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## **Nature and history : towards a hermeneutic philosophy of historiography of science**

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# Chapter 6: Bruno Latour and the Co-Fabrication of Nature and Society

## 6.1 Another Kind of Constructivism

The perspectives in the previous chapter had in common that they explicitly transferred explanatory power from nature to society; they were aimed at showing what nature could or should not explain in the history of science, and why society should fill the gap. We have seen that arguments for this failed. However, it is also possible to reject the very assumption of a zero-sum game between natural and social explanation. Bruno Latour has argued particularly strongly against the idea that the world is divided into nature and society, or that there is an interesting debate to be had about the extent to which explanatory power resides on either side of that distinction.

In his case, and that of similar approaches such as Andrew Pickering's, it is still strictly true that nature does not play a role in our explanations of scientific development – the position that we abbreviated to NN in the previous chapter. From some perspectives, then, constructivism in Latour's sense may be indistinguishable from Bloor's or Collins' sociology.<sup>406</sup> However, Latour's position is rather different. Rather than saying that what nature does not explain society does, he argues that nature does not explain because it, together with society, is the *result* of what happens in history of science.

This is still a kind of constructivism, and one that I should perhaps at the outset confess to be unsympathetic to. I believe, for instance, that there was an external world already before the history of science, and that it is possible, both in theory and in practice, to distinguish between those aspects of the world with which humans have had something to do, and those aspects that exist independently of what humans have done. However, while being able to retreat to these fortresses of common sense may give me (and my fellow intuitive realists) some comfort, it is not so obvious that Latour needs to conquer these fortresses. He might just be able to march around them, in which case we will need to confront him on more interesting and challenging grounds, and can only hope to succeed in extending the safety of our fortress to those outside fields – much like Latour's laboratory scientists do with *their* favorite places.

## 6.2 Kayaking over Bridging

### 6.2.1 Two Banks, One River

A metaphor that Latour has used multiple times for what has happened in science studies so far is that of a river with the 'social' on one bank and the 'natural' on the other.<sup>407</sup> Too much of the debate, in his opinion, has been spilt on choosing between the two banks, or trying to

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<sup>406</sup> E.g. Brown (1989, 92).

<sup>407</sup> Latour (2008a, 13; 2003, 39).

*bridge* the two banks. Latour describes his position as one not of bridge-building but kayaking with the flow of the river.

For what kind of mistakes and solutions is this a metaphor? Latour applies his point not just to explanations in history of science, but to *all* cases in which attempts are made to identify what is due to nature and what to society. For example, the distinction between “guns kill people” and “people kill people” contrasts a materialistic interpretation with a sociological one, but *both* assume that it is clear what is due to people and what is due to guns.<sup>408</sup> Now, Latour’s point is not just that *both* guns *and* people contribute something to the outcome, because that would suggest that we could still speak of them as different kinds of entities that subsequently come together. That is why ‘bridging’ the two banks is a metaphor for what he *rejects*: the bridge, after all, still departs from both banks, and therefore assumes the very division that it seeks to overcome. As Latour says elsewhere:

To distinguish a priori ‘material’ and ‘social’ ties before linking them together again makes about as much sense as to account for the dynamic of a battle by imagining a group of soldiers and officers stark naked with a huge heap of paraphernalia – tanks, rifles, paperwork, uniforms – and then claim that ‘of course there exist[s] some (dialectical) relation between the two’. One should retort adamantly ‘No!’ There exists no relation whatsoever between ‘the material’ and ‘the social world’, because it is this very division which is a complete artifact.<sup>409</sup>

The satire works, but we should subject it to some closer scrutiny: why should we say ‘No!’ to this? Why couldn’t we think of a battle in terms of people and their materials? That is because with that division comes a whole range of associations and expectations about what people and objects can and cannot do: that agency,<sup>410</sup> intentions,<sup>411</sup> and speech<sup>412</sup> are exclusively human properties;<sup>413</sup> that the role of material things is restricted to that of a blind force, being a building block that merely supports human ingenuity or exercises a ‘resistance’<sup>414</sup> (and finally, as Latour jokingly adds mainly against David Bloor: proving that you are not an idealist).<sup>415</sup> Some entities have come to be defined as animal or material, others as free; some as conscious, others as mechanical.<sup>416</sup>

These definitions and expectations are not inevitably chiseled into the eternal fabric of the universe; they are the result of a history, the marks of a specifically modern ‘Constitution’ which separates the power to represent things from the power to represent

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<sup>408</sup> Latour (1999, 176-7).

<sup>409</sup> Latour (2005, 75-76).

<sup>410</sup> Latour (1999, 182).

<sup>411</sup> Latour (1999, 192).

<sup>412</sup> Latour (2004, 65).

<sup>413</sup> Cf also Latour (2005, 107).

<sup>414</sup> Latour (2003, 32).

<sup>415</sup> Latour (2003, 32; 1999b, 116).

<sup>416</sup> Latour (1993a, 15).

subjects – science from politics.<sup>417</sup> (That this Constitution is a historical artefact can be seen from the fact that non-Western cultures have never found use for adopting nature as a category.<sup>418</sup>) Latour considers this Constitution to be unfair in multiple ways, and one of those ways is that it is unfair to objects. These objects have been wrongly portrayed as simply matters of fact, whereas in fact they are “much more interesting, variegated, uncertain, complicated, far reaching, heterogeneous, risky, historical, local, material and networky than the pathetic version offered for too long by philosophers. Rocks are not simply there to be kicked at, desks to be thumped at.”<sup>419</sup>

### 6.2.2 Behind the Two Cultures

Latour’s main objective is, then, to return objects to what he thinks is their proper place: not a separate category *outside* society (that can then be made to relate to society), but an integral part of collectives that also contain humans. “You discriminate between the human and the inhuman. I do not hold this bias but see only actors”.<sup>420</sup> This brings him in conflict with both sides of the ‘two cultures’ divide, which in a Latourian diagnosis is a direct reflection of the modern Constitution in its complete separation of talking about things from talking about humans: scientists want to be free from the suggestion that what they say about nonhuman objects has anything to do with human subjectivity or politics; humanists want to protect humanity from objectification. Science studies, according to Latour, undermines those demarcations. “We tell the scientists that *the more connected a science* is to the rest of the collective, *the better* it is, the more accurate, the more verifiable, the more solid [...] But against the other camp, we tell the humanists that *the more nonhumans share existence with humans, the more humane* a collective becomes – and this too runs against what they have been trained for years to believe.”<sup>421</sup>

Latour takes some time to explain to the humanists that they need not worry: when action is redistributed among all parts of the collective rather than reduced to a small number of those (i.e. humans), a new form of humanity is gained;<sup>422</sup> and when humanists “add interpretation of machines to interpretation of texts, their culture will not fall to pieces; instead, it will take on added density”.<sup>423</sup> Indeed, in my opinion this call for an interpretation of the role of material objects in science studies is a great step forward from the categorical neglect of them that we have seen advocated in theory in the previous

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<sup>417</sup> Latour (1993a, 29).

<sup>418</sup> Latour (2004, 43).

<sup>419</sup> Latour (2005b, 20-21).

<sup>420</sup> Latour (1988a, 303). ‘You’ refers to sociologists. The key idea in this witty article is that humans and non-humans can replace each other; if humans can ‘delegate’ work and competences to non-human actors, this means that what defines our social relations is not just something human (310). Cf. also Latour (1988b, 9).

<sup>421</sup> Latour (1999, 18). The first part of this quote also reiterates the refusal to separate the social from the technical side of science that can already be found in Latour and Woolgar (1979).

<sup>422</sup> Latour (1993a, 138).

<sup>423</sup> Latour (1996a, viii).

chapter – from the insistence, for example, that we ought not talk about scallops because they are not accessible to us (see section 5.3.2). However, are the fears of Latour’s humanist opponents not justified; do we not pay for this step forward by taking two steps back, if we have to talk about objects as though they share in human intentionality, and to talk about humans as though they are simply another part of a network of objects?

