



Mariya Konstantinova. A Review of **Bruno Latour** *Nauka v deistvii: sleduy za uchenyimi i inzhenerami vnutri obshchestva* [Science in Action: Following Scientists and Engineers within Society]. St Petersburg: Izdatelstvo Evropeiskogo universiteta v Sankt-Peterburge, 2013, 414 pp. [Russian translation of: Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, MA: Harvard University Press, 1987]

Bruno Latour's Dramatisation of Science: the Production of Impressions in Action

Among Russian scholars Bruno Latour has acquired a firm reputation as a post-modern philosopher and ethnographic anarchist. The latter is well deserved, for his open contempt for methodology and his thesis 'Follow the actors!', which Russian researchers usually treat as if it meant 'Follow Latour!' or 'Anything goes!' Latour is sometimes situated as a sociologist of science and ethnographer [Kharkhordin 2010; 2013], and sometimes as a theoretician of material objects and a sociologist [Vakhshtain 2012; Napreenko 2013]. Some people are for putting Latour's theory into practice [Alapuro et al. 2012; Kuznetsov, Shaitanova 2012], while others regularly warn against it [Vakhshtain 2013; Popov 2014].

But what about those of Latour's works that cannot be so readily pigeonholed? Those which belong neither to the completely empirical ethnography of the laboratory [Latour 1986] nor to the most general theory of the social [Latour 2013]? *Science in Action*, in particular, is one such book, published in 2013 by the European University in St Petersburg Press.

Science in Action was first published in 1987, eight years after the book that had made Latour's name, *Laboratory Life: The Social Construction of Scientific Facts*, and a year after a second edition of that book had been published without the word 'social' in its title. By that time Latour

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was no longer studying the ethnography of the production of knowledge. He had conceived an enthusiasm for theorising, and was developing the actor-network theory together with Michel Callon and John Law. It is interesting that Latour himself says that this, his most ambitious theoretical project (ANT), was born between 1986 and 1988, when Law published his work on Portuguese ships, Callon his on scallops [Callon 1896], and Latour himself on Pasteur and pasteurisation [Latour 1988]. As he puts it in his discussion of network theory, this was where non-humans — microbes, scallops, stones, and ships — were seen by social theory in a radically new way [Latour 2014: 23].

Nevertheless, *Science in Action* has not become part of the actor-network theory canon. It contains none of the scandalous subjects of ANT: no agency of things [Napreenko 2013], no generalised symmetry [Latour 2014], no interobjectivity [Latour 2007], and no semiotics of materiality [Vakhshtain 2006]. It is no longer empirical research, but it is not yet philosophy. The genre could be most correctly defined as ‘meta-ethnography’, the self-observation of an observer of observers.

Latour begins his narrative with a gripping subject.

1985. The Institut Pasteur, Paris. Visiting programmer John Whittaker creates a programme for visualising three-dimensional models of DNA molecules on the new Eclipse MV/8000 computer.

Flashback.

1951. Cavendish Laboratory, Cambridge. Unknown to their chief, the young researchers Jim Watson and Francis Crick are working with X-ray photographs of DNA, trying to beat their nearest rival, the famous chemist Linus Pauling.

Change of scene.

1980. Data General Offices, Massachusetts. Tom West (also without his boss’s knowledge) is working on developing the Eagle computer (which a few years later will be the prototype for the Eclipse MV/8000), trying as hard as he can to outdo his competitors at DEC (pp. 23–4).

The moral of this story is simple. In order for Whittaker to write his new programme in 1985, which would significantly advance the study of DNA, he had to have available two working ‘black boxes’: the *scientific fact* (Watson and Crick’s model) and a *powerful computer* (Tom West’s Eclipse). Latour narrates the production of knowledge as a tense struggle to convert the debatable into the indubitable, things that don’t work into things that do work, a ‘box of junk’ into a ‘black box’. As this little sketch shows, he sees no difference between technical adaptation and scientific discovery. Both are well made

(arte)facts. ‘The word **black box** is used by cyberneticians whenever a piece of machinery or a set of commands is too complex. In its place they draw a little box about which they need to know nothing but its input and output. As far as John Whittaker is concerned the double helix and the machine are two black boxes’ (p. 25). Nor is there any difference between the scientist and the engineer: they are both engaged in ‘technoscience’ (p. 278).

