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Demystification of early Latour

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There are two conclusions that one can safely draw from the debates on the relationship between sociology of science and philosophy during recent decades: The sociologists of science have been typically perceived as advocating social constructivism, and philosophers have generally attacked this position as indefensible or even bizarre. My intention in this paper is to muddy the waters and show that the issue is far from this simple. I concentrate on Bruno Latour, who is usually taken as one of the most extreme scholars in an already radical constructivist camp.[1] He has become famous for his wide-ranging constructivist theses, which include the claim that facts and reality are scientists' constructions.

This paper is an attempt to offer a common-sense reading of Latour with the conceptual machinery of analytic philosophy. More specifically, my paper intends to show that despite an appearance to the contrary Latour ought not to be taken as a metaphysical social constructivist in the sense that the philosophers of science use this label. If this perspective strikes one as being insensitive to the kind of scholarship Latour represents, my defence is that this is an attempt to translate Latour's discourse into a language that analytic philosophers also understand, in which Latour's statements are interpreted as authentically as possible. One hopes that this endeavour might help to enhance communication across various disciplines in science studies, from sociology to philosophy of science and vice versa.

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[1] A representative selection of various sociological approaches on science can be found in Pickering's (1992) edited book *Science as Practice and Culture*.

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My focus is primarily on Latour's earlier work. It offers a fruitful object of study, having generated many famous wide-ranging constructivist theses. The key text is Latour and Woolgar's groundbreaking *Laboratory Life. The Social Construction of Scientific Facts*, first published in 1979. Another central piece is Latour's first general presentation of his network model, *Science in Action*, which appeared in 1987. *Pasteurization of France* that was published in French a couple of years earlier and was translated into English in 1984 provides a good illustration of how Latour applies his network model to one well-know episode in the history of science, the scope of which also is historically much wider than that of *Laboratory Life*. Finally, I have used Latour's *Pandora's Hope* from 1999 as a further exemplification of the themes in his scholarship and as Latour's self-commentary on his earlier work. Latour has naturally published other books and articles afterwards, but they appear in this essay only in passing for two reasons. My intention is not to conduct a comprehensive exposition on Latour's whole career, which would not only be a challenging task to accomplish, but beyond the scope of an article length publication. Second, the early works form a sufficiently self-contained and an interesting object of research as such. I should not attempt to evaluate whether the analysis offered is applicable to all Latour's later works, but it certainly is a possibility that cannot be discounted.

Latour draws strict limits around the conceptual resources that may be used in studying science; therefore, it is interesting to ask whether within these limits we are able to give a satisfactory account or 'theory' of scientific activity. I will highlight both a more deflationary, and a more positive side of his philosophy, because my intention is to divert philosophers attention to the questions where one expects to find proper disagreements between Latour and most philosophers of science. This is to say, philosophers' engagement with (at least the Latour kind of) sociologists should focus on the question of how well the latter manage to explain the advancements and failures of science. The question is important, as it may help us to understand the limits (as well as benefits) of the kinds of explanation offered by sociologists. This examination also is well-justified in the current intellectual climate, because while Latour's science studies is often regarded as absurd by philosophers of science, it has fallen on fertile ground among many contemporary historians of science (cf. Golinski, 1988).

In this paper, I will first briefly consider different kinds of social constructivism and consider their relevance to our examination. Latour's various constructivist claims seem and are usually taken to fall in the category of metaphysical constructivism. However, I will show that he is not a metaphysical constructivist and that his constructivist statements are somewhat trivial claims. His theses become comprehensible also from a philosopher's point of view once one remembers that Latour studies science as an anthropologist. This demystification of Latour via his anthropological perspective on science is then continued by taking a look at other more specific constructivist statements that can be found in his writings, such as the claim that Pasteur constructed microbes. After that I examine Latour's 'positive' explanatory model trying to find the most fundamental explanatory principles in his theory. It turns out that the restrictions put forward by Latour on the kinds of notion permissible in the explanation of science stem from what philosophers would call epistemological anti-realism. For example, he denies not that there is reality but that one can have independent access to it. This examination ends with a sceptical remark on whether Latour is able to explain satisfactorily success and failure in science.

1 Social constructivism

Sociologists of science have been accused of various kinds of social constructivism by philosophers of science. It is therefore important to pay attention to how we understand 'social constructivism' and narrow the scope of focus accordingly. Paul Boghossian (2001) has distinguished two different theses of social constructivism that often cause controversy. The first is the metaphysical claim according to which something is real but of our own creation. The second claim is an epistemological one which says that the reason for having a particular belief boils down to the role this belief plays in our social lives rather than to the evidence in its favour. André Kukla has approached social constructivism slightly differently in his *Social Constructivism and the Philosophy of Science* (2000). He distinguishes three types of social constructivism: metaphysical, epistemological and semantic. The metaphysical thesis says that the objects of science are invented or made, rather than discovered. The epistemological thesis commits one to a view that there is no absolute warrant for any belief; any rational warrant is relative to a culture, individual or paradigm. The semantic thesis in turn means that sentences do not have determined empirical content because they are not appropriately connected with the world, and so any verbal outcome is subject to negotiation. Kukla emphasises that all these types of constructivism are independent of each other[2]. To take one more example, Hacking defines social constructivism more broadly, by three "sticking points": contingency, nominalism and explanations of stability. Seen from the social constructivist point of view, the position boils roughly down to three statements: Objects in science need not have existed at all, the world we describe does not have any pre-given structure and the explanations for the stability of scientific beliefs involve external elements to the content of science Hacking (2001, Chapter 3).

[2]Kukla (2000, pp. 5–6); cf. Chapters 1 and 2 for further references on social constructivism.