To be sure, the commonsensical observation that viruses don’t talk to us has not escaped Latour’s notice.<sup>424</sup> The point is “not to say that scallops have voting power and will exercise it, or that door closers are entitled to social benefits and burial rites, but that a common vocabulary and a common ontology should be created by crisscrossing the divide by borrowing terms from one end to depict the other.”<sup>425</sup> ‘Crisscrossing the divide’ looks a bit like ‘bridging’, but Latour’s desire not to separate propositions into two ontological realms is something that he states on multiple occasions,<sup>426</sup> and elsewhere he says that if we refuse to distinguish human from natural societies, this does not mean *naturalizing* human societies.<sup>427</sup> “Here we begin to see the advantage of *kayaking* over *bridging*: naturalization is what happens when you try to transport, to transfer the ‘senseless hurrying of matter’ from the nature bank to the social or human side. That is when you treat the human with the strange notion of primary qualities handed down to you by the *already* bifurcated nature.”<sup>428</sup> It should be clear that when Latour declares human societies to be nothing special, nothing worthy or requiring of ontological distinction, he does so not from a perspective of scientism or reductionism: human intentions are not ‘actually only’ brain states or chemical reactions, or anything else that is already described by the vocabulary of one or more of the natural sciences. Rather, we should find a language in which to talk about all these things.

### 6.2.3 Initial Problems

But could this satisfy the humanist historian? Or better: can it be done at all – can a common vocabulary be developed that describes the contributions of both humans and nonhumans to collectives without a severe loss of information or clarity? I tend to see Latour’s commonsensical admission that scallops don’t have voting power as a hint that the answer to that question is negative: voting is something that humans do and that scallops don’t. At least, that is the way we usually talk about it, and if we want to flatten out this distinction, we will either have to make a case that scallops *do* vote, or a case that humans do *not* vote, or re-describe what scallops and humans do in terms other than voting.<sup>429</sup>

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<sup>424</sup> Latour (2004, 67).

<sup>425</sup> Callon and Latour (1992, 359).

<sup>426</sup> Latour (1999, 147; 1993, 79-81).

<sup>427</sup> On the limits of naturalization see Latour (1993a, 5-6).

<sup>428</sup> Latour (2008a, 15).

<sup>429</sup> Cf. Lynch (1996, 249-250) on Latour’s use of language, and Collins(2012) on the ‘flattening’ of ontology with Latour and ANT (411) and the, according to Collins, resultant turn to animism: “[Latour and his colleagues] talked of non-humans as though they were human” (412). For Collins, Latour’s Actor Network Theory becomes a reactionary opponent of the relativism implicit in Collins’ own

Latour takes one or more of these strategies at different locations: when he says that “in the course of [an] experiment Pasteur and the ferment mutually exchange and enhance their properties, Pasteur helping the ferment show its mettle, the ferment ‘helping’ Pasteur win one of his many medals”,<sup>430</sup> the symmetry in word-choice is intended to show that the type of activity attributed to the human actor (Pasteur) can equally well (except for the scare quotes) be attributed to the non-human actor (the ferment). Elsewhere, he explains a general claim that “speech is no longer a specifically human property, or at least humans are no longer its sole masters”.<sup>431</sup> What he means is that who *speaks* in the laboratory is not simply the scientist herself – that would be the kind of object-free speech that Latour likes to accuse SSK of seeing everywhere – nor simply ‘the facts’; rather, something more complex is happening, namely “that lab coats have invented *speech prostheses that allow nonhumans to participate in the discussions of humans, when humans become perplexed about the participation of new entities in collective life.*”<sup>432</sup>

This is a re-description in which neither humans nor non-humans can speak ‘on their own’, and this seems to be more illustrative of the nature of the move that Latour wants to make than his attempts to show that what non-humans do can be described in the same terms as what humans do (which more often than not require more than a little bit of goodwill to swallow): a move that consists in re-describing *all* activity in such a way that it is distributed over *all* the involved actors. “Purposeful action and intentionality may not be properties of objects, but they are not properties of humans either. They are the properties of institutions, of apparatuses [...]”.<sup>433</sup> This is a kind of social holism that brings to mind Bloor’s insistence that there are social phenomena – like norms – that by definition cannot be localized in individuals;<sup>434</sup> but if it is a social holism, it is a social holism *with things*. Things, after all, are to be involved in proper social science, because there is no ‘social stuff’ with which they are to be contrasted;<sup>435</sup> they can (and have) *become* part of societies. “Humans, for millions of years, have extended their social relations to other actants with which, with whom, they have swapped many properties, and with which, with whom, they form collectives”.<sup>436</sup>

As an argument about the almost inextricably complex interplay of human and non-human causal contributions to developments and outcomes, Latour’s point is well taken: indeed, it is impossible to find a human action that does not in some way, however

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sociological approach, precisely by shifting the “power of the world” back to objects, by “[flattening] everything out so much that the old prosaic powers of things can return for those who want them.” (413).

<sup>430</sup> Latour (1999, 124).

<sup>431</sup> Latour (2004, 65).

<sup>432</sup> Latour (2004, 67).

<sup>433</sup> Latour (1999, 192).

<sup>434</sup> But cf. Latour (1996, 234-238).

<sup>435</sup> Latour (2005, 1-9).

<sup>436</sup> Latour (1999, 198).

trivially (because obviously), relate to non-human factors. Moreover, even if Latour may not have done full justice to SSK perspectives in suggesting that they deny this, I think that his point, that because of this near inextricability it is a bad idea to insist on an exclusive focus on human factors in science studies (e.g. because only those really matter (Bloor, Knorr-Cetina) or because that is the only terrain we are competent to say something about (Collins)), is to be applauded: history does not allow itself to be split into two domains that can be understood *in isolation*.<sup>437</sup> For the history of science, this means that there is not a history of *thinking about nature* that can be understood in isolation from the history of *nature*. If anything, that should be the takeaway of this entire thesis; and to a large extent, Latour proves a valuable ally to this cause, in his many examples of how in all circumstances, a specific outcome is the result of the influence of many different kinds of entities of which we cannot without consequence ‘think away’ the nonhuman ones.

*However*, this is not a sufficient reason to collapse all *conceptual* distinctions between human and nonhuman entities, or to claim of specific kinds of actions that they are distributed over different entities in different ways than we usually think. Different things happen in different parts of the world at different times, and the things that happen in scientists’ brains may best be described with words that we don’t need to describe what happens in the rocks they study.

This is a rather pragmatic rebuttal, a retreat into the fortress of common sense from which I can shout “we don’t have these words for nothing!”. Latour can answer this objection from several angles: by noting that it invokes an implicit distinction between epistemology and metaphysics (pleading, after all, for a distinction in thought that is not there in reality), as if these are separable domains; by noting that it fails to account for the *constructed* status and the *historicity* of our current ontology; and by noting that it fails to *suspend* our current ontology in order to look more openly to how actors *make* ontologies (and have made our own). These answers need to be dealt with.

## 6.3 The Construction of Real Things

### 6.3.1 Collapsing Nature and Its Representation

One of the most intriguing aspects of Latour’s thought is his repeated insistence on the co-production of nature and society.<sup>438</sup> Already in *Science in action*, he summarizes this in two of his famous rules of method:

3 Since the settlement of a controversy is the *cause* of Nature’s representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled.

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<sup>437</sup> Cf. Latour (2004, 33-35).

<sup>438</sup> E.g. Latour (1990, 147); Callon and Latour (1992, 349).

4 Since the settlement of a controversy is the *cause* of Society's stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enroll human and non-human resources.<sup>439</sup>

Both nature and society are the result of certain settlements, then, and not causes to which we can appeal. It will be noted that Latour slides from 'Nature's representation' to 'Nature' here,<sup>440</sup> in a way that seems to undermine the logic of his argument: if he does not hold his content terms stable, the conclusion certainly does not follow.

