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► **To cite this version:**

Bruno Latour. Biography of an inquiry: On a book about modes of existence. *Social Studies of Science*, 2013, 43 (2), pp.287 - 301. 10.1177/0306312712470751 . hal-03461030

HAL Id: hal-03461030

<https://sciencespo.hal.science/hal-03461030v1>

Submitted on 1 Dec 2021

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Biography of an inquiry: On a book about modes of existence

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Abstract

Since the Inquiry on Modes of Existence has been long in coming and has connections with all the successive field works done by the author, the paper tries to retrace the main steps that have led to the project. It shows that it has preceded the work done in actor-network theory and explains the link between philosophy and anthropology through the peculiar notion of ‘mode of existence’.

Keywords

anthropology, modernity, modes of existence, rationality

For a philosophy that is empirical and not simply empiricist, investigation offers the only way to ferret out its concepts and put them to the test before proposing a version that can be submitted to critique by one’s peers. And yet even though inquiry as a genre benefits from a distinguished and intimidating prestige in philosophy, it is fairly unusual for an author to propose to carry out an investigation with the participation of his readers. This is nevertheless what I propose to do in publishing a book titled *An Inquiry into Modes of Existence: An Anthropology of the Moderns* (Latour, 2013) alongside a digital site that allows its visitors, who will have become coinvestigators, to inspect its arguments and go on to suggest other fields to study, other proofs, and other accounts. By means of this arrangement, I invite my coinvestigators to help me find the guiding thread of the experience by becoming attentive to several regimes of truth, which I call *modes of existence*, after the strange book by Étienne Souriau (2009 [1943]), recently republished, that features this phrase in its title. This banal and quasi-ecological expression refers to a specific speech act – each with its peculiar felicity and infelicity conditions – to which is added the claim that a highly specific type of world is being inhabited. Souriau’s argument is

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not to say that there are several ways to talk about one world but several ways for the worlds (in the plural) to be addressed.

The use of these modes is what allows me to offer the Moderns (what this term encompasses will of course have to be specified) a more realistic description than the one presenting the advent of Western reason or the one authorized by the critique of that same reason. My hypothesis is that each of these modes makes it possible to respect, in the empirical areas I have pursued up to now, a certain tonality in the experience, the felicity or infelicity conditions particular to each case, especially (here is where things become tricky) a specific *ontology*. In fact, each mode requires us to encounter distinct beings that must be addressed in their own languages. The classic question of philosophy, 'What is the essence of technology, science, religion, and so on?' then becomes, 'What are *the beings* appropriate to technology, science, religion, and how have the Moderns tried to approach them?' But how can the multiplication of these modes be justified when the civilization that one claims to be studying conceives of itself on the basis of only two categories, object and subject (though in many different combinations, to be sure)?

When readers fail to understand why I have continually changed fields, and when they do not see the overall logic of my research – which leads them to look for my books in different aisles of bookstores (if they find them, i.e. if they look for them!) – their comments amuse me, for I know of no other author who has so stubbornly pursued the same research project for 25 years, day after day, while filling up the same files in response to the same sets of questions. This is why it may be useful to explain how I arrived at such an unusual form of philosophical anthropology. Not to tell my life story – if a system is solid, one need not be overly concerned with its author – but rather to sketch the biography of this argument on the basis of its history. No one can be astonished by the empirical birth of an empirical philosophy. In this article, I would like to pursue a contradictory exercise and recount the chaotic emergence of a systematic argument whose persistence over more than 30 years is surprising even to me.