The most radical form of social constructivism is arguably the one which says that the objects themselves are created or invented in science, and thus not discovered by scientists, i.e., metaphysical social constructivism. Yet, the radicality of this thesis depends on its domain of application. It is not striking to claim that beliefs or science are constructed. Both science and scientific beliefs are human constructions in a trivial sense. It is not either revolutionary to say that *some* objects of scientific research are constructed, because there clearly are objects which are not found independently in nature. The periodic table contains over 20 elements that are not found in nature and must thus be synthesized by humans.[3] Some of Latour's claims about construction seem, even at the first sight, to fall under this trivial construction category. Bioassay or any phenomena whose production depends on it are clearly constructed by scientists in their laboratories (cf. Latour and Woolgar, 1979, p. 64).

One also needs to be sensitive to the fact that Latour and Woolgar question the applicability of the term 'social construction'. In the postscript of the second edition of *Laboratory Life*, they explain their reason to drop the term 'social' from the title of the book, the consequence of which is that *Laboratory Life. The Social Construction of Scientific Facts* became *Laboratory Life. The Construction of Scientific Facts*. Latour and Woolgar write that the use of the term was ironic in the first place and that it denotes too broad a category, making the term practically meaningless. The point is that "*all* interactions are social" (Latour and Woolgar, 1979, p. 281; original emphasis) and so it is better to just talk about 'construction of scientific facts'. In light of some later writings in which Latour tries to overcome dichotomous explanations that rely on a one-dimensional axis whose poles are social and nature, it is surprising to deem all interactions social (e.g., Callon and Latour, 1992; Latour, 2005, p. 254). By doing this Latour seems to legitimate the practice of labelling him as 'social constructivist'. However, this would be an incorrect judgment, because on various occasions Latour stresses that non-human entities have a major role to play in the process of construction and that they can be taken as 'social actors' as well (e.g., Latour, 2005, pp. 92 & 106–107). Indeed, in *Science in Action*, Latour introduced the notion 'actant' that does not imply any *a priori* distinction between human and non-human factors.

Further, in a relatively recent publication, *Reassembling the Social*, Latour explicitly recognises the ambivalent message that the talk of 'social construction' and 'construction' more generally conveys, i.e., that most commentators of his works took it that "*either* something was real and not con-

[3] An 'objectivist' would probably point out that these elements are natural kinds, which exist independently of humans. This is however not important here, because the point is that the talk of construction in science makes sense if it is appropriately qualified.

structed, or it was constructed and artificial, contrived and invented, made up and false" (Latour, 2005, p. 90; original emphasis). Latour's point is not to cast doubt on the reality of objects, but to offer an account of the contingent process that underlies the emergence of entities in the ontology of science: "When we [actor network scholars] say that a fact is constructed, we simply mean that we account for the solid objective reality, by mobilizing various entities whose assemblage could fail" (Latour, 2005, p. 91; original emphasis).

It is therefore important to realise that being constructed is not the same as being unreal or less real, as one may be tempted to think. A telephone is not imaginary or fiction although constructed. The reality of artificially derived elements would become very clear to anybody who come to contact with them, as they tend to be radioactive. It seems that the actual issue behind metaphysical constructivism is whether an object is created by humans (perhaps in cooperation with non-human entities) or whether it is part of the human-independent ready-structured world. Common sense seems to dictate that a telephone needs human construction in order to come into existence, but a stone does not. But both are real. For this reason, the question that we have to pose to Latour is whether he maintains that the objects of science are made by humans or whether they are out there independent of human beings irrespective of the question of their reality (cf. Hacking, 2001, Chapter 5).

Latour claims that there was no microbe to be discovered by Pasteur and early microbiologists: it was just a temporary construction which eventually evaporated (Latour, 1984, p. 108). He also makes clear in *Laboratory Life* that, with the help of some material instrumentation, the hormone TRF was made by scientists in the laboratory (e.g., pp. 64, 125–126, & 176–177). Even better, Latour claims that facts in general are constructed and that the representation of nature is the consequence of the settlement of a controversy, not the other way round (e.g., his third rule of method, see Latour, 1987, p. 99).

Now this is something else. The view above has been dismissed equally by natural scientists and philosophers of science. Practically all the commentators in these fields are puzzled how anyone can defend such a position. Hacking writes that Latour is taken almost universally as a "constructionist", and probably because of this position, many regard him as the public enemy number one (Hacking, 2001, pp. 64–65; cf. Kukla, 2000, p. 9; Newton, 2000, p. 200). Boghossian writes suggestively that it is not easy to make sense of the idea that facts about elementary particles or dinosaurs are a consequence of scientific theorizing (Boghossian, 2001). Indeed, it is not, but I intend to show next that it is nevertheless possible.

2 Anthropological standpoint

In order to comprehend Latour's constructivism we have first to understand his approach to science. Fortunately, he is quite explicit about it. In *Laboratory Life*, Latour and Woolgar define what is new and specific in their study of science. Their concern is with daily activities of working scientists, or as they call it, 'the soft underbelly of science' (Latour and Woolgar, 1979, pp. 27 & 20). The investigators focus on "observations of actual laboratory practice" (Latour and Woolgar, 1979, p. 153) or draw attention to "the process by which scientists make sense of their observations" (Latour and Woolgar, 1979, p. 32). Further, the authors compare their orientation to that of an anthropologist and describe their point of view as ethnographic. More specifically, they utilise the idea of "anthropological strangeness", according to which the lack of prior knowledge does not prevent one from gaining understanding of science (Latour and Woolgar, 1979, p. 29; Latour, 1984, pp. 125–126).

Latour's approach reminds us of Quine's radical translator who studies a native tribe without any prior knowledge of its habits or language, except that the authors of *Laboratory Life*, of course, have some prior understanding of the functioning of science. They have to therefore pretend not to understand and concentrate only on what they see with their own eyes in the laboratory. All in all, the task is to understand science and scientist's activities by direct observation and without any presumptions as to what constitutes and explains scientific activity.

