Social Theory

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Let it Fly High! On the Need for ANT with a Positivistic Inclination

Abstract: Actor-Network Theory has proven to be highly successful, fulfilling much of its early theoretical and methodological promise. Proponents of ANT have argued, among other things, that an acceptance of the specific (techno-)social ontology which assumes consistent relativity of beings and anti-essentialism will enable us to address the aporia that are haunting sociology. The authors argue that, sociological applications of ANT (at least as regards the dominant understanding of the theory) result in a lowbrow methodology leading to a radical cognitive limitation of the discipline. The text finishes with an attempt to sketch an alternative version of ANT, one with a positivistic inclination opening the path for synthetic sociology.

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Actor-Network Theory (ANT) has undoubtedly been successful. This is evidenced not only by high citation rates of its proponents and the discussions which have been triggered by it, but also by the numerous attempts by mainstream sociologists to apply ANT in whole or in part (Becker 2007: 160; Castells 2009: 45). The impact of ANT is not limited to sociology: attempts at applying it can also be found in archaeology (Dolwick 2009), cultural anthropology (Oppenheim 2007), cultural studies (Kien 2009), geography (Van der Duim et al. 2013), theory of organisation (Alcadipani and Hassard 2010), theory of marketing (Bajde 2013), the theory of international relations (Barry 2013) and information systems research (Mähring et. al. 2004).

The popularity of the approach may be partially explained by its radical programme and the promises it makes. The proponents of ANT have claimed, for example, that they have discovered the missing elements of sociological explanations—the non-human actors—without which it is impossible to understand, predict, or design social processes or institutions (Latour 1991, 1992). They have also argued that acceptance of the specific (techno)social ontology which assumes a consistent relativity of beings and anti-essentialism (see e.g. Latour 1999a), makes it possible to handle the anthropological paradox which is haunting sociology. This paradox is the assumption that while a society shapes individuals, at the same time the capability of individuals to go beyond their society is not only the basic source of social dynamics, but also an epistemological condition for gaining knowl-

edge about that society. The promises of ANT go even further: the approach aims to help deal with certain practical problems: (1) the unavoidable uncertainty and risk generated by modern technologies, and (2) the complexity of social systems (Beck 1992; Perrow 1999).

However, we should remember the lesson given by ANT itself: there can be no scientific, social or technological change without incurring a cost or engaging in a negotiation, and consequently, a series of compromises must be made (Latour 1987, 1999b). This also concerns the implementation of elements of ANT in sociology. Thus, what is the price which sociology will have to pay if it is to accept what ANT has to offer?

We shall argue that the application of ANT (at least in the dominant reading of the theory) results in a lowbrow methodology in sociology, leading to the cognitive 'grounding' of the discipline. The reason for this is that within ANT sociology has not been read accurately, both regarding its previous achievements and its potential. Taking into account the historical genesis of ANT, this would seem inevitable and this is why we focus on development paths which the proponents of ANT have not explored. Here, we ignore the contradictory signals sent by advocates of ANT who simultaneously present it as part of sociological theory (Latour 2005), while also insisting on a distinction between it and such theory (Latour 1999a). We argue that sociology should sever the bonds which connect it to ANT for its own good. This is in marked contrast to the present trend to incorporate ANT into sociology, exemplified by the approach adopted by the anthology *Contemporary Sociological Theory* (Calhoun et al. 2012) and in the handbook *Sociological Theory* (Ritzer 2011). Cutting the bonds does not imply total rejection however. We argue below that what sociology needs is highly selective approach towards ANT tradition. We may risk oversimplification here by saying that it is advisable that sociology keeps ANT findings rejecting its methodological programme.