How could this subject be interpreted by devotees of the sociology of things and of the material turn? The computer is the actant, an active participant. It acts together with John Whittaker in the creation of a new scientific fact. Accordingly, the development of research into DNA after 1980 is the result of joint efforts, of collaboration between people and things — engineers, computers and scientists. But what about Watson and Crick’s model? It is not a material actant, either human or non-human. Here proponents of the actor-network theory have to make a deal with their conscience and acknowledge the agentivity of ideal beings, ‘because “non-humans” are not only scallops and liner pumps. They are also the “imperial identity” that is added to the technologies of Portuguese colonial expansion, and the mathematical equations that operate on the market alongside goods and traders’ [Vakhshayn 2014: 37].

But then we read on in Latour’s book and we discover that scientific fact (an ideal entity) and technological artefact (a material entity) are indeed juxtaposed — *but not as active participants, but as ‘black boxes’ whose main purpose is to be silent mediators between people*. Black boxes are devoid of agentivity. They do not act, but accumulate in ‘condensed form’ the actions of many scientists and engineers. There is no generalised symmetry here. Watson and Crick’s model and West’s computer are not actants.

And even if they were... In Latour’s book actants are not active subjects, but the silent and voiceless majority represented by competent actors. ‘As I showed above, both people able to talk and things unable to talk have spokesmen. I propose to call whoever and whatever is represented **actant**’ (p. 143; author’s emphasis). Whereas in his later work Latour insists upon the idea that ‘To do is to make happen’ [Latour 2007], in *Science in Action* we are presented with quite a conservative theory of action: things and ideas require delegates, without whom not only do they not act, they do not exist. *Science in Action*, it would seem, can destroy all our ideas about the actor-network theory, Latourianism and the material turn.

It is no accident that this is a text by Latour that is viewed favourably even by his most radical opponents, the ‘traditional’ sociologists of science. In essence, half the book describes which rhetorical strategies scientists use to demonstrate the reliability of their conclusions.

Latour analyses the modalities of their utterances, the practice of forming ‘alliances’, the many rhetorical techniques of persuasion to which researchers have recourse in order to legitimise the results they have obtained. He writes about the relationship between writers and readers of scientific texts: ‘The writer draws so many pathways going from one place to another and asks the reader to follow them; the readers may cross these paths and then escape. <...> All the objectors’ moves should then be controlled so that they encounter massive numbers and are defeated. I call captation (or **captatio** in the old rhetoric) this subtle control of the objectors’ moves’ (pp. 101–2). In Latour’s model scientists are outstanding politicians, generals and strategists. But... how does this differ from the much more usual sociology of science — Pierre Bourdieu’s version, for example [Bourdieu 2014]?

I will risk suggesting that the key distinction of Latour’s analysis is not focusing on the agentivity of things (which does not exist), nor following the actors (he doesn’t), but a brilliant *theatrical analysis* of his ‘field’. Moreover, Latour prefers to base his analysis on rhetorical categories, forasmuch as scientists’ primary aim is to convince other people that they are right: ‘Rhetoric is the name of the **discipline** that has, for millennia, studied how people are made to believe and behave and taught people how to persuade others’ (p. 60).

But let us return to our original subject of the invention of the computer and the discovery of Watson and Crick’s model. What is the most striking thing about Latour’s narrative? Its overtly dramatic character — flashbacks, changes of scene, instantaneous transitions from scene to scene, short cuts. The brilliant construction of a parallel composition of two intersecting plot lines: engineering and science.

This is the climax of the engineering line: ‘Tom West sneaks into the basement of a building where a friend lets him in at night to look at a VAX computer. West starts pulling out the printed circuit boards and analyses his competitor. Even his first analysis merges technical and quick economic calculations with the strategic decisions already taken. After a few hours, he is reassured. <...> Looking into the VAX, West had imagined he saw a diagram of DEC’s corporate organization. He felt that VAX was too complicated. He did not like, for instance, the system by which various parts of the machine communicated with each other, for his taste, there was too much protocol involved. He decided that VAX embodied flaws in DEC’s corporate organization. The machine expressed the phenomenally successful company’s cautious, bureaucratic style’ (p. 28).