This approach to science imposes serious methodological limitations for permissible action by the investigators. By paying attention to 'routinely occurring minutiae' (Latour and Woolgar, 1979, p. 27) one won't come across 'truth' or 'reality' in action in science. Since our anthropologist enters the laboratory without preconditions of what constitutes knowledge (Latour, 1987, p. 13) and without the pre-empirical presumption that scientific knowledge is qualitatively different from common sense, the authors refrain from using any epistemological concepts in their explanations (Latour and Woolgar, 1979, p. 153). One can thus see that the aim of the anthropological approach is to follow the action of scientists as close to the surface level as possible, at the immediate observable social level, without using any context-transcending notions in explanations and without making any assumptions prior to investigation. Further, any context-transcending explanation or employment of context-transcending concepts in one's explanations need to be justified empirically. Latour's method might well be called extreme empiricist and descriptivist.[4]

[4] Cf. Collins and Yearley, 1992.

3 Social construction deconstructed

The role and status of facts make a rewarding topic of analysis in Latour, because Latour is explicit and offers us a clear and direct definition of 'fact'. "A fact is nothing but a statement with no modality—M—and no trace of authorship" (Latour and Woolgar, 1979, p. 82). Latour describes five different stages of fact-making. At the lowest level, 'fact' has actually the status of an 'artefact'. Facts on this level are typically speculative statements or comprise conjectures by individual scientists, and appear at the end of papers or in private discussions. Type 2 statements contain modalities that draw attention to the generality of available evidence in favour of or against the statements. Type 3 statements include specific references to the evidence for the statements. Modality has been removed from statements of type 4, but they contain references. Finally, statements of type 5 are 'facts', because they are devoid of modalities and qualifications; they are straightforward statements of the state of affairs. What scientists try to do, according to Latour, is to get their colleagues to drop all the modalities from statements that they originated, which is to say that they try to make a statement more of a fact, and consequently, less of an artefact (Latour and Woolgar, 1979, pp. 91–82; cf. Latour, 1987, p. 42–44).

It is worth pausing at this point and spelling out clearly what Latour is getting at. He is talking about *statements* (and sometimes about sentences). It is hardly radical to insist that any statement was uttered somewhere at some specific point of time. So when Latour says that the fate of facts is in the hands of later users (e.g., Latour, 1987, p. 38) or that the construction of facts is a collective process (e.g., Latour, 1987, p. 29), this ought not to be surprising. Removing modalities from a statement that was a highly speculative conjecture and its incorporation into an encyclopaedia as fact knowledge certainly is dependent on numerous other scientists and experts in the field. Further, when he says that statements rarely achieve the status of 'fact', he must be absolutely right (Latour, 1987, p. 42). Most statements made won't ever be taken for granted or accepted as such. But if that happens, then 'fact' has been constructed in and by a community.

Latour's statements about 'reality' can be interpreted in a similar manner. He writes,

> Laboratories are now powerful enough to define reality... [R]eality as the Latin word *res* indicates, is what resists. What does resist? Trials of strength. If, in a given situation, no dissenter is able to modify the shape of a new object, then that's it, it is reality, at least for as long as the trials of strength are not modified... The minute the contest stops, the minute I write the word 'true', a new, formidable ally suddenly appears in the winner's camp, an ally invisible until then, but behaving now as if it had been there all along: Nature. (Latour, 1987, pp. 93–94)

How could laboratories define 'reality' or 'nature'? The answer: in the case where we are talking about 'reality' or 'nature' on the level of a scientist's action. Latour really studies *science in action* and one of the results of this process is 'reality' and 'nature'. Philosophers would probably be inclined to call them conceptions of what is real or nature, which is fine as long as one remembers that such notions are not allowed in Latour's theoretical model. Morphine, endorphin, the physiograph (which enables a graphical presentation of gut pulsation) and so on become 'real' or (part of) 'nature' when they are 'blackboxed', i.e., they become so well defined and tested that an individual scientist is no longer able to question them. 'Nature' or 'reality' could be understood as a collection of all taken-for-granted statements, standardised tests, conceptions, and objects (which stand up to any imaginable trials) in a community.

Against this background, it is worth asking why Latour's claims have caused so much controversy. That is probably because Latour's understandings of the notions of 'fact' and 'nature' are different from the ones that have been generally accepted by most contemporary philosophers and natural scientists, i.e., the realist understanding of facts and nature as human-independent that make our propositions true or false. If a commentator is not careful in distinguishing different senses, s/he is easily led to a wrong track. In other words, Latour does not use 'fact', 'reality' and 'nature' as 'elevating words', as Ian Hacking has called their usage in philosophy (Hacking, 2001, pp. 22–23). 'Fact' for Latour is not a human-independent thing out there in the world, but quite a mundane entity, as we have seen. If we try to read Latour's texts with a realist metaphysical understanding of 'facts' in our minds, the situation becomes very confused. If facts are independent of us, it does not make good sense to talk of constructing facts or of different stages of fact-making. If a scientist complains that the story of the emergence of the hormone TRF shows how scientists discovered a fact, Latour is inclined to respond that this is not possible because 'TRF' and other 'facts' are by definition constructed. He does not want to question the solidity of facts (as collectively endorsed), but to show how, where and when they were created. (Cf. Latour and Woolgar, 1979, pp. 127 & 175)

We can now see that scientists and philosophers of science, on the one hand, and Latour, on the other hand, are at cross-purposes. From a certain metaphysical point of view, it is almost outrageous to claim that facts are not qualitatively different from fiction (Latour, 1987, p. 42), but this becomes understandable if one remembers that we are talking about collective stabilisations of statements. Any statement could, in principle, be questioned, but some achieve a foundational status, at least for a certain limited time. It is as if Latour's idea is to label different statements according to their status of general acceptability and stability. The difference between the statements of fact and fiction is a difference in degree.

