



Elaborating the notion of performativity

Michel Callon

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BTP pour le viaduc de Millau et le contrat d'entreprise passé par un particulier avec une entreprise de nettoyage ?

Anni Borzeix : Si la question avait été : qu'est-ce qu'un service pour vous ?

Augustin Aynes : J'ai du mal à vous répondre, parce que cette notion n'existe pas en droit. Quand on dit que le droit de la concurrence s'applique à la production, à la distribution et aux services, on n'a pas de définition claire des trois éléments. On veut juste dire que le texte s'applique à toute l'activité économique ■

Notes prises par **Hervé Dumez**
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Au printemps 2006, Michel Callon était intervenu au séminaire AEGIS sur la question de la performativité de l'économie. Le compte rendu du séminaire a paru dans *le Libellio* n°3 de juin 2006. Dans cette interview accordée à des chercheurs brésiliens, Michel Callon revient sur la question en précisant certains points et en resituant cette approche dans la ligne de ses travaux antérieurs.

Elaborating the notion of performativity

Question : *In your paper called 'What does it mean to say that economics is performative' you state that 'both natural and life sciences, along with the social sciences contribute towards enacting the realities that they describe'. And then here in ESOCITE¹ on your lecture you spoke about the carbon market as an example of economical, environmental, technical and social heterogeneous artifact experimentation. In saying so, what would be the role of the social sciences as makers of some kind of artifacts and the social world itself?*

There are several ways of describing the main theoretical and epistemological achievements of science and technology studies (STS). We can say, for example, that STS have made it possible to conceive of a third way between realism and relativism : science and techniques "explicitate" reality by constructing it and construct reality by "explicitating" it. This process of explanation maintains both the existence of a reality that resists, that doesn't do just anything, and the idea that this reality, engaged in various trials, can resist in various different ways ; in short, it is multiple, ambiguous and, why not, constructed or instituted, instated. To use Austin's vocabulary, one could also say that scientific statements – to take only them – are performative. Above all, this assertion would shield us from the temptation to contend that they are constative, that they try to describe and analyse a reality on which they will not intervene.

The notion of performativity, as presented by Austin, has been criticized, first of all by Austin himself ! Of course it can be taken to mean that language creates the world from scratch, somewhat like the 'Fiat Lux' of the Old Testament. But those who adopt the repertoire of performativity are careful not to stick to this meaning. The STS have completed and enriched the concept, by showing that the signification and effectiveness of scientific statements cannot be dissociated from the socio-technical arrangements or *agencements* involved in the production of the facts that those same statements refer to. This concerns the heterogeneous material and textual nature of scientific practices. Statements are entangled with technical devices, incorporated competencies, rules of thumb, rules and procedures. With the facts that they

1. Michel Callon gave the inaugural speech (The contribution of SSSTS to the renewing of the conceptions of the social: the case of economic markets) at VII Esocite — Jornadas Latino-Americanas de Estudos Sociais das Ciências e das Tecnologias, on 28th, 29th and 30th May 2008 in Rio de Janeiro (Brazil).

describe, they are embedded in the *agencements* in which they are stakeholders. There aren't materialities on the one hand and text on the other. Statements contribute to giving meaning to the events that the *agencements* produce, and these events support what the statements say. To put it more philosophically, and following Bruno Latour (1999), statements are indissoluble from all the devices that cause the entities they talk about to exist, actually to act. As Latour has shown, this point of view is both realistic and constructivist : realistic because it isn't possible to make just anything exist (in the expression 'Fiat Lux', for the light to exist, it had to be able to exist !) ; and constructivist because for any one entity there are thousands of ways of existing, of being detached from the surrounding plasma : existing is acting. What science and techniques manage to do – and this is the project that gives them their strength and power of truth and effectiveness – is to make entities act in a controlled and predictable way. Presented like this, these entities can be analysed and become the object of knowledge. With the 'representing and intervening' twosome, Hacking (1983) captured this dual dimension of scientific practices perfectly.