The Strong Program 2.0

As claimed by Bruno Latour, one of the main proponents of ANT, it can be problematic when sociology attempts to be the provider of categories to explain the mechanisms for producing scientific knowledge. In his view, sociology is part of the problem, not part of the solution. Sociology cannot play a different role not because it misidentifies science but because of its cognitive powerlessness as regards society. He claims that contrary to statements by some sociology of scientific knowledge (SSK) proponents (see Collins 1981), sociologists do not have any better insight into what is social than do natural scientists: the latter ultimately modify the essence of social processes and phenomena in a surprising way, and consequently explain them better (Latour 1988a: 38). As he argues, to expect that sociology will explain science 'is a bit like expecting the water distribution companies to "explain" the telephone networks' (Latour 1988b: 161).

We would like to stress that the application of ANT implies a specific understanding of the process of explaining and of what is social. Here, it is necessary to refer to two elements of ANT: (1) the postulate of 'generalised symmetry' and (2) the model of scientific explanation as the intertwining of heterogeneous networks. Both these ideas complement each other and both are necessary to fully understanding the work of those using ANT.

The term *generalised symmetry* is not accidental: it is a reference to the third point of the strong programme of the sociology of knowledge (Bloor 1991). ANT criticised the strong programme not because it was too radical but because its proponents did not go further: they did not generalise symmetric explanations (Callon 1986). In Bloor's view, the role of SSK is to explain divergent statements, both true and false, scientific and non-scientific, in the same categories. ANT accepts this but it rejects the very basis on which SSK wants to build explanations, that is the knowledge of society. Since our approach is symmetric, why should we assume that explanations of a sociologist are more reliable than explanations of a natural scientist, the effects of whose work are analysed by the former? Here, ANT is consistent in trying to realise the fourth requirement of the strong programme: the postulate of reflexivity. Let us examine more closely the way ANT perceives scientific controversies and the process of their resolution.

In every argument between competing scientific positions, social and material, as well as natural factors play a role. These factors include people, symbols, institutions, authority, statements, social activities and laboratory equipment, along with Nature as a whole which stands behind them. Here there is consensus between SSK and ANT. They differ as regards the interpretation of which category of beings decides about the settlement of the argument. SSK wants to explain one group of factors with the other, while ANT considers them to be closely connected with each other, concluding that resolving the controversy results in not only a new state of the world described by science (a new social definition of nature) but also a new state of society.

According to ANT, the thesis which successfully survives a scientific controversy is the one which explains reality in the best way, which means that the authors of the statement have mobilised more powerful resources to support it and have fortified it more effectively. Such support and strengthening may take the form of merit arguments, but also references to texts, allies in form of investors, scholars, citizens, prestige and authority, catchy metaphors, expensive apparatus, and even the encapsulation of knowledge in various social institutions. Nature does not speak for itself: it needs advocates. However, when a controversy arises, competitive researchers also build a network of factors to stabilise facts, as well as trying to increase the costs of the possible deconstruction of these facts, and on the other hand, trying to deprive their opponents of their resources. The stronger the network, the more objective the fact is. Yet there is always the possibility of dismantling it: it is a question of the costs which have to be incurred. The rebutting of a scientific theory, an unsuccessful innovation or a technological systems failure, the collapse of an institution or organisation is nothing but the unravelling of the heterogeneous network.

We wish to stress that the same rule applies to all kinds of explanations. A good explanation is merely a stable network of factors supporting a specific thesis or an entire theory. Methodological research accuracy, the corresponding theses and reality amount here to the stability of a network of connections and to whether we are able to protect it against collapse or purposive dismantling (even by a competitive researcher).

To sum up, ANT meets the postulate of symmetry and reflexivity, avoiding the criticisms raised for example by Woolgar (1981) and Ashmore (1988). However, there is a price to be paid: it has to reject the postulate of the causal explanation, which in the strong pro-

gramme was a sociological explanation (ANT is a-sociological) perceived in a positivistic way. In other words, ANT partially waives theoretical claims.

Lowbrow Methodology

To demonstrate how to overcome certain conceptual limitations is certainly worthwhile in a philosophical plan, but is it a good compromise from the perspective of research techniques? Perhaps ANT is conceptually elegant, but is it useful in a methodological plan, for example, does it provide us with useful cognitive tools? In an attempt to answer this question, let us refer to the work of the second of the proponents of ANT, Michel Callon.