It is thus clear that the realist understanding of 'fact' is not a subject for the construction talk. Interestingly, this is something that Latour too comments on. He says,

> Facts refuse to become sociologised. They seem able to return to their state of being 'out there' and thus to pass beyond the grasp of sociological analysis. In a similar way, our demonstration of the microprocessing of facts is likely to be a source of only temporary persuasion that facts are constructed. Readers, especially practising scientists, are unlikely to adopt this perspective for very long before returning to the notion that facts exist, and that it is their existence that required skilful revelation. (Latour and Woolgar, 1979, p. 175)

I think this observation hits the nail on the head and it is encouraging that Latour is able to see the different perspectives. Practising scientists or analytic philosophers do not normally talk of the stabilisation of assumptions when they write about facts. In actuality, Latour's non-transcendental 'facts' are not facts at all for the majority of contemporary scientists and philosophers; for them, they are what a community believes or takes to be a fact.

4 Latour as descriptivist

What I have said above represents an attempt to give a common-sense reading of Latour's work. The basic premise of this task is to take the anthropological perspective that limits all the analysis of science on the non-theoretical and non-transcendental surface level seriously. I believe we can continue this approach further and demystify a number of other claims and views of Latour.

When the reader comes across the idea that neuroendocrinologists are a "tribe of readers and writers" that spend most of their time reading and writing neuroendocrinological literature with various inscription devices found in their laboratory, one is taken aback. But we can now see that it makes good sense in Latour's mind-set. If an anthropological fieldworker enters a laboratory without preconceptions of their activity and their knowledge, and observes their activities from beginning to end, the observer will see that it does involve a lot of writing (Latour and Woolgar, 1979, pp. 56 & 69). Further, a major achievement for a research group is to produce a text which is published in a scientific journal and which will be read and cited by other scientists. Latour calls scientific writing 'fact-writing', which may precede many other activities but which aims at persuading others of the facticity of one's statements. The whole scientific activity seems to be directed towards this aim. As he vividly says, in order to persuade others that one's statements should be taken seriously, "rats had been bled and beheaded, frogs had been flayed, chemicals consumed, time spent, careers had been made or broken, and inscription devices has been manufactured and accumulated within the laboratory" (Latour and Woolgar, 1979, p. 88). In brief, if facts are understood as generally accepted statements, then one can say that scientists are fact-writers, because they try to make their statements as widely accepted as possible, and the best way to achieve this is to publish a paper in a highly esteemed scientific journal.

Latour's view of how objects in science are born and defined is very interesting. As with so many of his claims, they strike one as being radical at first sight, but turn out pretty trivial under a closer analysis. How are new objects born in the laboratory? By recording the answers that a new object "inscribes on the window of instruments" (Latour and Woolgar, 1979, p. 90). Or, "the new object is a list of written answers to trials" (Latour and Woolgar, 1979, p. 87). At the time of the emergence of an object, one cannot do much more than repeat the list of constitutive actions of the putative object in the following way: 'with A it does this, with C it does that', etc (Latour and Woolgar, 1979, p. 88). Further, Latour likes to talk of 'somethings' which do not first have names, but which receive names like 'somatostatin', 'polonium', 'anaerobic microbes', 'transfinite numbers', 'double helix', or 'Eagle computers' after the initial characterisations of their actions. This leads to a practice which presumes their existence independently of the trials that produced them (Latour and Woolgar, 1979, pp. 89 & 92).

It is noticeable that Latour refers to an underlying entity ('it does') as the source of the readings and observations that make the 'object'. One wonders whether this is a sign of the implicit acceptance of minimal realism in Latour. Naturally, Latour would not be willing to postulate a context-transcending entity, but it sounds as if he is nevertheless doing just that. I will come back to this question at the end of the paper. Second, Latour is clearly, and not totally unreasonably, describing the process that philosophers tend to call baptising or initiation ceremony of reference, made famous by Kripke (1980). Latour is clearly describing a baptismal of an unobservable object, and the reference of a term is fixed by a description of the causal powers that the object is supposed to have. There is nothing in these ideas that most contemporary philosophers of science would not be able to accept. Some of those who commit themselves to the Kripke-Putnam causal theory of reference accept now that description is needed at the time of baptism, but not required for successful reference determination thereafter.[5] However, according to Latour, if we add an item to the list of actions, we redefine the object (Latour and Woolgar, 1979, p. 88). This is exemplified in his comment on how TRF, an object of research of endocrinologists, is defined. A research of TRF in 1976 created a new object, which was "not the TRF

[5]Cf. Devitt and Sterelny, 1999, pp. 83–114.

of 1963, 1966, 1969, or 1975". Why is this the case? Because "from a strictly ethnographic point of view ... the object was constructed out of the *difference* between peaks on two curves" (Latour and Woolgar, 1979, pp. 125–126; original emphasis). At that time there was a change in the curves produced by a bioassay which were used to detect the presence of TRF. In other words, any and all the attributes associated with a putative object defines the object, or is part of the set of predicates which pick out a reference. A change in the object-characterising description changes the object or reference itself.

Latour observes perceptively that this way of defining objects raises a philosophical problem. If the object is defined by all the attributes given to it and observations made of it by scientists, can we say that it existed at all before the trials which were used to individuate the object? (We may add that this problem is additional to the one which changes the object whenever its description changes.) Latour answers his own question in the negative. He says it is wrong to speak of 'discovering microbes'. Instead, we should say that Pasteur constructed or "shaped" them (Latour, 1984, p. 88). Furthermore, 'microbe' "existed only for a time, in the absence of anything better, before it in turn was distorted" (Latour, 1984, p. 107). Isn't this an absurd statement? One thing is that Latour informs the reader that this is a conclusion reached from a practical and not from a theoretical point of view (Latour, 1984, p. 80), which is an allusion to his anthropological approach. But even if we adopt here a theoretical perspective, his view makes sense. *Strictly speaking*, Latour is correct. To the best of my knowledge, there is no object which would uniquely satisfy the description given by Pasteur. It is just as Latour says, subsequent scientists 'broke the microbe' into its constituents (Latour, 1984, p. 108). This is to say that Pasteur's 'microbe' refers to a number of separate entities and that the properties attributed by Pasteur probably do not hold of all those entities.