Strangely enough, STS have shown an interest in the social sciences only episodically and marginally. This may seem surprising at first, since the social sciences appear to be easier to deconstruct than the natural sciences. But I think that there's another reason for this reluctance : it stems from the social sciences' uncertainty about their own scientificity. Take the case of economics. If you questioned sociologists, anthropologists and science studies scholars, you'd find that they have an irrepressible tendency to consider that neo-classical theory – to mention only one – especially when it is highly mathematized, is simply eyewash, closer to ideology than to science. For them, studying economics and its effects amounts to nothing more or less than studying a particular belief ! They think that if economists and economics have an influence, it is because they are able to make people believe that what they say is true. People therefore behave as if it were true ; they're alienated, and it's because of that alienation that what economics says is true. Economics is a vast self-fulfilling prophecy, from every point of view comparable to religion. It is the new opiate of the masses !

I find this type of attitude incomprehensible. Why make an exception and consider that in this particular case truth is defined by the correspondence between a discourse and a reality outside of that discourse ? Economics, like the other sciences, serves to represent. But to account for what it calls the economy, it has to contribute in one way or another to the constitution of the object that it is accounting for – like any other scientific discipline. In other words, it has to find in the world it is studying – and which, as I have shown, can be reduced to a simple world of paper ! – the elements that will enable it to define it, to act on it, and to account for it. I thought that to describe this approach, the notion of performativity could be useful. To avoid any misinterpretation, a mistake so easy to make (some people have thought that I was saying that the economy was created entirely by economics !), I finally opted for the notion of performance. This term underscores the fact that there are no effects of knowledge without well-designed interventions, and that it is these interventions, with the events that they produce and that they enable us to describe, which are at the origin of the production of facts. I moreover recently added that this activity of performance is always caught up in collective activities which don't mobilize only professional economists. That's why I spoke of co-performance.

Contrary to what some suggest, this dual terminological shift (from performativity to performance and then to co-performance) does not weaken and dilute the

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analysis. I haven't digressed since *The Laws of the Markets*, in which I emphasized the material devices and role of economic agents. The essence of the message presented in that introduction has been maintained and reinforced. It can be summed up in three points. First, the economy does not exist as an economy before the elaboration and implementation of the knowledge, statements and representations which cause it to exist as an object of both knowledge and intervention, in short, as an economy. Second, this knowledge and these statements and representations are largely the result of the relentless and competent work of professional economists, irrespective of their theoretical convictions. But they are not the only ones to be engaged in this elaboration ; one has to add all those who work in academic disciplines related to economics, like management science, and all those who are in the field – computer scientists, market professionals, social movements etc. – who develop increasingly formalized, systematic and abstract knowledge and competencies, as well as technical devices, and who give the economy its identity and robustness. I have proposed the notion of economics at large to denote this vast and heterogeneous population engaged in reflection, conceptual elaboration, and socio-technical design of the economy in all its forms. Third, the economy thus constituted is composed of technical elements, incorporated competencies, rules and ... sets of theories, models and statements : in other words, what we call the economy consists of *agencements* that qualify themselves as economic.

The economy, as a name denoting an object, points towards *agencements* which can be qualified as economic because economics is a stakeholder in them, in one form or another. The notion of co-performation stresses, even more than that of performativity, the fact that there is no economy without economics ! This applies as much to modern economies as to past or exotic economies.

The objection has often been made that markets existed well before economics started to talk about them, and that they will carry on existing even if economics no longer talks about them. I understand this argument but it's beside the point that I am making. This point is both difficult and obvious. Perhaps the notion of plasma used by Bruno Latour could help to make it clearer. Bruno tells us that the world obviously existed before anyone spoke about it, analysed it or experienced it through practices that introduced a certain order into it. This is as true of the so-called natural world as it is of the so-called social world. It's true of our contemporary world, and of the past world that we try to rediscover by following the traces that it left behind, for example in archives, memoirs or myths. This world, that can be compared to plasma, lives its life ; the numerous forces comprising and organizing it make it evolve. One day comes economics. Concretely : one day come Aristotle and Xenophon. Exploiting accumulated experience, existing discourses, and notions patiently built up, they reveal in this plasma, in this very real but not yet economized world, the main themes, discontinuities, gradients of resistance, divides and interstices that they play with and compose. They think they see the forms of what they call *oikonomia*. They build statements, give examples, draw conclusions and make recommendations. The quality of this work of explicitation is measured by its capacity to convince, that is, to define the right themes and to play with them in the right way. That's the stroke of genius. The explicitation has been successful. The economy starts to exist as a distinct object, because Aristotle and Xenophon knew how to divide up, reassemble and cluster the plasma surrounding them. Of course there were forces, entities, organized matter from which the work of economics, carried out intelligently and pragmatically, was able to produce entities. But the economy is born as an economy, by the grace of these well-adjusted discourses.