However, the confusion is not the result of a slip of the tongue or another kind of thoughtlessness: Latour believes that, indeed, it is not just *representations* of nature that are the result of a history, but *nature itself* – and not just in the sense that natural entities evolve over time.<sup>441</sup> Latour is well aware that his position is counterintuitive, and therefore he emphasizes that he indeed means to defend this counterintuitive claim, by explicitly rejecting the intuitive alternative that banks on a distinction between one natural world and different historically developed representations of that world:

There may be thousands of ways of imagining how kinships bring children into existence, but there is only, it is argued, *one* developmental physiology to explain how babies really grow in the womb. There may be thousands of way to design a bridge and to decorate its surface, but only one way for gravity to exert its forces. The first *multiplicity* is the domain of the social sciences; the second *unity* is the purview of natural scientists. [...] This is just the solution that ANT [Actor-Network Theory] wishes to render untenable. With such a divide between one reality and many interpretations, the continuity and commensurability of what we call the associations would immediately disappear, since the multiple will run its troubled historical course while the unified reality will remain intact, untouched, and remote from any human history.<sup>442</sup>

Elsewhere, Latour summarizes what is wrong with the position he tries to undermine in the slogan: "multiculturalism acquires its rights to multiplicity only because it is solidly propped up by *mononaturalism*."<sup>443</sup> It is this 'mononaturalism' that has to go, then; and the point is not just that what humans do to the world changes it, that human history influences the world – if that were the case, "the fact remains that there are two histories, or rather one history full of sound and fury that unfolds *within a framework* that itself has no history, or creates no history. Now, this good-sense conception is precisely what we are going to have to abandon."<sup>444</sup> No, there is actually a multiplicity of worlds, a 'pluriverse';<sup>445</sup> and that

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<sup>439</sup> Latour (1987, 258).

<sup>440</sup> Bloor (1999, 87).

<sup>441</sup> Latour (1999, 145-146).

<sup>442</sup> Latour (2005, 117).

<sup>443</sup> Latour (2004, 33).

<sup>444</sup> Latour (2004, 34).

<sup>445</sup> Latour (2005, 116).

pluriverse does not consist of worlds that are ‘just there’, but of worlds that are the result of the actions of actors. How does this work?

### 6.3.2 Science and Technology

The way in which Latour makes this position work, is by attacking the distinction between the independence of reality and the work that is done to create that reality. We may, again, proceed from an intuitive trade-off between reality and construction, from the idea that we can distinguish between what is due to how the world is and what is due to the way in which human societies look at that world; under this light, if something is *constructed* (-by-subjects) it becomes *ipso facto* less real (because grounded to a lesser extent in the way the world really is). This, however, *assumes* the clear distribution of different kinds of causal influence among different kinds of entities; and we previously saw that Latour was onto something at least in his insistence that those causal influences are not isolated enough to assume such clarity beforehand.

So why not, instead of assuming that the reality and constructedness that we ascribe to something are involved in some zero-sum game, assume that *real* things can be *constructed*? Is that not how we talk about buildings, for which it would hardly make sense to ask whether they are real or constructed? Then why couldn't we do the same for science, where currently there “seems to be no plausible way to say that *because* something has been constructed and *well-constructed* it is *thus* solid, durable, independent, autonomous, and necessary”?<sup>446</sup>

Indeed, Latour likes to draw and emphasize analogies between science and technology, because the construction and reality of technological products are, like those of buildings, much more obviously complementary than they *seem* to be in science: “no one would dare assert that the Diesel engine ‘was always already there, even before it was discovered.’”<sup>447</sup> The analogy is an interesting one, especially for historians: it suggests that history can produce solid and independent things whose solidity and independence does not derive from their history-transcendence, from the fact, that is, that they were ‘always already there’.

The big question, then, is: can we, in the case of scientific entities like in the case of Diesel engines, account for their solidity without assuming that they were already there before they were discovered? To sharpen the focus of this question once more: given the position that Latour has taken, we are *not* just talking about how historically developed scientific *theories* can become stable – we may agree that a theory like the second law of thermodynamics is probably there to stay, even if we also agree that it hasn't always existed, and we may subsequently ask where it gets its stability. In that case, the answer to the latter question could still refer to stable structures in a history-transcending outside world, which is the very thing that Latour so urgently wants to dispense with. No, the question extends to

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<sup>446</sup> Latour (2003, 36). Cf. Latour (2002, 16).

<sup>447</sup> Latour (1996a, 23).

the *entities* described by those theories as well: so, can we explain why microbes or exoplanets *themselves* are stable elements of the furniture of our universe *without* presupposing that they were ‘already there’ before they were discovered? (The simple recognition that they were not ‘always’ already there, because they evolved over time, is not the point, as we have seen.) Or perhaps even more radically: can we explain this about whole *time periods*? “The billions of years since the Big Bang date from the 1950s; the pre-Cambrian era dates from the mid-nineteenth century; as for the particles that make up the universe, they were all born in the twentieth century.”<sup>448</sup>

### 6.3.3 Adding Actors

For all these things, Latour advocates the notion of a ‘relative existence’: “an existence that is no longer framed by the choice between never and nowhere on the one hand, and always and everywhere on the other.”<sup>449</sup> He demands a ‘generalized historicity’. No-one, he argues, will ask the question “Where was Pasteur *before* 1822?”<sup>450</sup> – well then, *why do* we ask this about the microbes that he discovered? According to Latour, this question is just as meaningless.<sup>451</sup> This seems like a disingenuous analogy: surely, those questions are not the same? Surely we can all agree that Pasteur as a human individual came into being only in a specific year and did not exist before that, *in the same way* that we can agree that the individual microbes that we find under our microscope in 21<sup>st</sup>-century laboratories did not exist in ancient Egypt; but that in both cases, *other* individual humans and *other* individual microbes *did* exist before – e.g. in ancient Egypt?

But if Latour’s analogy fails, it is just one rhetorical strategy that turns out not to work. Latour is trying to get across an intuition in which the application of *any* concept in times or places other than where it has been developed becomes something problematic – not necessarily impossible, but problematic; and one way in which he tries to get this across is in trying to establish analogies between material objects and concepts. Just like we don’t expect humans or machine-guns to time travel and *do* something before they actually exist, we shouldn’t simply expect concepts to aid us at such a temporal distance.<sup>452</sup> The second aspect of the analogy, then, is the interchangeability – in Latour’s thinking – of microbes and the *concept* of microbes; these are not different entities, one being a natural kind in the universe and the other being a nineteenth-century interpretation of that stable thing in nature. No, it is microbes *themselves* that come into being in the nineteenth century.

For the point is that entities, and classes of entities, can *pass* from nonexistence to existence *through* fabrication. “It is possible to go from a nonexistent entity to a generic class by passing through stages in which the entity is made of floating sense data, taken as a

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<sup>448</sup> Latour (2004, 35).

<sup>449</sup> Latour (2000, 252).

<sup>450</sup> Latour (2000, 253).

<sup>451</sup> Latour (2000, 264).

<sup>452</sup> Latour (2000, 250).

name of action, and then, finally, turned into a plantlike and organized being with a place within a well-established taxonomy."<sup>453</sup> This whole process is the result not of *discovery*, then, but of *work* being done: "let us say that in his laboratory in Lille Pasteur is *designing* an actor."<sup>454</sup>

This, of course, Pasteur is not doing on his own – we can predict now that Latour will be consistent in his insistence on not giving all agency to one type of actor (in this case the human scientist). In fact, Latour emphasizes that even among the human actors, Pasteur is not the autonomous innovator from whom scientific change simply emanates;<sup>455</sup> what happens is that Pasteur and other agents translate their own and each other's interests in such ways as to forge alliances.<sup>456</sup> Networks are being built and in the specific network in which Pasteur moves, he *adds* a new agent:

Pasteur adds to all the forces that composed French society at the time a new force for which he is the only credible spokesman – the microbe. [...] If you reveal microbes as essential actors in all social relations, then you need to make room for them, and for the people who show them and can eliminate them. Indeed the more you want to get rid of the microbes, the more room you should grant Pasteurians. This is not false consciousness, this is not looking for biased world views, this is just what the Pasteurians *did* and the way they were *seen* by all the other actors of the time.<sup>457</sup>

Here we seem to approach an answer to our question: what happens when new entities are discovered is not that they are being created as if by divine decree, nor that they were already always there, but that they are *made in* being added to a network.

