

STS C200, Week 8: Laboratory Studies

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1 Laboratory Life / Bruno Latour and Steve Woolgar [3]

Latour and Woolgar present an account of how science is made, based on a two-year study centered in a working laboratory. The study uses anthropological, historical, and sociological tools. The authors aim to present an account of science at its normative phase, as opposed to centering their research around “tales of scandal and intrigue” (cf. [1]).¹

The anthropological method sees the authors assuming the position of an “outsider” that produces a description of the laboratory that is not based on their prior knowledge of the scientific process. The “outsider” (referred to in the book as “observer”)² arrives at the conclusion that processes in the laboratory revolve, in one way or another, around documents. The organization of the lab around literary inscription seems, to me, as a modern instantiation of Shapin’s observation about the first laboratories: “the transformation of mere belief into proper knowledge was considered to consist of the transit from the perceptions and cognitions of the individual to the culture of the collective.” ([4, Page 375], also see section 2). In the modern era, the individual uses apparatus in the laboratory to observe, and transfers these to the collective culture by writing.

Next, the authors narrow the discussion from the entire laboratory to the construction of a particular fact: the peptide TRF. Now their method is that of an historian: they aggregate relevant published material, conduct interviews with historic actors, and uncover archives of the events surrounding TRF research. Briefly, the conclusion of this effort is that once the scientific question reaches a state of closure, the object in question undergoes a “decisive metamorphosis”: the object transforms from being *similar to* a certain molecule, into an object that *is* that certain molecule. This shows how a societal change (the arrival at closure) affects the content of a scientific fact.

Returning to the broad anthropological perspective, the authors zero-in on social *microprocesses* that shape the construction of facts in the laboratory. These are, most notably, conversations between scientists. The authors demonstrate how even a single verbal discussion holds in itself a system of negotiations whose nature oscillates between social and technical interests.

This leads to a reiteration of an argument familiar to us from Bloor (and Wittgenstein, and others), that the “truth” of a fact cannot explain its emergence. But unlike Bloor, the authors quickly stress that their position is not a relativist one. If I am understanding them correctly, it seems that the authors avoid relativism by asserting that truth is timing-dependent. But I think that this is accounted for in the relativist programme: per Bloor, relativism allows truth to be circumstance-dependent, and timing, I think, is a circumstance. In sum, I am confused as to why, if at all, the authors’ position is not essentially relativist

¹[1, Page 21]: “Many studies within the [Empirical Programme of Relativism] have been most fruitfully located in the area of scientific controversy. Controversies offer a methodological advantage in the comparative ease with which they reveal the interpretative flexibility of scientific results.” Indeed, to capture the social component in scientific construction, Latour and Woolgar will have to make up for their focus on the normative phase by increasing the symmetry of their perspective (namely, adopting an “outsider” approach).

²I prefer “outsider” because it stresses that the observer is a stranger to the workings of a laboratory (as opposed to an observer that is an “insider”).

(though their avoidance of the actual label is understandable).³

Since we brought up Bloor and definitions, we might as well bring up *reflexivity*. The anthropological perspective adopted by the authors is, they claim, conducive to a reflexive description. However, they define 'reflexivity' to mean "the realisation that observers ... are engaged in methods ... similar to those of the practitioners...". It is unclear to me if this definition is equivalent to the 'reflexivity' that has been at the heart of many of our discussions, namely the definition of Bloor. (Adapted to this setting, Bloor would say that the observers' method is reflexive if it can be applied to itself.)

Laboratory Life has many more ideas that I did not get to touch on, such as an entire chapter(!) that views scientific construction as an economic process with *credibility* as capital (cf. [2]). Still, the above notes reflect my impressions of the text. To me, it is an example (exemplar?) of STS bricolage, a study that hops between different viewpoints and methodologies, and has an interesting (and, at times, confusing) dialogue with other pillars of the emergent STS field.

2 The House of Experiment in Seventeenth Century England / Steven Shapin

This work studies the physical location in which empirical knowledge was generated, and the connection of the physical with the social setting.

Since my notes will be about cherry-picked points from the paper, let me try and do some justice by outlining its structure. First, the author points to the connections between the physical positions of participants in the empirical process and the contents of the empirical process itself; of particular interest is the *problem of trust* that arises from differential sensory access to a phenomenon. Naturally, a detailed historical account of the actual 'laboratories'⁴ of that time is given. The author examines the access to laboratories; that is, who was permitted to enter the laboratory and participate in the empirical process (these are not identical groups), and how these physical and normative barriers affected the evaluation of an experiment. The text concludes with an analysis of the correspondence between stages of the empirical process and the transition between private and public spaces of a house (in which most laboratories were situated).