Callon's article "Society in the Making: The Study of Technology as a Tool for Sociological Analysis" (1987) concerns an unsuccessful attempt by the engineers of *Électricité de France* (EDF) to revolutionise the automotive market in France in the 1970's by introducing electric cars on a mass scale. Callon's elaboration focuses on contrasting tacit sociology, which is casually practised by engineers involved on both sides of the argument about the attempts to implement electric cars, with texts by professional sociologists—Pierre Bourdieu and Alain Touraine. As Callon wrote:

In fact, sociologists were little concerned with the EDF adventure and abstained from establishing some link between their theories and this astonishing story that was unfolding before their eyes. A story so much the more astonishing because, as we will see, the engineers at EDF were to become rapidly engaged in a controversy in which their Tourainian sociology would set itself against the sociology à la Bourdieu employed by the engineers at Renault (…)
EDF's engineers did not have to defend their ideas in an academic arena. Any brilliance or originality in the analysis they developed was of little import. For them the analysis was a question of life and death because the economic future of their project was at stake. No more sophisticated arguments and theorizing! What mattered was to be right: to be able to prove by the very success of their innovation that French society was evolving in the way they claimed it was (…). The rest was of no account. In short, if an engineer-sociologist is to be proved right he or she has to create a new market; success is measured by the amount of profit gained. This, in all its simplicity and toughness, is the test of truth. (Callon 1987: 89–90)

Callon presents engineers realising their concepts and in this way makes them 'real' as a pattern for sociologists to follow (which, due to the failure of the EDF project, is a rather unconvincing pattern). Underlying this is the apparent law of the survival of the fittest as the best test for truth (or rather what is offered by ANT in the place of truth). The role of the natural environment, to which the ideas must adapt or perish, is played by the market: not an idealised free market but one that actually exists in a particular place and time. This is quite a controversial idea but, despite appearances, it is not the essence of ANT's attitude to sociology.

It is rather an attempt to disclaim the whole pre-existing sociological legacy as represented by Durkheim: 'As well as individuals, there are things which are integrating elements in society. It is merely true that individuals are the only active elements in it' (Durkheim 2013: 16). For its proponents, the theoretical distinctiveness of ANT lies in the way it regards non-human elements as equal to humans in terms of their suitability as subjects for analysis. The key to ANT methodology remains the directive of 'following the actors,' that is of mapping what resources (technological, natural, narrative, human or social) have been included in the network, how they have been transformed.

For sociology, the effect of such a decision is devastating: it becomes reduced to a permanent ethnography of the social world, to be performed using an unfamiliar language. At the outset, in their efforts to reform sociology, proponents of ANT limit the available altitude and clip its wings, reducing it to a tedious idiography with no hope even of theoretical synthesis.

The problems do not end here, however. Assuming that, like detectives, we have followed the actors, shortlisted them, described their transformations, elaborated on their connections. At least since the times of Max Weber we have known that the world is extremely rich and any kind of analysis of it involves dividing the stimuli into information and noise. How can the detective be certain that he or she has followed all actors, identified the key moments, the crucial transformations, if ANT fails to offer intersubjective criteria? What makes his or her account better than one which could be produced by another detective who started at the same point but assumed a different pattern of selection and reduction of complexity?

Callon is aware of these problems but the solutions he offers are unsatisfactory. In his approach, the researcher makes discretionary decisions in which he or she is not limited by anything apart from the necessity to take into consideration both what is social and human, as well as what is natural and technical. The researcher's task is to depend on the selection of such a repertoire of factors which will enable the elaboration of a narrative that he or she will find satisfactory and which other members of the research community will find convincing (Callon 1986). From this perspective, ANT appears to be a mere idiographic specification of a somewhat forgotten postulate by Paul Feyerabend, according to which *anything goes* (Feyerabend 1975).

However, what if there are two competitive 'network' narrations on the same subject? The problem is even more difficult when we try to examine why something has been successful or ended in failure, rather than simply why it happened.