Afterwards, it's another story, that of performance : the object and its discourse are bound together for better or for worse. Their histories become indissoluble.

I think that we've passed a stage in this evolution. Economics has spread, diversified ; it is taken in charge by a multitude of groups that intervene in contradictory and divergent ways, but that all want to ensure the existence, in a lucid, organized and reflexive way, of the economies that they see as desirable and possible. I can therefore say, without being eccentric, that the market did not exist, but that there were things, entities, and systems of forces which lent themselves to this gentle violence of Aristotle and Xenophon and then of all those who have called themselves economists and have advised princes and presidents ; and that, thus explicated, these sets of entities started to think (or to be thought), and to act and exist differently. They became what they were not yet, strictly speaking : markets. This history was so successful that now everything is market, whereas previously nothing was ! Markets have been conquered, like the polders from the sea, both by searching history and the earliest societies, and by colonizing modern societies. Markets are not comparable to gold nuggets, already there, that the economist-gold washer, with talent and persistence, separates from the gangue in which they are hidden.

This leads me to challenge the notions that are usually used to describe the relations between economics and the economy : those of prescription, of self-fulfilling prophecy, and of applications. I've explained all that in Callon (2007).

The reality and pervasiveness of this collective activity of co-performance now seem to be acknowledged, even if they are not formulated in the terms that I use. This growing awareness is evident in the case of financial markets that resemble huge socio-technical artefacts, and whose regulation, as we are currently seeing, poses enormous problems to their designers, including a wide diversity of professional groups. It is also apparent in studies called market design. If some of the best economists felt the need to invent this notion, it was because they fully understood that the notion of application was misleading, as it is for describing and understanding classical technological innovations. Could one say that a car, a computer, a spaceship or an I-phone are simply applications of disciplines and of constituted scientific knowledge ? You have to be naïve, like Colander (2008) for example, to dare to say something of the sort ! Once you've recognized this design activity, the question is to know how it is organized, who participates in the design of markets, how experiments are conducted, and how the results are evaluated. My aim is to ensure that this design is as open and democratic as possible.

Question : Is it possible to understand national scientific communities as networks of socio-technical experimentation ? If it is so, what are the differences between these networks in developed and underdeveloped countries? Which are the consequences of these differences to the socio-technical experimentations ?

There is no general answer to this question, but we can clarify things by introducing the notion of objectification of economic activity, proposed by Tim Mitchell.

The question of autonomization of economic activities, that is, the existence of a sphere of institutions or social sub-systems that one can qualify as economic, was hotly debated for a long time and still is ! From this point of view, the controversy between formalists and substantivists in the 1960s, which has never been closed satisfactorily, is illuminating.

Some people believed that formalists and substantivists differed on almost everything, especially on the implicit or explicit definition of the economy, on what

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behaviours qualified as economic are, or on relations between individual action and social structures. Actually, apart from these disagreements, I think that they agreed on the essential : a) first, on the fact that it is possible to talk in general of the economy or of economic behaviours, irrespective of the society under consideration, as these notions have a near-universal signification ; b) second, on the fact that the economy as an autonomous sphere did not start to exist only with modern Western societies. Since this famous controversy, the terms of the debate have of course changed : sociologists of embeddedness, the heirs of the substantivists but who departed from them on this point, deny the existence of an autonomous economic sphere and of purely economic behaviours, even and especially in contemporary societies. Economists, on the other hand, have highlighted the importance of institutions, but mostly continue to support the idea that this does not preclude the existence of activities which are typically and specifically economic. Finally, some sociologists like Bourdieu or Fligstein, through the notion of field, posit the autonomy of the economy but equate it, from an analytical point of view, to any other social activity. Compared to the content of debates between formalists and substantivists, these variations are superficial. I have shown, with Koray Caliskan (forthcoming), that both believe in something that can be called the economy, which exists everywhere but in singular and variable concrete forms. So disagreements are not about the existence of the economy but on how it is defined. To simplify, there are those who set the economy in a form of individual (instrumental) rationality, and those who make it a constituent property of all human societies since their members are all confronted with a demand for subsistence (the meaning and scope of this notion, and the modalities of organization of the corresponding activities, obviously depend on the society in question).