## 6.4 The Inexplicable Development of Networks

### 6.4.1 Which Napoleon?

The question now – and, in my view, the question on which the plausibility of Latour's entire program hinges – is whether this adding of entities can be shown to be non-arbitrary in a way that doesn't silently assume something about extra-historical nature, about how things were and are *outside* the network *before* and *independently of* how the network came to be modified. Is there any way in which we can understand that it is *microbes* that Pasteur adds to the 19<sup>th</sup>-century French network, rather than anything else?<sup>458</sup>

One way to see that this is not arbitrary is that there were alternatives that did not make it – like Pouchet's defense of spontaneous generation. Are the different fates of Pasteur's and Pouchet's propositions to be explained by something else than the existence

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<sup>453</sup> Latour (1999, 122).

<sup>454</sup> Latour (1999, 122).

<sup>455</sup> Latour (1993b, 25).

<sup>456</sup> Latour (1993b, 65).

<sup>457</sup> Latour (1983, 157).

<sup>458</sup> See Shapin (1988, 541-543) on the lack of explanatory ambition in Latour's program.

(rather than non-existence) of microbes? Yes, because those fates are the fates of the entire networks with which they are connected; and those networks hardly overlap.<sup>459</sup> Especially not since apparently identical elements between them are, on a second look, not identical: Pouchet and Pasteur both write to Napoleon III for support, but Pouchet wants this support in the form of an endorsement of spontaneous generation; Pasteur in the form of money. They write to different emperors, then, who have different relations to the demarcation between science and politics.<sup>460</sup> Pasteur's Napoleon III respects the modern Constitution; Pouchet's doesn't.

Latour's remark that the two gentlemen did not write to the same emperor has prompted a sarcastic response from Nick Tosh, who noted that if we wanted to understand what Pasteur and Pouchet were thinking and doing, it might make sense to remember that they probably had different beliefs about Napoleon III, but that otherwise, "the man whom we call Napoleon III was an everyday, macroscopic object who did little to excite the retrospective attentions of philosophers of science".<sup>461</sup> This is an important point: it highlights the fact that Latour's radical metaphysics, if we went along with it, would have consequences not just for the scientific entities in our historical accounts, but for *all* entities. There is, after all, no reason for Latour to say that the existence of microbes needs to be regarded as relative to a network, but that Napoleon III is an unproblematic object. And indeed, if we take him seriously, he seems to be consistent and *does* say the same about Napoleon III.

In that case, however, deciding for a non-relative existence of Napoleon III will already be a sin against Latour's metaphysics. We do not need to be anachronistic historians of science who believe that there were microbes before Pasteur; the simple belief that Napoleon III existed independently of either Pasteur's or Pouchet's network would suffice to fail at doing science studies right. If good practice in general history and good practice in history of science are indeed so similar, perhaps this analogy works rather the other way round: the fact that most of their colleagues in general history unproblematically use Napoleon III in their accounts should serve as a sign to historians of science that, at least until the philosophers have settled the issue for all entities, they can unproblematically use microbes in theirs.

But suppose that general historians do not yet do Latourian actor-network metaphysics, but would want to do so; how would they need to proceed? To what would they 'relativize' Napoleon III's existence? Isn't there an infinite number of networks from which they can choose? And aren't many of those networks *independent* of both Pasteur and Pouchet up until the point where they make contact – e.g. when the scientists write to their emperor? If so, isn't the way Napoleon III is going to behave when his network and those of Pasteur and Pouchet extend in such a way as to partially overlap, determined in large part

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<sup>459</sup> Latour (2000, 260-261).

<sup>460</sup> Latour (2000, 261n11).

<sup>461</sup> Tosh (2007, 205).

by his previous movements in all the *other* networks of which he is a part? And isn't, then, the capacity of the Pasteurian network to *fabricate* its own Napoleon III very limited in the face of all those other networks?

Perhaps this is underestimating the absolute primacy of the one particular network that we study – for instance, that of Pasteur. When we are genuinely 'in' Pasteur's network, after all, there is no meaningful speech possible about networks independent of him. The idea just suggested, of having a bird's eye perspective on a multitude of networks, may be regarded by Latour as begging the question, if the right way to proceed is simply to immerse yourself in one network and follow the actants that make up that network. We may encounter a Napoleon III in the Pasteurian network, and he may do some work in there; and if we start all over again and immerse ourselves in Pouchet's network, we may again encounter an actant called Napoleon III; but there is no way to identify these two Napoleons, since they more or less literally operate in different worlds.

This reply, admirably consistent though it would be, would worsen two problems for Latour: one is that the features of the Napoleon III to whom Pasteur writes are not completely determined by the prior Pasteurian network. The other is that if the two networks are so thoroughly incommensurable that we cannot identify their Napoleon IIIs as the same entity, then any attempt to explain their different fates will beg the question how we could even conceive of comparing them.<sup>462</sup>

Latour's constructivism here is, I think, untenable, for a reason that is – after all and in spite of Latour's attempts to keep it at an arm's length – analogous to the reason why social constructivism fails: no more than physicists are able to single-handedly or collectively *create* quarks, is the specific Pasteurian network (both human and non-human actors included) strong enough to *create* its own French emperor in all relevant aspects, *or* to create microbes. Sufficient explanations for how this emperor or these micro-organisms behave are not to be found in the network: phrased differently, the entire network underdetermines who the emperor is or what microbes are. Where does the resistance to certain possible developments of networks take place, if the external world does not exercise an influence of its own and the internal structure of the network fails to determine its entire development? This is a question to which I believe Latour has, in the end, no answer.

#### 6.4.2 Tracing Networks

Then again, perhaps Latour has no problem embracing a certain open-endedness of events in one network. And perhaps the point of limiting oneself to one locality lies precisely in taking seriously the actor's perspective.<sup>463</sup> We can write a separate history of Napoleon III and the networks in which *he* moves, but if we write a history of *Pasteur's* networks, Napoleon III only enters the stage at a specific moment, and what we can assume and know

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<sup>462</sup> Latour seems to deal with the question of how Pasteur succeeded in "*withdrawing* Pouchet's common phenomenon [i.e. spontaneous generation] from space-time" (Latour [2000, 253]; see also *ibid.*, 255).

<sup>463</sup> Latour (2005, 235); Crease (2000, 22).

about who he is and how he reacts to letters will need to be restricted to what this specific Pasteurian actor-network allows us to trace. If we happen to know *other* things about Napoleon III from different studies, we need to suppress this knowledge, because it would detract from our purpose of seeing the internal consistency and stability of the Pasteurian network. Does following this maxim mean that we cannot talk about how things *really* are? No, and the reason why not should be obvious by now: what is real is a construction of this network. Latour has said that ANT's main tenet is that "actors themselves make everything, including their own frames, their own theories, their own metaphysics, even their own ontologies."<sup>464</sup>

If we write one story about microbes (if they have left behind traces)<sup>465</sup> and a separate story about Napoleon III, we do not just provide different interpretations of the same reality that can be made to match later; in tracing Pasteur's movements, we follow the construction of *ontologies* that are different from (and often incommensurable with)<sup>466</sup> the ontologies of other networks. And we need to take seriously those local ontologies.

But how do we demarcate networks? How do we know that Napoleon III or microbes are not part of Pasteur's network at a given time? Isn't it up to our arbitrary judgment how we cut up or unite different parts of history? No, because the network can be located only where it has left traces: we cannot arbitrarily say that we are studying the network of 'all of history' or of 'the universe', because that universalizes the local rather than *extending* it.