Let us consider the example of Aramis, the history of which is told by Latour (2002). It was a project developed in France, lasting from the end of 1960's to the late 1980's, and which did not proceed beyond the experimental phase. In brief, it was a transport system based around the idea that individual (and specific) 'cars' of the inhabitants of suburban areas would be joined at special stations to form a kind of a city rail system (in which every 'car' would be a separate carriage). Next, this train would go along the rails and into the centre of the city. It was assumed that Aramis would reduce traffic in the city centre, facilitating commuting to work and eliminating the problem of traffic jams. At the same time, it would also retain some of the advantages the car as a means of transport (e.g. flexibility in leaving and returning home). Yet, in spite of the visible potential, along with promising results in initial trials, Aramis was not implemented. The reasons for its failure are interesting with respect to ANT.

Keeping to the convention of the detective story, the French researcher presents the events from several different perspectives. Although we may get the impression that no actor has been left out, this does not get us any closer to answering the question 'Who killed Aramis?' This is because in many other narratives in the ANT convention, apparently similar factors contributed to the (cognitive, engineering, social) success, and not to the failure.

Olga Amsterdamska in her review of *Science in Action* (Amsterdamska 1990: 500) observes that Latour's approach—and in our view, the approach of ANT as a whole— 'shares its foolproof character with a number of other social theories, and if that were its only flaw, perhaps it could still be of service in describing and interpreting science.' ANT's flaws do not finish here, however.

ANT favours 'narratives of the winners.' Hans Radder (1996: 111–112) notes that this tendency results not from methodology but from the very ontological assumptions of ANT and the accepted vision of the process of change. ANT requires some of the actors to undergo stabilization while others are annihilated through cutting the connections which have kept them alive. Strong networks are also capable of redefining weaker actors situated within their scope of influence. Theoretically, there are winners and losers in the socio-technological transformations, but if we do not witness the process of 'pulling the rope,' then in practice when the time passes we are not able to say anything about the losers apart from what we learn from the entities which were successful in the process. In other words, on the basis of the assumptions of ANT, the notion of 'loser-actor' should be treated as an oxymoron. Radder also notes that the discovery of the course of events is made more difficult by the fact that it is assumed in ANT that the scientist should be tracked rather than asked difficult questions.

Should we accept the coexistence of competitive narratives or should we instead aim for a rapid unification of the diverging world? Perhaps we should wait for the winner to emerge and then take his/her side? This issue has been interestingly addressed by John Law, the third of the proponents of ANT. Unlike Callon and Latour, he does not attempt to deal with the problem by sweeping it under the carpet: his whole book *After Method* (2004) is devoted to it.

The Afterlife of Scientific Method

It is difficult to interpret the title of Law's book other than as an attempt to announce the end of methodology. Yet is science possible after 'the death of the method'? Law maintains that it is. Let us look into what conclusions he draws, starting with ANT.

The directive to follow the actors results in very long and complicated reconstructions, referred to by Law as a *mess*. Instead of rejecting ANT for its messiness, he suggests that we accept the fruits it brings because perhaps it is they that best reflect reality, as opposed to the simplified, distorted, souped-up pictures offered to us by science as practised in the positivistic approach.

In the mess, the issue is not only that the reconstructed networks are long and that they contain heterogeneous resources, but also the lack of singularity of the descriptions of the world. How are we able to accept the coexistence of parallel descriptions which do not 'hold water' and which are sometimes mutually exclusive? This seems to contradict the way all the sciences function. However, it turns out that agreeing to a certain lack of singularity is not as absurd as it might appear at first glance. Here, Law refers to an important work within the field of STS: *The Body Multiple* by Annemarie Mol (2002).

In her study, Mol focuses on atherosclerosis of lower limbs. This disease has been known for a long time and has been thoroughly studied and well-described; there are a variety of standard methods for diagnosing and treating it. However, as we move between different disciplines, hospitals, and even wards of the same health centre, there emerge certain significant discrepancies: the diagnoses of particular cases are not consistent with one another; explanations and visions of the same disease presented by different specialists turn out to be disproportionate; the therapies adopt dissimilar approaches.