With the debate formulated in these terms, it is highly unlikely that we reach an agreement since every discipline believes in the economy but defines it in its own way, which is normal. To get away from this controversy, which was productive but is now sterile, I think we need to move on from the study of the economy to that of processes of economization. The question is not : what do we call economic behaviour, or what is the economy, but how are behaviours, institutions, *agencements* and rules of the game economized ? This new formulation leads to that of objectification of the economy in the form of an independent reality that becomes 'The Economy'. So, the question : what are the conditions of the constitution of 'economic' entities that are endowed with some degree of autonomy, are organized objectively, have interdependent elements, and on which it is possible to act ? In short : how does 'The Economy' appear ?

The notion of objectification leads to that of the role of national frameworks. Tim Mitchell has suggested, in my opinion very convincingly, that the first form of objectification of the economy, which causes one to talk of 'The Economy' as an entity that holds together, on which one can act as on a well-defined object with its internal regulations, is the national economy. If we follow Mitchell, the first historical form of objectification of the economic sphere is that of national economies. This objectification has been prepared for a long time. The physiocrats, for example, contributed to it. But national economies as an objectified form of 'The Economy' reach maturity with the establishment of welfare states and the institution of macro-economic policies based on the idea of national economic systems, endowed with their own rules of functioning, regulation and equilibrium. The different versions of Keynesianism constitute the theoretical counterpart of this objectification. This first form of objectification, whose pre-eminence culminated in the late 1960s and early 1970s, was followed by a second form that I believe will in

turn prevail. National economies take second place and disappear behind markets which, designed to be transnational, are henceforth considered as the natural form of 'The Economy', the one which enables individuals to behave rationally and in so doing to participate willy-nilly in collective progress. The market is a reality that suffices unto itself, and that circumscribes a territory in which the economy is expressed and exists. Here micro-economics takes pride of place ; it explains how markets ought to be designed to be efficient. With this second objectification, the first doesn't disappear, far from it, but it shifts into the background. National frameworks seem to be less relevant and sometimes even hindrances to the second objectification.

The second objectification has its fair share of conflicts, but they are different from those of the first. Contradictions between national economies are considered to be backward, and everywhere forces are mobilizing for or against the extension of markets. While decolonization and the assertion of the sovereignty of colonized countries was both a source and a consequence of the first objectification, the search for alternative forms of market organization, as advocated by micro-economics, is associated with the second one. Evidence that the latter does not oust the former, but instead is articulated to it, is provided by the movements struggling for alternative kinds of economies : they rely on national sovereignties and tend to consider national or regional contexts as natural frameworks for experiments which, if satisfactory, will be able to be transposed elsewhere.

Collective experiments that strive to invent new forms of organization of the markets will not result in a uniform model, a one best way. The forms of definition and organization of the economy will vary, precisely in relation to national frameworks, the actors involved and, almost as importantly, objects and issues. Carbon markets will be very different from fishing quota markets because carbon molecules and fish don't allow themselves to be economized in the same way !

Question : How does your work in 'Some elements of a sociology of translations' differ from 'What does it mean to say that economics is performative ?' Are there similarities between the concepts from your earlier works (translations, actor-network) and the latter ones (as performativity and agency)? Does this conceptual change mean ruptures or methodological advance ? And how ?

There is both continuity and change. One way of defining this dual movement is to revert to the distinction between humans and non-humans.

I think that the sociology of translation, or what some (not us !) have called ANT, will remain in the history of the social sciences because it accomplished a decisive step. It (re)opened the question of social theory's treatment of non-humans. Of course, non-humans were never forgotten ; one simply has to reread Marx, Durkheim or Weber to be convinced of that. But, for these three founding fathers – for the Marx of the crystallization of social relations in technology, the Durkheim of elementary forms of religious life who equates totems to mere surfaces on which society is projected, and the Weber of *Economy and Society* who tends to reduce techniques to their instrumental dimension – what the human sciences call non-humans are handicapped beings. Their agency is inferior to that of humans or, to put it bluntly – and this is completely true when it concerns inanimate entities – they are denied all agency. They exist as resources, constraints or objective realities, but they don't act. The sociology of translation, designed to understand the functioning, effectiveness and effects of science and techniques, has shown how mutilating this view is. Those that the social sciences call non-humans (a very negative term, as if one said of blacks that they were non-whites !) do well and truly act, and in