Philosophically speaking, Latour commits himself not only to a descriptive theory of reference fixing but also to what might be called a wide descriptive theory of reference. According to the wide descriptive theory of reference, in order for a term to refer, all the assumptions postulated about the putative reference have to be satisfied by the reference. This is a doctrine which has sometimes been used to explain Kuhn's idea of meaning change and specifically his claim that references change in theory transitions.[6] As Latour himself indicated, the doctrine is philosophically problematic. Most contemporary philosophers would not be willing to go as far as to claim that no microbe existed before Pasteur's postulations or that reference changes whenever theory changes.[7] Even Latour himself drafts a rudimentary an-

[6] Cf. Bird (2000, pp. 164–179); Kuukkanen (2008, pp. 62–63 & 188–189).

[7] One option is to adopt a causal theory of reference, already mentioned. Further, some

swer to the question of how to preserve the continuity of reference in theory transitions. He says that a matter of revealing the 'true agent' from the false ones requires showing that a new translation includes all the manifestations of the (true) earlier agent. Otherwise, the argument and scepticism about it continue and others will try redubbing (Latour, 1984, p. 81).

What this shows is that Latour comes across as more trivial and comprehensible than may look at first sight. Above all, he is not a metaphysical constructivist, but his construction talk derives from his anthropological point of view. The wide descriptivist perspective to reference is a consequence of this standpoint, as writing down all the characterisations of a putative reference without further theoretical reflection or critique makes the object identical with its description. Although these positions may not be philosophically favoured by most contemporary analytic philosophers, they nevertheless enable (one hopes) a rational dialogue with Latour.

5 Success and failure (un)explained

We have seen that symptomatic of Latour's anthropological approach is minimalism. It sticks to the notions that are immediately empirically verifiable and avoids using transcendental concepts. However, it is not clear whether it is able to offer us also explanations of science, or more specifically, of explanations of failures and successes in science. Further, in this context, it is worth noting that Latour has also formulated something that might well be called a theory of scientific advance. I will next attempt to evaluate how explanatory his model of science is with regard to success and failure.

It may appear that Latour's more positive philosophy of science is detached from his anthropological approach. This is however not the case. As we have seen, Latour demands that any concept used in explanations has to be empirically justified, and this is also the case with the 'theory' of science that arises out of Latour's other works and especially from *Science in Action.* In this sense, the deflationary and positive sides of Latour can be conceived of forming a continuum. My intention is to divert philosophers' attention away from the debate on social constructivism, where, in my view, there is less controversy to be found, to the questions where more significant disagreements may be expected to arise.

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realistically minded philosophers, such as Philip Kitcher (1978), have suggested that we might be able to identify contexts where a reference can be taken to be properly referring from reference failures. Hartry Field (1973) has suggested that we could conceive of there being "partially denoted" entities, which may subsequently undergo "denotational refinement". For example, Newton's mass, according to Field, partially denoted relativistic mass and proper mass, and in the Einsteinian revolution went under refinement to denote only the latter.

It is true that not all Latour's books are as anthropological as *Laboratory Life*, which has served us as a kind of ideal model of the anthropological orientation. Nevertheless, the anthropological-empirical orientation characterises Latour's general approach to science also in his other works. For example, *Pandora's Hope*, published at the turn of 21st century, contains an anthropological study about soil research in the Amazon forest. Latour also confesses he believes in "a universalist anthropology", pointing out that also the "modernist settlement" makes an object of "a true anthropological curiosity" (Latour, 1999, p. 277; cf. p. 14), which also is the central topic in his book *We Have Never Been Modern* (1991). In a similar fashion as earlier works, Pandora's Hope is peppered with claims that the conclusions reached are empirically justified. The book talks about dropping modalities and fact-making (Latour, 1991, pp. 93–94), the impossibility of making any a priori divisions (e.g., pp. 86–87 & 126), empirical documentation of the historical conclusions (e.g., pp. 166–167), or how Latour simply follows "the veins and arteries" of science (p. 106). Most important, Latour's 'positive explanation' of science, his network model expounded in *Science in Action*, is alleged to be based on empirical findings.[8] His six general principles in *Science in Action* represent Latour's "personal summary of the empirical facts at hand after a decade of work in this area" (Latour, 1987, p. 17). Let us have a look at some of them.

For our purposes, the third and sixth principles are most relevant:

> *Third principle.* We are never confronted with science, technology and society, but with a gamut of weaker and stronger associations; thus understanding *what* facts and machines are is the same task as understanding *who* the people are. (Latour, 1987, p. 259)
>
> *Sixth principle.* The history of technoscience is in large part the history of all the little inventions made along the networks to accelerate the mobility of traces, or to enhance their faithfulness, combination and cohesion, so as to make action at a distance possible. (Latour, 1987, p. 259)

I do not intend to evaluate here how strongly these are supported empirically. In any case, although they represent a step towards abstraction and generalisation from what we find especially in *Laboratory Life*, Latour puts these forward as empirical hypotheses which ought to be debated and falsified if necessary.

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[8] In a relatively recent book, *Reassembling the Social* (2005), Latour continues to explicate his 'actor-network-theory'. He attempts to redefine sociology not as the 'science of the social', but as a study of the 'tracing of associations' (p. 5). The 'movement of re-association and reassembling' is of specific interest and a problem that calls for a social explanation in this new 'sociology of associations' (pp. 7–9).

Further, Latour's model is meant not to provide an alternative epistemology, but to discard totally all cognitive explanations of science. However, interestingly, he proposed "a ten-year moratorium on cognitive explanations of science" promising to "turn to the mind" if anything remains to be explained after that (Latour and Woolgar, 1979, p. 280). This moratorium is also expressed in Latour's Rule 7:

> Before attributing any special quality to the mind or to the method of people let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors. (Latour, 1987, p. 258)

Now it is more than twenty years since the moratorium was first time announced (in "Postscript to Second Edition" of *Laboratory Life*) and high time to update the situation. Should Latour keep his moratorium in place or is it time to employ cognitive explanations again?