For that is how scientific theories increase their reach: by extending the networks of which they are part. Again, technology provides the paradigm. "Is a railroad local or global? Neither."<sup>467</sup> It is local at all points, since you always find sleepers and railroad workers, and you have stations and automatic ticket machines scattered along the way. Yet it is global, since it takes you from Madrid to Berlin or from Brest to Vladivostok."<sup>468</sup> This is the same for scientific theories. "No one has ever observed a fact, a theory or a machine that could survive outside of the networks that gave birth to them."<sup>469</sup> It is not just that theories are produced locally and then applied with more or less success *outside* (the laboratory, the local network); no, their application is synonymous with an extension of the local network to a bigger local network.<sup>470</sup>

There is no 'outside' to the 'inside', then,<sup>471</sup> and we can see this best by studying the movements on a micro-level: in the case of Pasteur, "if you watch carefully the prior displacement of the laboratory to capture farmers' interests, then to learn from veterinary

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<sup>464</sup> Latour (2005, 147).

<sup>465</sup> Latour (2005, 150).

<sup>466</sup> Latour (2000, 261).

<sup>467</sup> Latour seems to mean 'both' here.

<sup>468</sup> Latour (1993a, 117).

<sup>469</sup> Latour (1987, 248).

<sup>470</sup> Cf. also Latour (2005, 173-190; 1990, 153).

<sup>471</sup> Latour (1983, 155).

sciences, then to transform the farm back into the guise of a laboratory, it is still interesting, extraordinarily clever and ingenious, but it is *not* a miracle."<sup>472</sup> These are all concrete interactions: there is no miraculous action at a distance, which is about the only thing that Latourian metaphysics, in its Cartesian predilection for matter in motion, categorically forbids. Concretely, this otherwise open-ended metaphysics, in which every event in the history of science has to be understood in terms of things (of whatever kind) circulating in networks (made up of whichever entities), underlines and supports the call for an exclusive attention to actor's categories: we can be confident that we will not *need* to look beyond what traces have been left behind within the network that we want to study, because what it hasn't touched and what hasn't touched it doesn't exist to it; and we can be sure that we *ought not* look beyond the ontology created by the actors within the network, because that amounts to an invocation of non-existent entities and a betrayal of the actors.

And of course, this also excludes the ontology that *we* might bring to our investigation.

Abandoning the fixed frame of reference offered by ether, as physicists did, appears in retrospect a rather simple affair when compared with what we will have to let go of if we want to leave the actors free to deploy the full incommensurability of their own world-making activities. Be prepared to cast off agency, structure, psyche, time, and space along with every other philosophical and anthropological category, no matter how deeply rooted in common sense they may appear to be.<sup>473</sup>

## 6.5 When Are Electrons and Microbes Anachronisms?

### 6.5.1 Conceptual Anachronisms as Causal Anachronisms

The discussion has been expanded, then, to the general question to what extent we are allowed to bring our own categories and our own opinions or knowledge about the world with us when we study past or other societies. Latour clearly does not simply reiterate old arguments against anachronism,<sup>474</sup> but our considerations of 'Whig history', presentism, and anachronism in chapter 3 become relevant here.

One distinction we made there (section 3.3) was between 'conceptual' and 'causal' anachronisms: a conceptual anachronism consisted 'merely' in using current words and concepts in describing the past, where a causal anachronism consisted in asserting that something happened in the past that could not have happened – where 'could not', of course, still meant: could not *according to us*. This distinction, it seems, explains why we can legitimately say that there were electrons and microbes in Antiquity, while also upholding that saying that there were laboratories in Antiquity clearly constitutes a genuine error of anachronism (since not only did laboratories not exist, but their existence contradicts our

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<sup>472</sup> Latour (1983, 151).

<sup>473</sup> Latour (2005, 24-25).

<sup>474</sup> Cf. Latour (2000, 249).

positive beliefs about what was possible and impossible in Antiquity). More subtly, it shed light upon why things become more complex when we debate the question whether it is viciously anachronistic to call Aristotle a biologist: for some things, the existence of certain concepts in a culture is among the conditions for their existence in that culture, and this is evidently true for certain social roles. Since 'biologist' has a lot of meanings – among which are someone who studies living things, or someone who has a PhD in a modern academic field – and there are no clear rules that decide which of these meanings are essential, it can be unclear whether calling Aristotle a biologist is only conceptually anachronistic (applying a concept to him by which he did not actually identify himself) or causally anachronistic (calling him something that he was not and that he could at the time not possibly have been).

Now, how does this distinction relate to the questions we are presently dealing with? First, it brings the stakes into a clear focus: when we say (or silently assume) that there were electrons or microbes in the past, we do not need to be afraid to be accused *just* of conceptual anachronism: the easy counter-move there would be to profess that we are aware that 'electrons' here have a lot of cultural connotations that they did not have, say, in Antiquity, because the ancients did not have a concept of electrons. We mean 'just' the electrons themselves.

What we need to be afraid of, then, is the accusation of causal anachronism: the argument that the *conditions* for the *existence* of what *we* call electrons were not met in Antiquity. And we have also seen how such an argument might get off the ground: it might get off the ground by saying that among the conditions for existence of electrons is the existence of a concept of electrons – or better, that the two go hand in hand. This, indeed, seems to capture precisely what Latour is doing. Microbes, according to Latour, came into being in the 19<sup>th</sup> century because that is when they were added to our ontology: the histories and fates of the microbes themselves and of the concept of microbes are not different.<sup>475</sup> Latour rejects dualism of every flavour; we can expect that for him, conceptual anachronism merges completely with causal anachronism. And if every conceptual anachronism is a causal anachronism, then every anachronism is a vicious one. Which is precisely why we should only ever listen to the actors and study their networks.

How would this identification come about? One way to make sense of this is to say that 'electron' doesn't refer to anything other than what happens when we do experiments with oil-drops and write down the results, or when we apply any of the other modern scientific procedures in which electrons play a role. Electrons, we can imagine Latour saying, are everywhere now, since we have added them to our ontology; but all the places where we do the work through which this addition has become possible are places that did not exist in Antiquity. If *none* of the things (and relations between things) that work to fabricate electrons existed in Antiquity, then what do we refer to when we talk about electrons in Antiquity?

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<sup>475</sup> Latour (1999, 147-148).

### 6.5.2 Relativizing Relativized Existence

Does this not bring us back to square one, to the same impasse with which we started, namely that there is no way to get around Latour's insistence that things come to exist only when they are added to ontologies in local networks? No, for two reasons.

First, Latour's own vocabulary provides a solution: not only electrons and microbes are recent addition to our ontology, after all, but so are time periods like 'Antiquity' or 'ancient Egypt': a case can be made that in the sense in which we understand these things, they have been constructed very recently – and, crucially, that the addition of electrons and microbes to our ontology has effected a new construction of Antiquity and ancient Egypt. Before microbes were fabricated, we lived in a world with an Antiquity that did not contain microbes; but after the fabrication of microbes, our ancient Egypt changed as well as our present, and in this new ancient Egypt, there may well be microbes. Taking Latour's radicalism seriously, there is no point at which it stops, and its final implications turn out to be rather conservative. Rather than altering the way we look at the entities in our world and relativizing their existence, our whole world, including its past as we commonsensically tend to think of it, springs back into existence; the only thing that has changed is that it has been folded up, so to speak, in our own locality.

To clarify: Latour's notion of relativized existence is radical only if it is selectively applied: for instance, if we implicitly still believe that there is a nineteenth century independent of our own local networks, with which we can make contact and in which we can trace the actors until we discover that at one point (t1) in this nineteenth century microbes were not among those actors, whereas at a later point (t2) they were. In fact, t1 and t2 are both constructions fabricated in the present – and the present, as we know, is a locality in which microbes are everywhere. There is no reason why t1 would be excluded from this present – unless, again, we believe that something happens between t1 and t2 that is independent of our locality but that still pertains to it. For electrons in Antiquity the same holds: unless we believe that Antiquity independently existed long before our present, we need to consider the possibility that the ways through which we have fabricated and are continuing to fabricate electrons in the present have enabled electrons to travel to Antiquity as well.