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thousands of different ways. An electron acts differently to a gene, which acts differently to a cell phone, and so on. All are engaged in courses of action that produce differences, alter the state of the world, produce unexpected events, and trigger changes which would not have happened without them. So-called non-humans participate actively in collective action : they influence it, redefine it from the inside, and generate changes of direction and trajectories. Nothing of what the world is or is becoming can be understood if these actants are disregarded. The term actants, borrowed from semiotics, has been very useful for describing the actions of non-humans, as it has the twofold advantage of saying that entities act and of not predicting the modalities of their action. It applies moreover to all acting entities, both human and non-human. I am convinced that this perspective has allowed for considerable progress and has profoundly contributed to reshaping social theory.

An obvious limit of this first wave of studies stems from the fact that, by talking of the symmetrical treatment of humans and non-humans, sociologists have helped to strengthen the idea that they could be distinguished ! In short – and some of our British colleagues have gleefully pointed this out to us – one cannot simultaneously call for equality between humans and non-humans and deny the possibility of distinguishing between them. We were perfectly aware of this little bug, but it bore no weight compared to the number one emergency : making visible and once again problematical the status that the social sciences had granted to non-humans. There was no reason for us to feel responsible for a distinction that had been introduced explicitly and deliberately by social theory to preserve the irreducibly different character of human beings (intentionality, language, capacity to elaborate symbolic structures, etc.). Faced with this ostracism, the first step is the virtually inevitable one of positive discrimination, that is, affirmative action, by taking the distinctions that were made to stigmatize certain populations and showing their arbitrary, problematical and illegitimate nature. Affirmative action always starts by reaffirming a difference, thereby contributing to making it exist ! – but in the hope that it will one day be transcended.

Once this principle of equality had been established, the next step was of course to get rid of the distinction itself. You start with racism first, to show its limits ; and then you get rid of the notion of race !

Several strategies were possible. The paths explored by Bruno Latour, John Law, Anne-Marie Mol and myself differed quite substantially, which is a good thing. Bruno (forthcoming) focused on existence through otherness and engaged in the exploration of what he called modes of existence or regimes of enunciation. John Law and Anne-Marie Mol undertook a programme of experimental ontology. Tracking the complexity of beings' ontological statuses, they introduced crucial notions such as multiplicity, fluidity and 'messes', and in particular studied the methods of the social sciences and their engagement on the actors' side. As for me, I turned to the classical question of agency, first to emphasize the nearly infinite nature of its forms and modalities, whether they are endowed with intentionality, are articulated or not in languages which are extraordinarily varied, are adaptive or interactive, or are limited to automatic behaviours. The extraordinarily poor vocabulary that repeats *ad nauseam* the incommensurability of forms of human and non-human action, by reducing each of them to a caricature, makes way to a much richer repertoire that highlights the multiplicity of configurations which thus become visible and can be counted, studied and analysed, from a point of view that is not only dynamic but also takes into account their interactions and cross-overs. Once this space has been given back to agencies, and without any preconception of

the way in which they are distributed, the question remains of their analysis.

This is where the notion of socio-technical *agencement* that we have borrowed from Deleuze comes in. First, it enables us to build on a set of recent work which has contributed to renewing the study of action. Of course I have in mind the multi-disciplinary research (with which computer science is associated) on distributed or situated action and cognition, the empirical analyses inspired by ANT and, more recently, disabilities studies. To cut a long story short, let's say that with these studies a new view of action and cognition has gained recognition, in which a large number of different entities are mobilized. Each of them participates in the constitution and course of the action, in its own way. All action is collective, and its deployment is starting to be clearly understood. At last we know empirically what piloting, calculating, directing, choosing, making a statement, and so on means. Second, in relation to these studies, the notion of *agencement* contributes an additional idea, also found in Deleuze's and in Foucault's work. The place and the source of action are built into the *agencement*. There is no agency without *agencement*, and no *agencement* without agency. Studying the diversity of agencies means studying the diversity of *agencements* : intentionality, language, will, the capacity to programme, selfish calculation, and altruism are properties of *agencements*. We started for example by studying economic and market *agencements*, but also those that are generous or filled with compassion, and, more generally, (non) calculating *agencements*. In this respect the disabilities studies programme is especially useful, with its notions of prostheses, the right to compensation, and the constitution of personhood.