As we just saw, central to Latour's explanation of science is networks. Latour does not make distinctions between science, technology and society. Outside and inside of science are linked to each other so that any sharp distinction between them is meaningless. According to Latour, there is a positive feedback loop between them: "the bigger, the harder, the purer science is inside, *the further outside other scientists have to go*" (Latour, 1987, p. 258, original emphasis). In other words, the existence of the 'inside' of science depends on the resources that scientists have managed to collect 'outside', i.e., what kind of funding, public image, political relations, etc. a particular institution or group has managed to gather. Latour takes all the people who provide the context of science as scientists as much as anyone who creates its content. Crucially, success in this 'outside' activity is dependent upon how many people are convinced that support and concentration on certain scientific work is "necessary for furthering *their own goals*" (Latour, 1987, pp. 156–157, original emphasis). A good example of how the content of science depends on the external enabling conditions is botany. Latour asks if botany may be constructed "everywhere in a universal and abstract space?" His answer is, "certainly not, because it needs thousands of carefully protected cases of dried, gathered, labelled plants; it also needs major institutions like Kew Gardens or the Jardin des Plantes where living specimens are germinated, cultivated and protected against cross-fertilisation. . . Botany is the *local knowledge* generated inside gathering institutions" (Latour, 1987, p. 229, original emphasis). The general lesson is that all sciences, no matter whether we are talking about the laws of physics, biology or mathematics, depend on the application of enabling material and non-material conditions (cf. Latour, 1987, p. 250).

'Technoscience' is thus entirely dependent on its ability to spread networks further. The explanatory model boils down to the struggle to extend one's own networks, persuade others to become its advocates and control their behaviour for this end. Latour introduces a term 'sociologics' (in contrast to logic), which focuses on the strength and number of associations in the network (Latour, 1987, p. 202). "The only things we want to know about these sociological pathways is where they lead to, how many people go along them with what sort of vehicles, and how easy they are to travel; not if they are wrong or right" (Latour, 1987, p. 205).

The question we have to ask is whether this model is enough to account for both successes and failures in science. Latour's seventh rule of methods instructs: "First to look at how the observers move in space and time, how the mobility, stability and combinality of inscriptions are enhanced, how the networks are extended, how all the informations are tied together in a cascade of re-representation" (Latour, 1987, pp. 246–247). The implication is that, if this is not enough, then we are allowed to look for cognitive explanations.

However, it should not be expected that the history of science provides us a certain correct model or shows indubitably that another model is incorrect. As John Preston has reasonably pointed out, it would be naïve to expect that the history of science provides us an unambiguous answer to the question whether the cumulativist or revolutionary model of scientific development is correct, for example. Up to a certain point, history is an interpretative and therefore philosophical discipline (Preston, 2008, p. 54). Therefore, the most sensible strategy is to focus on evaluating the explanatory power of rival models, and ask which one gives us the most satisfactory explanation.

On a certain level Latour's attitude is fully reasonable. He advises against using 'nature' (or 'God' for that matter) to explain why scientific disputes were settled (e.g., 94–95; 183). And this has to do with studying science in action. Scientific disputes are about what nature is or what is real, and it would be unreasonable to assume that one of the participants in these disputes possessed a God-like access to nature, truth or reality (cf. Bonjour, 1985, p. 7; Rescher, 1973, pp. 5–9). According to Latour, this would amount to Whig history which explains past developments by pointing out who was right and wrong. Latour remarks that one needs more fine-grained explanations, for example, as to why "people slowly turned N-rays into an artefact" (Latour, 1987, p. 100).

It is surely true that *at the time* physicists did not have the philosopher's stone which would have told what the true fact of the matter was. However, Latour also says that when the controversy is settled, nature is used as "the ultimate referee". Even if we were to agree with him that at the microsociological and -historical level the transcendental references cannot be used as explanatory notions, it is reasonable to ask whether avoiding using such notions leaves something unexplained when the results of the controversies are known. Let us therefore have a closer look at how Latour explains scientific advancement by studying his explanation of Pasteur's success.

As we have already seen, Latour adopts an agnostic stance. He makes no *a priori* distinctions between science and the rest of society, reason and force, main actors of the story, or in general, between 'allies' that make the pathways of science. However, he postulates that everything is involved in a relation of forces although he remains agnostic about the nature of these forces (Latour, 1984, pp. 7–9). According to Latour, what was striking in Pasteur's case is not his intellectual ingeniousness, but his ability to translate the intentions of the hygienists to support his own case so that both the hygienists and the Pasteurians were strengthened as a result. They together managed to make microbiology and the sanitization plans indisputable (Latour, 1984, pp. 34 & 54). According to Latour, this had nothing to with Pasteur's cognitive capacities or with the purported truth of theories (in the modernist pre-settlement sense, at least). Pasteur was thus extraordinary only in "translating the wishes of practically all the social groups of the period, then getting those wishes to emanate from a body of pure research that did not even know it was applicable to or comprehensible by the very groups from which it came" (Latour, 1984, p. 72). For example, he had to convince others that not only medical practice but also laboratory work was relevant at the time when infectious diseases were killing people around them.

In other words, the advancement of science is making new allies and extending one's network further and further. Pasteur became undisputed; hygienists gained positions in public administration and replaced engineers. The hygienists thus used Pasteur to secure positions, which suited Pasteur very well (Latour, 1984, pp. 56–58). This also implies that there is no qualitative difference between 'right' and 'might' or 'real' and 'unreal'. Differences between these kinds of notions as well those between 'illogical' and 'logical', 'contradictory' and 'consistent', etc. are a matter of different strengths of forces in the network (cf. Latour, 1984, pp. 153–154, 155–157 & 183). Let us ask now yet more explicitly: How does Latour explain success and failure in science? Because of his sixth principle above, we already know that the history of technoscience is recording the nature and strength of the networks that makes science possible. And so, "every time a fact is verified and a machine runs, it means that the lab or shop conditions have been extended *in some way*" (Latour, 1987, p. 250; original emphasis). But what is the difference between the cases where science's predictions are fulfilled and those when they fail? Latour answers, "The rule of method to apply here is rather straightforward: every time you hear about a successful application of a science look for the progressive extension of a network. Every time you hear about a failure of science, look for what part of which network has been punctured. I bet you will always find it" (Latour, 1987, p. 249).