This argument should satisfy anyone who is convinced by Latour's way of thinking about ontology; in fact, Latour applies it when he says that "just as historians are not forced to imagine one single nature about which Pasteur and Pouchet would make different 'interpretations,' neither are they forced to imagine a single nineteenth century imposing its imprint on historical actors."<sup>476</sup> One way to answer the question what we refer to when we talk about electrons in Antiquity, then, is to say that we are talking about a recently fabricated Antiquity that contains our recently fabricated electrons.

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<sup>476</sup> Latour (1999, 165).

Second, if we do not want to outflank Latour in his Latourianism, we do not need to. We have also seen in all our examples that we can differ about the question to which extent specific kinds are dependent upon the existence of specific concepts. And the recognition that it is *our* beliefs about the relations between concepts and reality that matter in history-writing, means that positions other than Latour's have become possible: Latour's approach is not the most general or neutral; it is one of a range of possibilities.

We have seen that when we use different concepts, we have different expectations regarding the conditions under which the phenomena described by those concepts can be realized. Now, we commit causal anachronism only when we assume the existence of phenomena in the past whose existence was impossible at the time because at that time specific necessary conditions for their existence were not met. Whether we believe this to be the case, then, depends on our expectations about these conditions and about the nature of these phenomena. Is what we mean by electrons exhaustively described by all the inscriptions of them in our own network? Possibly, but it is equally possible to mean something else; most scientists would claim that when they talk about electrons, they mean to refer to something in nature (whether or not they do so successfully). The dynamics of our own expectations, therefore, may just be more complex than the simple rule that the conditions for existence are always and always only met as soon as the related concept is added to our ontology. At the very least, this simple rule is just one end of the spectrum – the end taken by Latour.

Sure, there is room for more people at that end of the spectrum; perhaps Latour's metaphysical position is convincing to some, in which case they are right to avoid assuming the existence of electrons, microbes and the pre-Cambrian before the 19<sup>th</sup> century (or at least: before *some* 19<sup>th</sup> centuries; as we have seen, it is rather unclear why this should hold for our own 19<sup>th</sup> century). But these Latourians will then have to answer the objection that historicists have always had to struggle with, and which I consider to be insurmountable, that approaching the past without any prior concepts is impossible. Though Latour tries to be as consistent as he can in letting the actors decide on their own ontologies (a most admirable example being that when he says that Pasteur extends a network that spreads laboratory products "all over France", he adds that 'all over France' is itself "a construction made by statistics-gathering institutions"),<sup>477</sup> in the end it is not the actors talking but Latour – even 'actor' or 'actor-network' is not necessarily an actors' category, as both Latour himself and Nicholas Jardine have noticed.<sup>478</sup>

We always proceed from our own ontology. This we may modify and restrict for numerous reasons, including historical knowledge; but those reasons do not reduce to the

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<sup>477</sup> Latour (1983, 152).

<sup>478</sup> Jardine (2000, 263-265); Crease, Ihde, and Jensen (2003, 22), where Latour says: "But if you begin your fieldwork by presupposing a common world in the sense of positing that there exists a culture, that humans are defined by being in culture, have a body, genes, and neurons, then you are finished. You are not an anthropologist because you have already decided for the actors what is the world they have in common even if they refuse to have a body and a culture ... or for that matter to be actors at all."

simple rule that we are completely undecided and should ask the actors as if we had no clue what the world looked like.<sup>479</sup> In the end, this is nothing more than the pragmatic acknowledgment that we get our categories from multiple sources, not just from the ones we are looking at presently. Why should our study of 19<sup>th</sup>-century networks containing microbes refer only to concepts as defined by (and circulating in) the network we are studying? We don't use one case study of a 17<sup>th</sup>-century painting to define colors, even though we are allowed to refer to colors when we describe a 17<sup>th</sup>-century painting; we don't use one case study of ant societies to define insects, even though we are allowed to call the ants insects. Similarly, we don't use one study of 19<sup>th</sup>-century scientists talking about microbes to define microbes. We could, of course – it is possible to say: whatever Pasteur is doing in his laboratory, that is what constitutes microbes – but that would amount to a redefinition of what we usually mean by microbes. Therefore, the description of how Pasteur fabricates his microbes would fail to be an answer to the question how we came to believe in the existence of what we usually call microbes. This is a good reason to avoid such a redefinition. And unless we demand a definition of microbes along these lines, there is no reason why our own definition of microbes – and to be sure: we don't need something fixed and Platonic here; just think of something that covers 'the practices that determine when to speak of microbes; the states of mind we are in when we read the word', or whatever you think constitutes something like a definition – should be disqualified from being used in a study on 19<sup>th</sup>-century science only because it does not completely follow from the specific 19<sup>th</sup>-century network.

The assumption here, which Latour would probably find far too modest and unexciting, is that as historians we do not have a monopoly on the creation of ontologies. I assume that by and large, historians use the concepts available in their culture: they do not have the goal of radically changing our perspective on the universe we live in, of rewriting from scratch the list of entities that populate that universe. They have the rather modest aim of understanding a bit of the past – for its own sake but not therefore (because that is an impossibility) on its own terms, using whatever current-day explanatory concepts they can use without anachronism as defined by the current usage of those concepts and our current ideas about the past.<sup>480</sup>

This does not preclude the possibility that what they find in the sources modifies those concepts; it is important that it can, as will be discussed more extensively in chapter 8. It just means that their ontologies are not created merely by the historical sources that happen to form the basis of a particular historical study. They have been created by history, but this history is always larger than the aspects of it that are being investigated in one specific research project. Our concepts come with beliefs about the world and with expectations about which circumstances need to be met in order for the things that those concepts refer to, to exist. Those beliefs and expectations are usually not the *result* of one

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<sup>479</sup> Cf. Rule 5 in Latour (1987, 258).

<sup>480</sup> Tosh (2003; 2007, 198-209).

specific case-study (which would be the only proxy to their arising from the past local networks themselves, which are by definition inaccessible), and there is no reason why they should be.

But – retracing our steps further – if Latour has to concede this much (which is simply that there needs to be no wholesale *a priori* rejection of our inherited ontology for the sake of exotic local ontologies), then there is more that has to give. For if we are allowed to bring our own concept of microbes to our history of the 19<sup>th</sup> century, rather than being stuck with ‘the things that are fabricated by Pasteur and added to the network’, then what *our* concept means may be something *different* from what *we* think Pasteur’s concept meant – even if this meaning, to paraphrase Putnam, was not just in Pasteur’s head, but was a property of (what *we* think were the relevant aspects of) the entire local network. That is, the history of microbes can, again, have a relation to the history of the *concept* of microbes other than of identity. The things that Latour glued together to look like one thing immediately fall apart again.

## 6.6 Nature, Politics, and Critical Science Studies

When we phrase it carefully, it seems so simple: what *we* mean by microbes now is something conceptually distinct from what *we* think went on in 19<sup>th</sup>-century laboratories, farms, and statistics-gathering institutions – even if (no, *especially* if) it is not separable from what goes on in 21<sup>st</sup>-century laboratories *et cetera*. In fact, this is something that would have required no explanation to David Bloor or Harry Collins, as we saw in the previous chapter: the distinctness of objects and beliefs informed their program of explaining beliefs independently of objects.

The major reason Latour has tried so hard to conflate these things and to deconstruct or hide the distinctions, is his aversion to the mutual isolation of subject and object in what he calls the modern Constitution, of which the separation of microbes and beliefs about microbes would be a clear example. I am sympathetic to his forceful and consistent insistence that objects and beliefs do not have segregated histories that can be understood separately (and the whole previous chapter was an argument to that effect), but I think that he goes too far, and that this is in part because of his assessment of the political implications of the modern Constitution.

This is that the separation of things and people in this Constitution serves to “use nature to abort politics.”<sup>481</sup> By separating reality into two domains – nature and society – and decreeing that one of these domains should be one of certainty and unity, and the other one of disagreement, the open-endedness of politics becomes unduly restricted. This Constitution, essentially, gives an awesome power to those who can bridge the gap – or who

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<sup>481</sup> Latour (2004, 19 [italicization removed]).

can, in another metaphor now, leave the cave of society to look at nature and return<sup>482</sup> – because they are supposed to bring objectivity and unity to society.