And here is the climax of the scientific line: ‘Peter’s face betrayed something important as he entered the door, and my stomach sank in apprehension at learning that all was lost. Seeing that neither Francis nor I could bear any further suspense, he quickly told us that the

model was a three-chain helix with the sugar phosphate backbone in the center. This sounded so suspiciously like our aborted effort of last year <...> To their amazement, the three chains described by Pauling had no hydrogen atoms to tie the three strands together. Without them, if they knew their chemistry, the structure will immediately fly apart. <...> When Francis was amazed equally by Pauling's unorthodox chemistry, I began to breathe slower. By then I knew we were still in the game. <...> When his mistake became known, Linus would not stop until he had captured the right structure. <...> We had anywhere up to six weeks before Linus again was in full-time pursuit of DNA' (p. 30).

Latour is not a sociologist analysing the scientists' rhetoric, he is a dramatist opening up the dramatic, compositional structure of the production of knowledge, although his drama is more cinematic than theatrical, as he gives the unities of time and place, expressiveness, verisimilitude, and other obsolete canons of theatrical production short shrift. In this light attempts to 'cross' Latour's sociology of science with Erving Goffman's dramatic theory [Vakhshstain 2007] do not look so ridiculous. Early Latour and early Goffman really do have a lot in common.

Furthermore, some 'insights' of dramatised sociology could widen the analytical frame of Latour's meta-ethnography. In essence, the theatrical metaphor's fundamental innovation is the idea of *expressive intention*: people behave as they do not out of material self-interest, but to make an impression on others [Goffman 2000]. What does the application of this idea to the field of science give us? 'Imagine for a minute,' writes Vakhshstain, 'that our accustomed routine of scientific communication (including writing articles, giving conference papers, attending dissertation defences and the dinners that follow) is not a dreary manufactory of academic solidarity, not an arena for the desperate struggle for symbolic resources, but a stage on which an amateur production of doubtful quality is being mounted. The director has gone AWOL, the stars are writing their own monologues, the stagehands are making arbitrary changes to the scenery, and the competing troupes are making up for the lack of an audience by a superabundance of performers. Theatre of the absurd? But the absurd has its logic too. People in this hermetic world are moved not by the urge to gain advantage, nor by the habits inculcated by their socialisation, but by an insurmountable urge to express themselves, to present themselves to other people within the aura of the role that they are playing (the so-called "expressive intention")' [Vakhshstain 2003: 32].

In my opinion, a little injection of theatrical description could do Latour's research nothing but good. It does in fact have all the prerequisites for such a theatrical revision. However, Latour gets too

carried away with his analysis of rhetorical strategies, and forgets what motivates his heroes. In Latour's version their only motivation is to convince the others that they are right. But let us examine a few cases that would look well in the book under review.

1. In 1948 the *Physical Review* printed an article by three authors, Alpher, Bethe and Gamow [Alpher, Bethe, Gamow 1948]. The article had in reality been written only by Alpher and Gamow, but, as one of them explained, 'It seemed unfair to the Greek alphabet to have the article signed by Alpher and Gamow only.' The well-known physicist Hans Bethe had had nothing to do with Alpher and Gamow's research, but he enjoyed the joke and was listed as a co-author.

2. Professor William Hoover of Lawrence Livermore National Laboratory listed an imaginary Italian professor 'Stronzo Bestiale' (roughly, Italian for 'uncouth bastard') among his co-authors after an article had been rejected by two journals. The addition of an author with such an outlandish name was of assistance to the authors of the article, which was accepted for publication by the *Journal of Statistical Physics*. Ever since this author with his unfortunate name has been a sort of talisman for Professor Hoover, who has 'co-written' more than a dozen papers with him [Moran, Hoover, Bestiale 1987].

Are scientists aiming to achieve a particular rhetorical persuasiveness when they manipulate their references, sign their articles with pseudonyms, cite each other sarcastically, or express gratitude instead of citing? Or are these all parts of the impression management machine? And how does the idea of expressive intention mesh with Latour's theatrical analysis — the clashes of antagonistic 'representatives', delegation of voices, and use of instruments and apparatus?

These are questions worthy of further consideration. For the time being we shall note that the publication in Russian of such an important book by Latour (and one undeservedly neglected by exegetes) is yet another act of rhetorical captation, yet another scene in the sociological drama of the Latourification of the Russian academic community.

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