As a consequence, there are created, as Mol puts it, different ontological versions of the illness, and the question here is not at all different types in the sense of the medical classification of illnesses or the specificity of particular examples. As a result of the variation in medical practices, atherosclerosis turns out to be something slightly different in different areas. The same patient may be diagnosed in a completely different way in accordance with different approaches. Different modes of examining atherosclerosis result in different, mutually exclusive therapies. In spite of this, doctors are able to connect all the versions once again into a relatively coherent whole, although most often this assumes the isolation of various specialisations in order to prevent conflicts.

Why instantly refer to this as ontological splitting when it is possible to reduce the problem to an issue of difference in perspective: different medical specialisations and different practitioners approach a phenomenon in different ways, hence it is no surprise that they see something slightly different even when these are only aspects of the same thing. However, in the case of medical phenomena like atherosclerosis (but also hypoglycaemia (Mol and Law 2004), alcoholism (Law 2004: 70–81), anaemia (Mol and Law 1994)), it does not work like this. Different research procedures not only provide various perspectives on the same phenomenon, but actually constitute the object of the research differently. For example, people diagnosed using one of the procedures, while applying another one, may be considered healthy or ill but with a completely different condition. Such situations go beyond the approach of perspectivism. We may accept that representations are faulty but this is only a partial solution since it is impossible to diagnose an illness within the domain of medical diagnostics without using a method.

Doctors could harmonise the world, for example, by establishing a golden diagnostic standard. In medicine, however, truth and singularity are not the primary values: human life and health are at stake here. In medicine, to have more representations is better, even at the expense of coherence.

Ontological harmonisation occurs in practically all the natural sciences covered by SSK and STS analyses. This is the subject matter of the scientific controversies which were investigated by the School of Bath. A researcher obtains results incompatible with the theoretical expectations; competing teams obtain divergent results; two prominent theories prove impossible to reconcile: in each of these cases something or somebody will have to be sacrificed on the altar of science.

As was shown by Randall Collins (1994), the success of the areas of natural sciences which may be called research fronts can be reduced to the rapidity with which they finish arguments. The category of the black box is essential here (Latour 1987: 2–17). A black box is any combination of heterogeneous factors (i.e. a network) which has been stabilised to such a degree that it can serve as an unproblematic component of another network. It is

a system whose inner mechanisms one does not need to understand in order to be able to use it. Key to this stabilisation, is the incorporation of black boxes into other networks: as a result of the accumulation of elements, a given black box becomes 'unquestionable' not due to its internal qualities but due to the significance it assumes through its connections with the other elements in the networks.

Let us now return to Law's argument. He argues that in science the alternative paths of development (of knowledge, technology, society) are shut down too soon and there are new 'realities that might be made too soon' (Law 2004: 117–118); the black boxes are closed too early and they will almost instantly be used as building blocks in other constructions. Instead of reducing, we could—as Law suggests—choose ontological pluralism. Since the world is a mess, then perhaps both its descriptions and research methodology should be less orderly. Law postulates not only nonstandard ways of expressing the richness of the world (Law 2004) but also conscious practising of ontological politics (cf. Mol 1999): since different decisions of the researcher result in different worlds, perhaps while making them we should explicitly be directed by moral, ideological or political considerations. In this perspective, the researcher's task is to make sure that certain versions of reality are made more realistic (that is 'more stable thanks to better networking').

Law's proposal may be criticised in two ways: from the outside and inside. Let us begin with the external criticism, and more precisely, with a question which it would not be possible to pose in Law's language: Is it the world that is disorderly, or rather is it ANT itself? The postulate of following the actors and taking into account social, technological and natural beings in analyses by no means implies that we are condemned to a holism whose result is the mess in the layer of description. After all, we are able to accept certain criteria of distinguishing noise from information, and the world itself does not have an impact on this.

