of relations which we described above as the *scientific network*.

This network is tiny and rather modern, and cannot account for constructing all knowledge. This chapter examines what lies outside the network; that is, people and things that do not "do science", such as so-called laypersons and primitive civilizations.

Briefly put, Latour challenges the common (asymmetric) view in which non-scientists hold "(irrational) beliefs" to be explained in sociological terms, yet scientists possess "(rational) knowledge" that is explained by nature and reality. Latour suggests moving the discussion to *sociologic*, not logic; rather than a graph of inferences and their validity, examine the graph of connections and their strength. Avoid evaluating the rationality or irrationality of a fact or belief, and instead look at the direction and magnitude of the observer's displacement.

Chapter 6 This chapter explains the transformation of a *new object* into a *thing*, then a *fact* (Chapter 2). This transformation occurs by creating a thing that is *mobile*, *stable* enough to be moved, and *combinable* with existing (stable) knowledge. Thus, the discovery of a fact is the process of extending the scientific network.

Latour therefore instructs the reader to prioritize studying the network that extends into a fact over the cognitive or methodological background to the fact. He complements this instruction with a negative criterion: "Every time you hear about a failure of science, look for what part of which network has been punctured" [3, p. 249].

Latour views the carrying out of a scientific fact to the outside world (e.g. a prediction about a disease, or a "wet run" of a space flight) as an extension of the network of the fact to the outside world. For a successful interface and extension, the outside world needs to be adapted to the format of the network. This adaptation is what we refer to as *metrology* (the study of measurement).

2 Institutional Ecology, 'Translations', and Boundary Objects / Susan Leigh Star and James Griesemer[6]

As we've seen in Latour's work, scientific work is carried out by a diverse groups of actors: researchers, amateurs, administrators, animals, instruments, etc. Star and Griesemer seek to understand the effects of this heterogeneity on science. They outline a framework that centers *translations* (in Latour's sense?) and boundary objects, and instantiate this framework in a study of the Museum of Vertebrate Zoology in UC Berkeley.

Due to space constraints, I will focus on my limited understanding of the relation of this work to Latour's *Science in Action* [3]. The starting point of Star and Griesemer's framework is [3, Chapter 3]: the translation of the concerns of scientists to those of non-scientists (which these authors refer to as *interssement*). The authors propose that Latour's network analysis gives primacy to the viewpoint of a particular actor in the network, typically a manager or principle investigator. Instead, the authors build on Hughes's *ecology of institutions* which takes the whole enterprise as a unit of analysis.

Another issue the authors take with Latour's approach is that interessement is a 'funneling' where the concerns of many are reframed into a narrow passage point; their approach, however, allows for multiple passage points.

I must say that Star and Griesemer's brief presentation of the issues with Latour and their suggested alternatives [6, p. 388-391] was not very clear to me.

Questions and topics for discussions

- Latour's [3, Chapters 2 and 3] imply the following rule: the settlement of a controversy is the *cause* of both nature and society, and therefore we cannot use these to explain how and why a controversy has been settled. It is unclear what remains of this scorched-earth attack on methodology. Nature nor society can be used in explaining the emergence of facts, yet Latour is writing a book on how to explain the emergence of facts. Do we conclude that Latour's method (of network analysis) transcends nature and society? Where, then, does it lie?

References

[1] Wiebe Bijker, Thomas Hughes, Trevor Pinch (eds.), The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology, Cambridge, MA: MIT Press, 1987. Essays by Bijker-Pinch and Hughes (1-82).

[2] Pierre Bourdieu, The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason [1975], in Social Science Information, 14:19-47.

[3] Bruno Latour, Science in Action, Cambridge, MA: Harvard University Press, 1987.

[4] Bruno Latour and Steve Woolgar, Laboratory Life: The Construction of Scientific Facts, Princeton: Princeton University Press, 1979.

[5] Steven Shapin, 'The House of Experiment in Seventeenth Century England', Isis, 1988: 79, 373-404.

[6] Susan Leigh Star and James Griesemer, Institutional ecology, 'Translations', and Boundary Objects, Social Studies of Science, 1989, 19:387-420.