One of the consequences of this approach is that we jettison the human/non-human distinction. Not only does it become useless, it also constitutes an obstacle. It is necessary to have exceptional circumstances and, to be honest, to have the somewhat complicated brain of certain social theoreticians or philosophers, to imagine that this classification could account for the infinite diversity of *agencements* and *agencies* ! This comment brings me to another point : *agencements* (may) include discourses, texts, theoretical statements, and models. This makes it possible to show how science in general, and social science in particular, participates in the performance of these *agencies* and the divides that they create. For example, the human sciences have contributed substantially to the distinction between human and non-human, and between human agency and non-human agency. It is this successful performance that now has to be deconstructed. The new social sciences have to undo what the old ones did, with as much zeal and effectiveness.

Question : There is no rupture at all ?

Between the sociology of translation of the 1980s and 1990s, and the theory of socio-technical *agencements* of the 2000s, there is no discontinuity, only enrichment and deepening of the analysis. We can for example draw a direct link between translation and *agencement* : *agencements* are operators of translation, and translation is the basic module on which *agencements* are built. Acting means translating, and translating means influencing the capacities and modalities of action, since it means establishing links, connections, circulations, exchanges of properties, and original distributions.

I gave an example of this complementarity in my talk for ESOCITE meeting. The economic model developed by Hardin is the basis for the organization of the fishing quota market. This model was presented in a scientific article in 1968. Thus, if we go by the precepts of ANT and the sociology of translation, it can be analysed as a socio-technical network (even if that means stopping at the relations woven by references). But as I showed in my lecture, this model is a stakeholder, in the sense of

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action and distributed cognition, in the market socio-technical *agencement* that organizes fishing activities, especially in Norway. Without this model, this market *agencement* would not exist and would not act as it does. The same applies to every element of an *agencement*. Every one is an active part within the *agencement* and at the same time an operator of networking with other entities which are translated and participate in the *agencement* but indirectly. If you read Deleuze's definition carefully, you'll see that this ambivalence constitutes his definition of *agencements*. With the notion of *agencement* you have the notion of network or rhizome, but you lose nothing of the substance. The agents are no longer points or nodes, as in the network theory ; they have all the substance, the flesh, depth and unfathomable mystery of *agencements*.

Question : Which are the effects of the dissolution/disintegration of a socio-technical network ? How can we act in an uncertain network, in an uncertain world ? What happens with the performative action when there is some kind of interruption such as a natural disaster ?

The notion of risk isn't satisfactory. It was very useful to highlight the paradoxical effects of the technosciences and a change of regime in the modalities of policy-making. It also contributed to salutary reflection on the nature of politics and, in a sense, helped to gain recognition for the usefulness of the precautionary principle. Unfortunately the notion of risk mixes many things that are worth distinguishing. It is at the origin of a lot of confusion and favours a certain intellectual laziness.

To clarify the questions we need to revert to the famous distinction between risk and uncertainty proposed by Knight, but also to surpass it because it is far too imprecise. Knight distinguishes the notion of uncertainty from that of risk on only one point. In both cases, possible worlds are known and can be described convincingly, but in the former the probabilities of occurrence of each of these worlds is known (especially when they are envisaged as consequences of decisions to take), whether they are objective or subjective, whereas in the latter case the assignment of probabilities is impossible. Now, most of the uncertainties facing us today defy this binary classification. The technosciences produce as much ignorance and as many badly formulated questions waiting for answers that cannot be imagined a priori, as they produce robust knowledge on the states of the world. Rather than chanting that science produces as many bads as goods, it is fairer to say that it produces as many new questions without answers as positive knowledge. From this point of view, the opposition between goods and bads, that I can clearly see constitutes a powerful driver of mobilization, is anything but relevant and useful : everything that is unexpected – to use dedicated terminology – is not necessarily bad, and everything that is explicitly and voluntarily sought after is not necessarily good ! The technosciences are a formidable machine continuously producing matters of concern, that is, problems for which there are no theories or available answers but which can end up improving existing situations, if they are managed well.