In the light of *After Method*, sociology is not to be an active science which intervenes in reality and rebuilds it not only in the narrative mode (here singularity and order are imposed onto the world). It is also not to be oriented towards the things which are repeatable in the world, like a kind of astronomy of socio-technology, in order to be able to offer accurate predictions (in this model, the singularity and order belonging to the world). According to Law's proposal, sociology is to become a specific type of pluralistic history.

From the outside, Law's entire narrative looks like an attempt to save ANT. ANT was supposed to lead to a methodological breakthrough in the social sciences but what has been obtained in no way resembles a scientific revolution: there is a kind of sociology inundated with innumerable case studies and systemically deprived of a view that would enable any generalisations to be made. However, instead of preventing the avalanching monograph of everything, Law legitimises these activities claiming that this is the way the social sciences should be practised.

Criticism of such a hermetic concept coming from the outside will certainly lead to deadlock: Law will always be able to counter that any given argument is derived from a lack of understanding of ANT. Asked about the relation between the world and its description(s), he may close the discussion by stating that on the grounds of ANT, the problem does not make sense. Let us now change tactics and try to dismantle Law's concept from the inside, in line with its own principles.

From this perspective it transpires that Law is highly inconsistent. He proposes that paths not be closed too early and that the constructs not be turned too hastily into black boxes. But doesn't Law actually accept singularity in the political and normative layer? The best demonstration of this is the choice of mess as the key category of his post-methodology. Where does the conclusion that the world is disorderly come from? Other perspectives are clearly possible, even fully traditional ones. Law may be avoiding singularity in ontology but he makes resolutions about other points, even at the level of politics, deciding which worlds are worth making more stable.

It does not matter whether we agree with the values or opinions on science which Law adopts. The problem is wider and has been known for a long time: it has been identified by epistemologists, philosophers of language and traditional sociologists of knowledge. Returning to the foundations of our knowledge or language, we do not finally reach any basis in the form of facts or axioms of the scientific method that are obvious for all but arbitrary decisions and assumptions. We are able to provide definitions of almost all notions but the basic notions in their essence have to remain indefinable: otherwise we fall into a vicious circle. We have to agree on certain rules of playing science and conducting scientific research, otherwise we face a deadlock that is impossible to overcome. Even the definitions of what is scientific as normative judgements have to be unscientific. Regardless of how we try to put the elements together to form a whole, arbitrariness, inconsistency or contradictions are going to appear somewhere. This is one of the fundamental lessons of sociology: to some degree everything relies on consensus, often a forced one. And SSK, especially the School of Bath, has shown many times how paradoxes that are theoretically impossible to overcome have been resolved in practice (Collins 1985; Pinch 1986).

ANT is particularly inconsistent in the way it presents science and how it postulates its practice. Within this concept, while creating facts, scientists construct black boxes connecting various elements: human and non-human. In the case of controversies, they inundate their opponents not only with words and texts but also with data generated by research instruments, working prototypes. In both situations the key role is played by heterogeneity. However, the proponents of ANT build homogenous networks with a slight share of non-human elements. Although ANT attempted to develop new techniques (co-word analysis, various methods of innovation visualisation), it has ultimately limited itself to seductive narratives.

Let us go further, however. Science in action is surprisingly close to political negotiations (Latour 1983), but its effectiveness requires that the scientists remain unaware of it and problematize neither the objects of science nor the tools and methods which they use for a large part of the research process. In other words, skilful proper closing of black boxes consists in the scientific community's denial of the knowledge of the construction process. However, Law, Latour and Callon, postulating consistent reflexivity in the spirit of *After Method* on the one hand, and on the other hand, opting for explanation to be regarded as the survival of the strongest networks, paralyse the research process. Building long, strong networks which will make ANT a more attractive theory or methodology than the competitive approaches involves the skilful proper closing of black boxes. How can this be achieved while remaining conscious of consent, discretion, etc?