On to the cherries. Witnessing is a physical act that forms the foundation of an empirical claim to knowledge. I want to note the interesting fact that even seventeenth-century thinkers recognized the relation between the physical and the social. Put differently, a popular view (today) is that empirical knowledge comes from an interaction between an 'objective' individual and a natural phenomenon, and is therefore removed from social considerations; yet even the "original empiricists" recognized that such private witnessing is insufficient for their programme due to epistemological, moral, and physical reasons:

- First and foremost, the transformation of belief into knowledge in the empirical scheme starts with witnessing, but (crucially) ends with the transfer of the individual's perception to the collective culture. Thus, knowledge generated by an individual witnessing a phenomenon is not valid until it is somehow shared with (and validated by) the broader community.
- Second, mankind is inherently and naturally corrupt, and so the perception of any individual witness is imperfect and biased. I am assuming that the rationale was that a group of independent observers can somehow average-out this noise.
- Third, the public (or interested parties) are physically prevented from making certain observations, for example due to the phenomenon occurring in a remote and expensive-to-reach location. In this case, direct witnessing is substituted for testimony, which is an inherently social act.

³Historical note: I seem to be adopting a stance that Bloor's definition of relativism is the "right" one, and since I can apply Bloor's definition to the authors' position then they are essentially relativist. However, the Bloor texts we read in which relativism is defined were all published shortly before or after *Laboratory Life*. Therefore, I want to raise the possibility that the authors were using one definition and Bloor another. This same point will hold in the next paragraph, when I bring up an apparent difference between the authors' reflexivity and that of Bloor.

⁴The term 'laboratory' is somewhat anachronistic, but I shall nonetheless use it instead of "the place in which knowledge is generated via the empirical process".

In the early phase of the empiricist movement, most experiments took place in private residences of empiricist gentlemen and their affiliates. But what were the norms within this space? These were yet undefined, and were “carved out and rearranged” from the existing and elaborate norms of hospitality among gentlemen. (Bricolage!)

The formal mechanisms that governed access to scientific spaces were therefore not explicitly written (as they were known to members of the relevant class). Entry to one’s space was granted if the guest was known personally, by reputation, or by association.

In addition to the formal mechanisms, informal access mechanisms derived from the empirical ideology (rather than social precedent) were in place. Specifically, I am referring to the expectation that guests witness, not spectate: guests were expected to take an active role in the process of knowledge discovery by critiquing the experiment.

That said, physical access to the place of experiment did not guarantee access to the epistemological process itself. Apart from gentlemen, technicians were also present at experiments, in a limited sense of the word. They had the skill to operate machines, but lacked the qualifications to make knowledge. They were seen to lack “free verbal action”: a witness’s credibility was tied to their disinterestedness, yet the technician’s livelihood was directly tied to the outcome of the experiment.

Thus, the condition of participants themselves was a source for the reliability of experiments. Care was taken to select ‘free’ participants. This excluded all servants (such as the aforementioned technicians) as well as other classes. Merchants, for example were not deemed ‘free’, because their interests were corrupted by a need to turn a profit.

These class-based discriminations were expressed in the most generality in the explicit desire of the Royal Society to create a space with no inequalities of rank. In addition to the ideal of disinterestedness, the goal seems to have been to distinguish the new movement from existing intellectual communities. Thomas Sprat (1635-1713), in his *History of the Royal Society*: “Philosophers have bin always Masters, & Scholars; some imposing, & all the other submitting; and not as equal observers without dependence ... scarce any man’s mind, is so capable of thinking strongly, in the presence of one, whom he fears and reverences...”

Lastly, a few words on the empirical experimental process and public vs. private spaces. Shapin distinguishes between three stages of an experiment: *trying* an experiment by tinkering with it and “getting it to work”; *showing* a working experiment to others; and *discoursing* about an experiment shown. Trying was done in private spaces, while showing and discoursing naturally took place in a public space.

I must admit that the actual conclusion of this connection between privacy and the phases of an experiment was not entirely clear to me. My own conclusion is that perhaps it was this delimitation between private and public, afforded by the gentleman’s home (which has both private and public components), that shaped the structure of the empirical experimental process. In other words, if the early laboratory was sited in a purely public or private space, then perhaps the early experimental process would have had a different structure.

Questions and topics for discussions

- Following Footnote 3: Are Latour and Woolgar relativist? Do they practice reflexivity?

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 and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts*, Princeton: Princeton University Press, 1979.

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