These uncertainties are therefore of a different kind to those studied by Knight and by all those who are fascinated by risk. They are linked to problem-framing that reveals vast zones of ignorance of differing degrees of intensity or completeness, but which are presented for what they are : areas of theoretical and practical non-knowledge. In these situations, decision-making procedures are of a new kind. When science and technologies can't serve as a benchmark, a baseline, when they are so embryonic that ignorance is the rule, you don't know what situation you're in nor what's going to happen if you take such-and-such a decision. Yet this is no reason to give up all demands for rationality. On the contrary, the greater the ignorance, the

more urgent it is to act, and the more attentive one has to be to the reasons driving you to act. In situations of uncertainty, the need for rationality is twice as strong.

In situations of radical uncertainty, rationality coincides with the wish for collective experimentation. Since one doesn't know, one has to resolve oneself to organizing evidence that enables one to learn little by little. The main requirement is therefore to organize collective, controlled and debated experiments, to measure what can be done, and to take the necessary measures to do so. The precautionary principle sums up very well how decisions should be taken in situations of radical uncertainty, but it has to be connected to the notion of collective experimentation. In this form of experimentation, one takes small decisions and moves forwards by successive iteration. Gradually one can attain a situation that resembles those described by Knight, but nothing is written in advance, and in any case the usual notion of risk describes only certain situations or trends. All of this can be linked to what I said before on performance : it's by performing that one learns and that one can re-launch action and enrich practical and theoretical knowledge.

Question : How the whole collection of works commonly known as ANT (actor-network theory) handles with the political content of techno-science ? In which of these works the political concern is more evident ? Can the disabled people case studies be an example of this political concern ?

The sociology of translation is known for having established an almost direct link between the technosciences and politics. There was nothing new about this approach. We're used to hearing that everything is political and that science and techniques are no exception. One just has to think of the debates between externalists (who claimed that society could explain the content of science) and internalists (who argued the opposite) in the first half of the twentieth century. But I think I can say that the way in which the sociology of translation has described this link has completely renewed the subject. If science and techniques call for a political debate, it is because they cause a host of new beings to exist, that we have to accept (or reject) and with which we (may) have to learn to live. The issue here is the composition of the collective, and this question is now raised with urgency, for research and innovation have become highly productive. We are therefore forced into a political debate.

This has led us to think a lot about the institutions, procedures and devices that make it possible both to maintain scientific and technical creativity (especially through the constitution and extension of socio-technical networks or innovation networks) and to discuss their organization, the limits to place on them, the configurations to exclude and those to favour. No innovation without representation ! That is the slogan of the sociology of translation when it gives itself a political conscience !

The paths that Bruno Latour and I have followed are different but complementary. Bruno has clarified the operations constituting a politics of networks, by showing how to reconsider relations between politics and nature. With my colleagues Yannick Barthe and Pierre Lascoumes (2009), I have explored the institutional configurations that make it possible to reconcile scientific adventure and political concerns. One of the main results that we have obtained pertains to the role of specialists and professionals. We have shown that the distinction between experts and lay people is meaningful and interesting only in situations where the question of the formulation of problems or matters of concern is basically sorted out. In these cognitively cold situations (but which can be politically very hot !) the knowledge to produce or to mobilize is fairly well defined. But these situations are not the most interesting. The

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challenges increasingly concern the formulation and framing of issues and problems, the research orientations to promote, and the modalities of implementation or adjustment of the knowledge and techniques elaborated. Examples of such situations abound in the fields of health and the environment. With Vololona Rabeharisoa, I have studied patient organizations that have become heavily involved in clinical and biological research, as well as in technical innovation (to compensate for their disabilities). These patients behave like real researcher-investigators, and we have called them researchers in the wild. They weave dense networks of collaboration with professional researchers and practitioners. Thus, communities of research and innovation are constituted in which patients and specialists collaborate. Patients are unquestionably lay experts – a term that Epstein used to describe the behaviour of patients in the case of the Aids epidemic. But in my opinion it is more accurate to get rid of both words, lay and expert, and to keep only one, that is, researcher or investigator.