There is yet another problem which this time would be considered significant by the representatives of ANT themselves. ANT encourages one to undertake detective work consisting in following actors. It also assumes the conscious practice of science-as-politics. However, both politics and detective work involve making decisions. A politician has to frequently choose between excluding options and a detective has to collect evidence for court proceedings based on which certain actors will be considered as perpetrators and others will not. Perceiving researchers within the ANT current as detectives is a mystification since ANT narratives most often lack conclusiveness: we are not offered agents—a network of causative factors—but a dispersed cloud of agency. The quantum revolution in physics also depended on rejecting the vision of atoms as 'balls' and replacing them with clouds of possible states. The difference between clouds in physics and in ANT is still fundamental: physicists determine probability, make predictions based on their models, and the cloud maps of atoms allow for modelling structures like proteins, whereas in ANT there is nothing analogical. Indeed, some narratives within the current of ANT are characterised by a high degree of conclusiveness. However, this is usually a result of the fact that they are the narratives of winners. ANT analyses do not offer certainty—they try to derive it from external sources instead. Generally, one may get the impression that a work within ANT resembles efforts by a politician, though not one who has to make definite and binding decisions for which they may be held accountable, but rather one who prevaricates, who does not want to confront anybody, and when forced to decide, struggles to minimise responsibility.

If we take all of these problems into consideration, it is no surprise that Latour and Callon preferred to refrain from writing about those areas which were explored by Law in *After Method*. This is a rather trivial explanation. What is not trivial, though, is to state that what matters is not whether a system is marked with paradoxes but how they are managed.

Conclusions: ANT with a Positivistic Inclination

We are not endangered of oversimplification by stating that ANT offers theoretical maximalism accompanied with methodological minimalism. On the level of ontology everything is radical here: relativism, reflexivity and symmetry. As long as we limit ourselves to descriptions that works really well. Yet that radical theory limits methodological scope to a barely widened anthropological perspective. Storytelling provides deceptively comforting feeling of understanding but it doesn't explain anything. The latter needs regularities to be uncovered, patterns identified, mechanisms exposed and laws applied.

ANT tried different approaches to explanation which involved suppression and equalizing the concept of explanation with that of narration or story (Latour 1988b). On the other hand, even ANT analysis showed that storytelling may be the form of explanation but it is inevitably a weak one; especially when compared to explanations that involve more than just talking and writing.

What to do with ANT then? We suggest three possible paths. Two of them are rather radical. The first one requires full acceptance, and consequently the attempt to develop the ANT community even further. That should result in ANT becoming a stable construct and

a sort of all-encompassing theoretical framework. As we argued this must result in lowbrow methodology for sociology. The second path involves the total rejection of ANT which is viewed as a harmful intellectual fad.

Undoubtedly, the above mentioned options are not satisfactory. Using sociology of knowledge approach one could argue that ANT had to abandon sociology to achieve a radical redefinition of our thinking about society. But there comes the time for comeback. The problem is—as we argued—that contemporary ANT could only increase methodological problems for sociology and social sciences in general. Thus the third path involves the end of ANT by incorporation back into sociology. We do not know whether proponents of ANT are ready for this. A much more important question for us, however, would be: are sociologists ready to reconstruct sociology so that it could incorporate at least some of ANT's findings? In other words, it is not about (ab)using ANT as loose inspiration, reservoir of fresh metaphors or new sensitizing vocabulary. It appears that the aim is to build a sort of synthetic sociology.

We may check what could happen if we started with ANT findings on the functioning of science but made slightly different decisions along the way. What could happen if ANT treated seriously the category of explanation in science? Would it be possible to practise ANT with a positivistic inclination? What is meant here is an attempt to conduct research assuming the network and hybrid ontology proposed by ANT but retaining the intention to formulate conclusions, predictions, testable models, etc.