The explanation above offers us the following two principles: When there is success, networks extend. And when there is failure, networks shrink. But this strikes one as being no explanation at all, but more like an empirically based re-statement of the facts of the matter. It is not surprising that successful science has managed to develop more extensive support networks, including institutions, the support of funding bodies, practical applications and publicity. Isn't it part of the definition of why it is successful? One wonders why did the particular application of science become such in the first place. Is it just because the participants in its networks were more cunning negotiators? Perhaps the networks extend because the forces behind it have been stronger and better at negotiation and diverting others for their cause. As we just saw, Latour attributed such skills to Pasteur. And when the networks fail, they do so because the forces are weaker and less skilful. This admittedly is *an explanation*, but we have to ask whether it is good enough.

Would Latour's model explain satisfactorily, for example, the failure of Lysenko's biology in the Soviet Union, considering it had managed to build the most extensive networks with the most powerful 'outside' support structures? Lysenko's theory received the acceptance of the Bolsheviks and became the Soviets' official application of dialectical materialism after 1935, gaining the support of Stalin, which enabled exceptional theoretical and material resources for its application and implementation (Lecourt, 1977, p. 99). It became part of the new science of agronomy and the collectivisation of Soviet agriculture, which led to wide attempts to transform numerous plants into others and in some cases the utilisation of wide areas of land for this purpose. Further, these experiments received wide publicity as well, as in nearly every issue of *Agrobiologia* from 1950 to 1955 articles appeared reporting transformations of wheat into rye and vice versa, barley into oats, peas into vetch, vetch into lentils, cabbage into swedes, firs into pines, hazelnuts into hornbeams, alders into birches and sunflowers into strangle weed (Medvedev, 1969, p. 170).

The question we have to ask is why did Lysenko's biology fail despite all this? Why did the extension of his networks come to an end? The obvious reason that he lost his most powerful supporter, Stalin, cannot be right, because his success survived well into the 1960s and received Khrushchev's personal support still at the beginning of the 1960s (e.g., Medvedev, 1969, p. 205). To say that the Western genetics had yet more powerful networks and thereby managed to divert Lysenko's biology (or the advocates of it) for its benefit sounds ad hoc. At the time, contacts were not direct and multiple, but Lysenko's biology nevertheless came quickly to its end around the mid 1960s. One cannot either really say that Lysenko's opponents were better at negotiating, as he himself was obviously a master in that skill.

We recall that Latour makes no *a priori* assumptions, which applies also to the distinction between human and non-human actors in the networks. Would it not then be possible to say that the reason for the failure is that non-human 'actants,' as they are called in Latour's parlance, did not co-operate, or Lysenko didn't manage to persuade non-human agents to further his goals? Indeed, we might. But what would this type of explanation amount to? David Bloor remarks that both Mendelism and Lysenkoism were engaged with nature but in two different ways (Bloor, 1999, p. 88). The point is to spell out the difference in their engagements so that it would add something to the explanation of the asymmetrical outcomes of these traditions. The upshot is that, if Latour were to say that Lysenko did not deal with non-human actants (i.e., 'nature') in the correct way, it would raise cognitive questions of the correctness and justification of his theories and experimental predictions. One would be bound to ask whether there was something wrong with his engagement with non-human actants. Did he not ask the right questions or place the actants under the right kinds of tests with respect to the expected answers?

Lysenko's failure could, indeed, indicate that his ideas and predictions of these non-human actants were not correct, and this, in turn, would provide us a more complete explanation of the failure. Naturally, Latour can choose not to engage in any deeper examination of non-human actants and the relationship in which they stand to theories and experiments. But this would mean admitting that actants (human and non-human alike) do explain, or are at least part of the explanation of, the asymmetrical outcome between successful and unsuccessful networks without trying to uncover what lies beneath; without trying to specify the exact reasons for the outcome. Interestingly, Latour goes a long way towards conceding this conclusion: "Why can't we say that Pasteur was right and Pouchet was wrong? Well, we can say it, but only on the condition that we render very clearly and precisely the institutional mechanisms that are *still at work* to maintain the asymmetry between the two positions" (Latour, 1999, p. 168; original emphasis). Latour thus accepts that the question can be put in these terms although he maintains that the support networks of science would explain the asymmetrical position between these two traditions. But, surely, the actants other than 'institutional mechanism', including non-human, are also responsible for the failure. One wonders what they are and how their role can be used to explain the outcome. Would it not be conceivable to accept that one (if not the only) explanatory factor of Lysenko's failure is that there was something wrong with how Lysenko described nature? One possibility is to say that the extension and upholding the networks became gradually more and more laborious and difficult when faced with the apparent failures of his theories and applications. This would be to say that, ultimately, Lysenko's biology failed because it was too wrong despite a determined effort to the contrary. Similarly, one might insist that the failure of Pouchet to get non-human actants to work in his favour as well as Pasteur did is a symptom of being wrong about nature in some important way.