The subtlety of this organization rests entirely on the power given to *those who can move back and forth between the houses*. [...] these few elects, as they themselves see it, are endowed with the most fabulous political capacity ever invented: *They can make the mute world speak, tell the truth without being challenged, put an end to the interminable arguments through an incontestable form of authority that would stem from things*.<sup>483</sup>

It is scientists who claim the power to represent things – and it is in this sense that, for example, Latour wants to call Pasteur political: not that the content of his scientific beliefs somehow mirrors his orientation in the constellation of human political interests, not that he sought to get involved in elections and law-making and used his scientific work for that purpose; but that he put himself forward as a spokesman of forces that he had made visible (the microbes).<sup>484</sup> Because the scientists speak for objects to which they have exclusive access and which are so construed as to be unable to speak in any other way than through the scientists,<sup>485</sup> what they say acquires a disproportionate authority that serves to depoliticize science and immunize it from criticism. “Like all modernist myths, the aberrant opposition between mute nature and speaking facts was aimed at making the speech of scientists *indisputable*; thus, this speech passed through a mysterious operation resembling ventriloquism, to ‘I speak’ to ‘the facts speak for themselves’ to ‘all you have to do is shut up’”.<sup>486</sup>

This is an interesting perspective on the source of authority of the natural sciences in modern societies, and the political aim to draw science from a realm of immunity to criticism to one of pluralism and open-endedness is an understandable and, if I may say, sympathetic one. However, the question is whether the diagnosis is right and whether Latour’s metaphysical radicalism is a proportionate and effective antidote. The separation between science and politics is nowhere as strong as in Max Weber’s lectures on *Science as a vocation* and *Politics as a vocation* – in that sense, they are excellent examples of the modern Constitution. However, their separation here is one not between things and people, but between ‘is’ and ‘ought’: science seeking to provide knowledge about what is the case, and politics deciding what, given this information, should happen.<sup>487</sup> There is a plurality of possible answers to both questions, given that which questions science seeks to answer is decided by values that are, in the end, subjective; but true answers to the question what is

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<sup>482</sup> Latour (2004, 13-14).

<sup>483</sup> Latour (2004, 14).

<sup>484</sup> Latour (1983, 156-159).

<sup>485</sup> Latour (1993, e.g. 30-32).

<sup>486</sup> Latour (2004, 68).

<sup>487</sup> Weber (1919a; 1919b). Latour (2004, 115) makes the same distinction, but it remains associated with the divide between nature and society.

the case are indeed, in the end, logically in harmony because there is just one reality, whereas answers to the question what should happen can be in conflict, and are therefore the domain of power struggle – of politics.

Latour should not forget that the social sciences, too, strive for objectivity: they seek to ‘represent’ people just like natural scientists seek to ‘represent’ things – and this goes to show the double meaning of ‘representing’:<sup>488</sup> claiming to say true things about X is different from claiming to defend the interests of X – the second is clearly not implied by the first. Still, it is possible to say that, in practice if not in theory, reality, claims about what reality is like, and claims about what reality should be like, are interwoven in such complicated ways that it is misleading to subject them to completely different kinds of expertise in society, with completely independent sources of authority and mechanisms of control. Latour may be right to that extent.

However, I do believe that he makes a severe misjudgment when he tries to put the blame on nature, on the idea of a shared external reality. “When the most frenetic of the ecologists cry out, quaking: ‘Nature is going to die,’ they do not know how right they are. Thank God, nature is going to die. Yes, the great Pan is dead. After the death of God and the death of man, nature, too, had to give up the ghost. It was time: we were about to be unable to engage in politics any more at all.”<sup>489</sup> The reason why Latour cheers the death of nature, as we understand now, is because supposedly apolitical, it has inescapably served political goals – since of course the river is *not* separated in two banks, and the separation of science and politics is therefore inescapably something of a fiction: “never has anyone appealed to nature except to teach a political lesson.”<sup>490</sup>

Different radical conclusions may be drawn from this: the modernist, Weberian one would be to say that, if this is the case, science and politics have not yet been separated *enough*; the other, Latourian one would be to say that the modern Constitution is dead and something completely different should come in its place. Any other normative conclusion could be built on the same insight, including the more moderate conclusion that things should go on roughly as they do now. An interesting question at this point is which move would be most conducive to the possibility to subject science to external criticism. This, after all, seems to be Latour’s main point in his rejection of nature: “if we have to give up nature, it is neither because of its reality nor because of its unity. It is solely because of the short-circuits that it authorizes when it is used to bring about this unity once and for all, without due process, with no discussion, outside the political arenas, and when something then intervenes from the outside to interrupt – in the name of nature – the task of gradually composing the common world.”<sup>491</sup>

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<sup>488</sup> This undermines the symmetry suggested in Latour (1990, 154-159). Here as before, Lynch (1996, 249-250) has a point when he suggests that Latour may be ‘bewitched’ by his language.

<sup>489</sup> Latour (2004, 25-26).

<sup>490</sup> Latour (2004, 28).

<sup>491</sup> Latour (2004, 91).

Latour's program of studying 'science in action' and 'following scientists and engineers through society' comes down in the end to a prolonged attempt to deny scientists any shortcuts: to extract from them precisely what they actually do, without any myths or other embellishments. Latour's is the laudable goal of a criticism of *power*:

People who speak of nature as if it were an already constituted unity that would make it possible to throw back onto social representations everything that calls for disunion – such people *exercise a kingly power*, the most important of all, a power superior to all the purple mantles and all the gilded scepters of civil and military authorities. I ask no more of them than one minuscule concession: since you have granted yourself the power to define what unites us and what drives us apart, what is rational and what is irrational, show us also the proofs of your legitimacy, the traces of your election, the motivations for your choices, the institutions that permit you to exercise these functions, the *cursus honorum* through which you have had to make your way.<sup>492</sup>

But is dispensing with nature in the sense defined in this passage a good way to increase the possibility for such criticism? I would say that it is precisely because of the intention to talk about a common world that communication, and therefore criticism, becomes possible.<sup>493</sup> If I say that the earth goes round the sun, and you say that the sun goes round the earth, the conclusion that apparently we live in two different solar systems is not one that fosters mutual criticism. Note that this is not an argument against the idea that we live in two different solar systems; only an argument to the effect that the assumption that we do is not a step towards a criticism of scientific authority. The 'minuscule concession' that Latour demands is a step towards that – an insistence on being able to retrace where my claim that the earth goes round the sun came from (and, possibly, to discard that claim depending on where it came from) – but this retracing becomes easier the more the worlds we live in overlap.

In that sense, the strategy that we saw Harry Collins take in the previous chapter – to look at what scientists claim to be talking about, and then proceed to show that the dynamics of scientific decision-making can often be understood in terms other than those that scientists claim are important – is much more critical than the strategy that Latour advocates: to criticize absolutely nothing because actors are supposed to build their own ontologies.<sup>494</sup> Only when we agree (however tentatively and potentially subject to revisions based on new insights in either nature or history!) on what scientists are trying to do can we discuss questions as to whether what they do fails, succeeds or goes further than they claim to, if we are interested in such questions.

It may be that SSK and Latourian actor-network theory both fail in making historical knowledge about science a departure point for possible criticism. If SSK fails to do

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<sup>492</sup> Latour (2004, 222).

<sup>493</sup> For a similar view, see Rescher (1987, 136).