ANT has reconstructed the patterns of the natural sciences and then used them in designing its own 'method.' However, there is no work on methodology within the domain of ANT in which the focus would be placed on the issue that is key to the functioning of the natural sciences in the view of ANT itself: replicability. How can we, as scientists, know that something is an experimental fact and not a mere artefact of a procedure? Because the same phenomenon is observed in different contexts independently of the researchers' convictions, their personalities, individual preferences for modifications to research procedures, devices used, samples, etc.—in brief, because the experimental effect may be replicated in various laboratories. The replicability of generated effects is also characteristic of the black box. Perhaps if proponents of ANT had not focused so much on individual cases, they would not have overlooked this issue. The paradox is that it is thanks to ANT researchers, among others, that we have learned exactly how replicability in science is obtained and what its price is.

As has been proven by STS, replicability in science involves the engagement of considerable resources, the best example of which are problems with replicating the more innovative results of scientific and engineering works. The best way to obtain replicability often involves turning the laboratory experiment into a hermetically closed device which would produce the same results out of the laboratory, or making the world resemble the laboratory (Latour 1983). Interestingly, the results obtained by scientists happen to be more stable than the theories by means of which the results are explained: the physical or chemical effect often precedes the theoretical explication, the discovery of the effect forces a revision of the theory, or the explanation of results changes but there is no doubt that if several standard elements are skilfully connected, a replicable result will be obtained, even if we fail to comprehend the science which underlies it.

What a sociology that realizes the patterns of natural sciences could look like? In brief, it could be a sociology based on synthetic methodology that pursues an understanding of the world through the building of artificial systems imitating or triggering social phenomena. In such a perspective, the task for sociologists would be to create closed techno-social systems which would produce predictable effects, for example group behaviours. There is no need to go that far, however. The positive version of ANT would assume, for example, a different application of the ethnographic approach. Instead of conducting parallel unconnected studies of individual cases, the realization of comparative research or the approach of multi-sited ethnography could be considered. There have been such proposals (Beaulieu et al. 2007; Geels 2007; Hine 2007; Rappert 2007; Wyatt and Balmer 2007). Yet it remains difficult to carry out such projects if there is a lack of a coherent vocabulary. Such research would have to abandon the concept of consistently following the actor. Instead, it would be necessary to narrow the possible entities taken into account in the studies.

We should also examine what hinders the analysis of human and non-human factors that are carried out using standard sociological tools. What prevents us from constructing and improving the battery of indicators which would allow us to describe the world as it is assumed to be viewed in ANT? As we cannot see any impediments other than historical ones for SSK to absorb the findings of the sociology of science on the Matthew effect, we cannot see any problem for ANT to try to use the results of research into 'preferential attachment' in the analysis of (techno-)social reality. The obstacle is at most a historically developed attachment to a set of tools, borrowed from anthropology and the history of science, which has been legitimised.

The constructivist vision of cognition, present in ANT, is no impediment to conducting research using standard sociological instruments. The fact that something is constructed does not mean that it does not exist, does not have an influence and should be treated with indulgence. We may go even further: long before ANT, sociology had been investigating non-human factors while at the same time not dispensing entirely with standard methodology. Such investigation took the form, for example, of social ecology interested in physical space, or in Erving Goffman's approach, drawing attention to the material elements of the façade of the social spectacle, or in the analyses of aesthetic preferences by Pierre Bourdieu, for whom objects have become indicators of social distinctions. In each of these cases sociology is wilfully and incessantly interested in the material world. Let us determine the boundaries of sociology even more freely so that it also encompasses work within 'action research' initiated by, among others, Kurt Lewin (1943), studies in the anthropology of urban areas of William H. Whyte (1968, 1980), the scholarship of British industrial sociologists organized around the Tavistock Institute, Norwegian researchers developing the concept of industrial democracy (see e.g. Emery and Thorsrud 1976; Johansen 1978; cf. Lezaun 2011) or the research produced by social scientists such as Lucy Suchman (1987) and Julian E. Orr (1996) in the field of information technology. Having taken such a point of reference, it will prove that ANT has become stuck in its attempt to convince its audience that things are significant, while the above researchers have not only described them but also undertaken engineering interventions resulting in concrete changes to the networks of human and non-human actors. Hence, according to the criterion proposed by Callon, they have been more effective scientists than

the proponents of ANT: they brought ideas into reality, rather than simply writing about them.

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