Whether the interpretation above is true or not, these *kinds of explanations* (that use cognitive notions) would form a more complete explanation than a mere referral to forces and agents that tried to extend Lysenko's networks. However, it is important to add a caveat here. I am not suggesting that Pasteur's success shows that he was necessarily right; or more generally, that the success of a scientific theory is an infallible sign of its truthfulness. Kuhn's *Structure of Scientific Revolutions* (1970), Laudan's *The Confutation of Convergent Realism* (1981) and the subsequent discussion on the progress of science have made the unfeasibility of this kind of reasoning painfully evident.[9] There have been numerous successful theories in the past, which are however judged to be false if measured on modern standards and conceptions. But what can be said is that the refusal to consider any cognitive explanations of science threatens to leave one's explanations half-baked. First, one needs to give some account of why some networks extend while others fail. Second, if one attributes an explanatory role symmetrically to all kinds of actants to explain the asymmetrical state of networks, then one has to be ready to consider that role more specifically, including of what can be said of the properties and causal powers of the entities postulated.

In the current situation, where the successes of certain scientific traditions are evident, a mere reference to the fact that some networks extend while other shrink, would very likely leave something unexplained. Although it is challenging to specify the link between success, failure and the truth in science, it is reasonable to suggest that the cognitive considerations in terms of rightness and wrongness of theories become more compelling when failures and successes of them and their advocates become more blatant. And although the predictive and explanatory success of certain theories is not necessarily a sign of them being true or approximately true, it can be.

6 The end of the moratorium

I wish now come to back to the question of social constructivism. My conclusion above was that Latour is not an ontological or metaphysical

[9] There is, of course, a plenty of discussion about the relationship between success/failure and the truth. Laudan (1981) is a central piece. For some other important initiatives on the topic, cf. Van Fraassen (1980), Lipton (2004) and Psillos (2005).

social constructivist despite the appearance of some of his statements. It is tempting to classify Latour's network model as social constructivist in the epistemological sense, because scientific knowledge in his model looks like a result of different kinds of forces furthering their interests and causes. However, this too would be a mistake, as we see soon. Further, one might be tempted to read Latour as saying that the transcendental layers of nature, world, reality etc. do not exist. However, this does not seem to be the correct interpretation in light of Latour's own words. Alternatively, it would show that Latour is internally inconsistent.

Latour does not deny that there is reality, nature or truth (now understood in the philosophers' transcendental sense). Latour says, "Philosophers fool themselves when they look for a correspondence between words and things as the ultimate standard of truth. There is truth and there is reality, but there is neither correspondence nor *adequatio*" (Latour, 1999, p. 64; cf. also p. 15). What he specifically denies is the modernist settlement, i.e., the view that one could have independent access to them or that one could separate their influence from many other factors which have a role to play in the construction of scientific knowledge. According to Latour, in science studies, it does not make sense to talk independently of epistemology, ontology, psychology, politics, or theology. The central point is that they all "*go hand in hand and are aiming at the same settlement*" (Latour, 1999, pp. 13–14; original emphasis). Latour actually purposefully mixes epistemology with ontology (e.g., Latour, 1999, pp. 93 & 141). And this attitude implies that natural and social factors cannot be separated and talked about independently, and 'forces' in action contain inseparably both human and non-human elements (e.g., Latour, 2005, pp. 254–255). In brief, they form a hybrid.[10] Latour writes, "science studies does not say that facts are socially constructed... There exists only *one* settlement, which connects the questions of ontology, epistemology, ethics, politics, and the technology. There is thus no longer much sense in pursuing in isolation questions like 'how can a mind know the world outside"' (Latour, 1999, p. 293; original emphasis).

Now the crucial question is whether we should accept this kind of 'nonmodernist' stance, which might be said to be a form of epistemological antirealism in the philosophy of science. The problem is not the non-existence of the natural world, but that it is impossible to have independent unmediated access to it. However, it seems that even Latour himself cannot stick to the

[10]It is worth pointing out that this is not true of another major tradition in the sociology of science, The Sociology of Scientific Knowledge, although one can find generalisations that suggest otherwise (cf. Sokal and Bricmont, 1998). Bloor quite explicitly commits to a view that both social and non-social factors have a role to play in the construction of scientific knowledge, and that their role is analysable (cf. Bloor, 1991, p. 166; Bloor, 1996, p. 84; Bloor, 1999, pp. 81, 88, 90, 93, & 102; similarly Barnes, 1974, p. 43; Barnes and Bloor, 1982, p. 33).

restrictions of his model that forbid using the modernist kinds of context-transcending notions in the explanations. Earlier in the paper, it seemed that Latour implied that context-transcending entities are the causal factors behind the observable features used to define objects ("with A it does this, with C it does that"). The same sentiment is also found in his allusion that reality is defined by the resistance of trials that objects meet, as it sounds reasonable to assume that it is the world or objects in the world 'out there' that are the sources of this resistance. And as argued above, the nature of non-cooperating and cooperating non-human actants may need to be taken into account in the explanations of science's successes and failures. This is what I think shows the limitations of Latour's explanatory model of science. Latour's network model may be useful in giving an account of how the inside content of science and its outside support network are intertwined, but his explanation of science is insufficient at best. As much as any proper explanation of Lysenko's failure in biology may require a reference to context-transcending notions, so may explanations of success stories in science. At the very least, one has to be open for this possibility. All in all, Latour's examination of the history of science shows some signs of implicit inclination towards such context-transcending explanations as limiting cases of other explanatory strategies.

In conclusion, we have seen that conceptually it is possible to accept Latour's story about construction of facts, reality and scientific objects, and still maintain that there are facts, reality and scientific objects (although not necessarily in all cases) in the philosophers' transcendental sense. In judging Latour, my suggestion is not to focus on Latour's seeming ontological radicality or his apparent contradiction with common-sense approaches in philosophy, but to ask about the concrete value of his explanatory model of science. More specifically, his denial that 'nature' or 'reality' cannot be understood independently of social, political and technological aspects has to be examined critically. On several occasions, Latour remarks that the retrospective characterisation of the microprocesses in science often uses epistemological notions. The proper reaction is to point out that this is a laudable orientation, if we want to find a properly explanatory account of science at some future point of time. And if so, Latour's moratorium must be ended belatedly and science studies should consider employing cognitive explanations once again.

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