By struggling to produce new knowledge, new therapies and suitable prostheses, these patients are also struggling to define and assert an identity that suits them. Rabinow spoke of biosociality to highlight the interlinking between these two movements : acquiring knowledge to exist socially, and existing socially to be able to conduct successful investigations. Hence, the importance of conceiving of institutions – and the actors fervently apply themselves to it – that allow for the synchronization of this dual dynamic, without for all that eliminating what makes each of them specific. We need political and scientific institutions that enable the simultaneous development of collaborative research between professional investigators and investigators in the wild, on the one hand, and the composition of the collective, on the other.

This view of political and scientific processes opens the ways to new forms of sociological research. With the concerned groups, sociology can form research communities that endeavour to collaboratively introduce organized reflection on this complex process. Such reflection serves both to construct new identities and to produce knowledge and know-how. This is what we have done with the 'Agence Française contre les Myopathies'. After ten years of collaborative research, everyone can review their results. I believe that the organization has learned to know itself and to situate its action. It has integrated the idea that patients are researchers in the wild, that they have to exert their influence on research, and that by structuring research activities they can construct their identity and make it recognized and accepted. And we, as social scientists, have been able to elaborate an analytical framework that helps us to understand the subtle relations between the new way of doing research and the new way of composing the collectives in which we live. Moreover, this adventure has helped us a lot to define institutions capable of maintaining this dynamic in which science and politics are practised in a radically new way.

Question : Considering that this interview is going to be published in an academic journal from a post-graduation program, we would like to ask you about what kind of methodological decisions you would suggest to a young researcher who is starting his career in STS studies ?

Your question is difficult and tricky. I suggest that the choice of research subjects be based on the opportunity they afford for collaborative research. One should start with matters of concern, problems which have not been framed, situations of strong uncertainty, to constitute an investigation collective that will benefit the persons concerned and 'professional' researchers who wish to become PhD students. We are

obviously very far from research-action, in which social actors arrive with questions for which they expect answers. The idea is no longer to study those who come to see us and who ask us, the experts, what they should do, but those who are asking questions on what they are and on their way of being. In the one case the expert/lay (expert) divide is maintained ; in the other a cooperative investigation is set up to reveal new identities and to construct the society that will be composed of these identities. ■

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Prix Tocqueville 2008 intervention de Monsieur Valéry Giscard d'Estaing

Résumé de l'intervention de M. Valéry Giscard d'Estaing

Deux questions se posent, pourquoi un prix Tocqueville et pourquoi le prix Tocqueville à Raymond Boudon ? Ces deux questions sont liées entre elles, Raymond Boudon reprenant les interrogations et les analyses de Tocqueville.

Raymond Boudon se demande notamment pourquoi on peut parler en France de « pouvoir de la rue », expression intraduisible en anglais ou en allemand (et pour cause...). La réponse à cette question est à trouver dans l'analyse que Tocqueville fait de la centralisation en France, notamment dans son livre *L'Ancien Régime et la Révolution*. Tocqueville montre la manière dont les organisations locales, ce qu'il appelle « l'ancienne constitution de l'Europe », ont été balayées par le mouvement de centralisation propre à la France de l'Ancien régime, mouvement continué et amplifié avec la Révolution. Depuis, le pouvoir exécutif qui essaie de réformer, pouvoir apparemment fort, se trouve affaibli par la cristallisation des mécontentements. Ces mouvements de rue se conjuguent avec le développement du relativisme. Dans son analyse, Tocqueville oppose la centralisation gouvernementale, qui se combine avec l'existence de multiples associations et organisations intermédiaires, propre aux États-Unis qui l'ont héritée du Royaume-Uni, à la centralisation administrative propre à la France. Par ailleurs, dans la société française, la passion dominante est celle de l'égalité. Du coup, on estime que toutes les opinions se valent, qu'il n'y a que des interprétations et des points de vue, et pas de faits. C'est en quoi le pouvoir de la rue, s'opposant frontalement au pouvoir central, est lié au relativisme.

Raymond Boudon reprend Tocqueville et Weber et, rejetant quant à lui le relativisme, estime comme eux que si, à court et moyen terme, la démocratie se caractérise par le polythéisme des valeurs, les controverses, les affrontements, à long

En 2008, le prix Tocqueville a été décerné à Raymond Boudon. Le président du jury, M. Valéry Giscard d'Estaing, n'avait pu se rendre dans la Manche à cette occasion et une cérémonie a été organisée à l'Institut de France le 9 février 2009.

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