<sup>494</sup> Cf. Collins (2012).

so, it is because in refusing to refer to nature in what it says about science, it cannot account for those features of science that relate to nature. When it says: “you think you are talking about nature, but in fact what you do refers only to society”, it states not its conclusions but repeats its axioms – axioms that, moreover, cannot be plausibly upheld, as we saw in the previous chapter. Nonetheless, SSK provides the possibility of saying that what scientific actors want or claim to achieve is not actually what they are achieving – the claim that scientists’ actions are oriented only upon the social world rather than upon the natural world is already an attempt at criticism. Subsequent descriptions of the nature of this orientation upon the social world – e.g. based on furthering one’s own professional interest rather than on finding out truths – might constitute a further criticism. Latourian actor-network theory fails in a more fundamental way, because it collapses the external in the internal; it conflates nature with what happens in local networks, and thereby takes away even the conceptual possibility that what is said about the world is not what is actually going on in the world.

## 6.7 Science and the World

This, in fact, is a repeatedly stated aim of Latour: to remove the *problem* of the relation between science and the world; to show that the way scientific thought relates to nature is not something magical or unreasonably effective, but a direct result of their co-creation. “Most of the difficulties associated with science and technology,” says Latour, “come from the idea that there is a time when innovations are in laboratories, and another time when they are tried out in a new set of conditions which invalidate or verify the efficacy of these innovations. This is the ‘adequatio rei et intellectus’ that fascinates epistemologists so much.”<sup>495</sup>

It is now clear where Latour thinks the fascination comes from, why according to him the *explanandum* does not exist, or at least not in such a radical form, and why it is important to make this point and to reinterpret the problem. The fascination results from the idea that something constructed by humans in society can ‘match’ something that has come to exist completely independently of those humans. This leaves the huge problem of understanding how the gap between subject and object has come to be bridged in such an effective way. Meanwhile, the experts whose job it is to bridge this gap derive a huge amount of authority from their success at doing so. All of this, however, makes sense only under the assumption of the ‘bifurcation of nature’:<sup>496</sup> the idea that nature and ideas about nature exist separately, like the two banks of a river, and that their connections are problematic.

Latour thinks that we tend to see scientific theories as abstract objects floating above the (empirical or experimental) world. When they then turn out to be ‘applicable’ to

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<sup>495</sup> Latour (1983, 155).

<sup>496</sup> Latour (2008a, 10-13).

what happens below them, it seems to be miraculous. In fact, Latour says, they function in no more miraculous a way than any other centers or hubs within networks: “Miracle indeed to see a clover-leaf intersection fitting *precisely* with the freeways whose flow it redistributes!”<sup>497</sup> It is, of course, not a miracle, because it is evident that freeways and their intersections are constructed to match each other. In the same way, microbes and the laboratory environment that identifies them are constructed in the same movement, and when Pasteur’s predictions ‘work’ outside the laboratory, this is the result of work done to extend the reach of the laboratory; “it is [...] *not* a miracle.”<sup>498</sup>

Especially, it is not a miracle that takes place in the minds of the scientific genius: if there is this miraculous match between reality and theory, an unexplained *adequatio rei et intellectus*, it may seem like a good strategy to take a close look at the minds that have generated these adequate ideas. For Latour, this is looking at the wrong place, and he devotes one of his rules of method to this point: “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.”<sup>499</sup> Latour’s move has, again, a bit of a Cartesian flavor to it: apparently we could try to explain science by the special intellectual capacities of some humans, but it is much better to explain it by the movement of things.

Thus, in this methodological materialism or monism, the dualist problem of the correspondence between intellect and world disappears. What are we to think of this position, in the light of the preceding discussion with Latour in this chapter? The most important conclusion would be that, in rescuing and affirming the possibility of divergence between what we think about microbes or elementary particles and how we think past scientists thought about these, or between how our network fabricates microbes or elementary particles and how our network fabricates past networks containing these, we have placed a wedge between Latour’s monism at an important point: the structure once again cracks neatly into its two constituent parts of (our views – always our views – of) the world and (our views of) its past representations.

What, then, remains of Latour’s glue- and patchwork? It is important to note that we did not place the wedge at the point that matters most to Latour: the conceptual split between nature and society. He can be satisfied to see – and we have learned this from him – that we have remained undecided about how much of our own representations of things is ‘due to’ nature and how much is due to society. We need to think of our notion of microbes as neither culture-free nor nature-empty, and the same holds for past notions of microbes. We are even permitted to think of past microbes ‘themselves’ in ways other than as things on one side of a nature-society-divide – I say ‘permitted’, because this would be the point

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<sup>497</sup> Latour (1987, 242).

<sup>498</sup> Latour (1983, 150).

<sup>499</sup> Latour (1987, 258).

where I might like to opt out and permit myself instead to say that microbes ‘themselves’ are natural and not part of human society.

But that is not the point now. The point is rather this: the relation between science and the world can, again, become a problem – a status that Latour would deny it. But this is not the problem of a pre-established harmony between nature and intellect, a problem to which there would indeed be no mundane answers; rather, it is the problem of how one conceptually delineated part of the world relates to another conceptually delineated part of that world. (The reason why these are two conceptually distinct parts of the world is not that they are substantially different, but ‘simply’ that we relate to them in different ways.)

Nor is this problem a problem of correspondence, mimesis or adequacy. Almost all the views we have come across in the last two chapters commit the straw-man fallacy of identifying the view that reality independently constrains views about itself with the idea that scientific theories correspond to the world, and reason from the untenability of correspondence to the omnipotence of society (in Bloor’s case) or to the untenability of a nature-science distinction (in Latour’s case).<sup>500</sup> Once pointed out, it is easy to see that this reasoning fails, for the simple reason that science *could* have another relation to the world than one of ‘matching’ or ‘corresponding’.

What relation, then? Can we say things, in general, about ways in which the world can *constrain* science – exercise causal influence upon it? And can we do this while taking into account Latour’s lesson that we should not overlook the fact that ‘science’ and ‘nature’ are not causally isolated entities with one or two designated points where bridges between them may be built – while taking into account, in other words, that we have only conceptually distinguished them, and that in fact they are connected by causal interaction throughout history on an indefinite number and variety of occasions?

Perhaps a promising way to do this is by seeing science as adaptive to nature, in a way analogous to the adaptation of life to its natural environments. In that case, after all, there is no problem in saying that we can conceptually separate animals, for instance, and their natural environment; or in saying that these animals rarely ‘correspond to’ their environment simply by ‘representing’ or ‘mimicking’ it (instances of camouflage notwithstanding of course). Both these claims can be harmonized with the idea that environments underdetermine what animals live in them, *and* with the idea that there are indefinitely many local interactions between animals and their environment, rather than a bunch of animals on the one hand and an environment on the other which are subsequently ‘bridged’.

Last but not least, there may be a hint here at how to solve the problem of their relation without resorting to miracles. After all, it may seem that animals are miraculously well attuned to their environment – just like it may seem that science is miraculously well attuned to nature (or that clover-leaf intersections would be miraculously well attuned to

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<sup>500</sup> Cf. Galison (1987, 11) for a similar point on Andrew Pickering.

freeways were we not allowed to assume intelligent design by humans here). But in the case of the biological world, we have access to theories that provide completely naturalistic explanations of this adaptation – explanations that have in common that they do not approach animals and their environment as having come to be independently and complete in their current shapes, but that the shape of animals (and of their environment) is the result of a historical evolutionary process.

In the next chapter, we will explore a family of approaches to the relation between science and the world that attempt to grasp this relation in a naturalistic, causal way.

## 6.8 Conclusion

From our evaluation of Latour's perspective on the study of science in history, we can draw the following conclusions:

- 1) Latour has successfully undermined an essentialistic opposition between nature and society, where science belongs wholly to the social domain and its relation to nature corresponds to the bridging of an ontological gap. He has done so especially by appealing to the constant interactions between human and non-human entities in networks.
- 2) Latour succeeds in showing that there are things that are fruitfully considered as *both* robust *and* a historical product – both 'real' and 'fabricated'. This can be the case for the content of our science as well; its explanation is then properly historical.
- 3) Latour's conflation of the world with its representations does not hold up to scrutiny. This means, first, that it is still possible to talk about the relation between science and the world that it is about, and, second, that there is no reason to believe that our representations of the world are causally anachronistic with respect to periods in which they are unavailable.