If I am to go back to the past, the conscious past – I shall spare the reader the tribulations of my unconscious – we shall have to begin with a convergence between Charles Péguy and Rudolf Bultmann. Every September, despite the harvest that was so important in the wine trade, my parents used to take me on a pilgrimage to Orléans for the annual Péguy fest. If I was deeply influenced by reading Péguy's *Clio* (1961), it must have been because I conflated the lessons of *Clio* the muse, that great master of hermeneutic, with the meticulous, maniacal, and fertile scholarship of Biblical exegesis. From 1966 to 1973, when I was a militant Catholic student at the University of Dijon, I had the good fortune to have as a philosophy professor André Malet, a Protestant minister and Rudolf Bultmann's French translator (Bultmann, 1971). In his hands, which were as lustrous as parchment, the Biblical text finally became comprehensible, revealed as a lengthy process of transformations, inventions, glosses, and diverse rationalizations, which, taken together, sketched out a layer of interpretations that played out – this is the essential point – each in its own way, the question of fidelity or treason: faithful or falsified invention, impious reworking, or astounding rediscovery? We spent hours outside of class comparing the various resurrection narratives; for example, should they be read as *informational* stories (the tomb really is empty) or as *transformational* stories (the angel with a raised

finger makes it clear how the scriptures are to be read)? Moreover, how do such stories manage to *resuscitate* the person to whom they are addressed?

Because they escaped from an inexplicable form of transcendence and immobility, because they became localized, historical, situated, artificial – yes, invented and constantly reinvented – and because they raised the question of their own veracity anew at every turn, these texts finally became active and accessible. The crushing responsibility of the reader invoked so marvelously in *Clio* was given a scholarly description by Bultmann. Strangely, to my eyes, the systematic deconstruction by exegesis of all dogmatic certitudes, far from weakening the truth value that the successive glosses played out over and over, made it possible at last to raise the question of religious truth. But only on condition of acknowledging that there was an itinerary of veridiction with its own felicity conditions, an itinerary whose traces remain in exegesis and of which Péguy had tried to reproduce the disturbing tonality with his repetitive style at the turn of the 20th century.

In a thesis defended in 1975 and quickly given over to the biting critiques of dust mites, I had developed this argument in an analysis of Mark's gospel and of 'Saint' Péguy (Latour, 1975). (I had added a third saint, the poet Saint-John Perse.) A bit of Derrida and Lévi-Strauss plus a large dose of Deleuze helped give the argument the contemporary sheen that neither Péguy nor Bultmann, of course, could have provided. According to my analysis, if the texts on the empty tomb did not convey information, they did something much more significant, by indicating the possibility of other regimes of veridical and above all *verifiable* speech (Latour, 1977). What is certain is that I emerged from that formative period armed with total but somewhat paradoxical confidence about the fact that the more a layer of texts is interpreted, transformed, taken up anew, stitched back together, replayed, and rewoven, each time in a different way, the more likely it is to manifest the truth it contains – on condition (this is the part I retained for later use) that one knows how to distinguish it from a different mode of truth, pure and perfect information (which I did not yet call 'Double-Click', since computer mice had not yet arrived to tickle our fingers). A long struggle against the eradication of meditations was about to begin.

Since it was still possible to escape from military service by engaging in 'cooperation', a sort of French Peace Corps, I left the Gray lycée in Haute Saône behind and went to teach in the technical lycée in Abidjan. Imagine the brainwashing in store for a provincial, bourgeois Catholic with an advanced degree in philosophy who finds himself transported into the cauldron of neocolonial Africa, with his wife and child, no less. In the Abidjan of 1973–1975, I discovered all at once the most predatory forms of capitalism, the methods of ethnography, and the puzzles of anthropology. One of them has never left me: why do we use the ideas of modernity, the modernizing frontier, and the contrast between modern and premodern, before we even apply to those who call themselves civilizers the same methods of investigation that we apply to the 'others' – those whom we claim, if not to civilize entirely, then at least to modernize a little?

By good fortune, the field study proposed to me by my colleagues at ORSTOM (now the Institute of Research for Development (IRD)) dealt with factories in the Ivory Coast and the impossible question of 'Ivorizing' the cadres: why did the expatriate bosses find no African cadres competent enough to replace them? I felt right away that if, to answer

this question, I were to use the schema of a struggle between modernization and archaism, I would not be able to understand anything. But I realized at the same time that there was no alternative schema, since we did not know how to describe in ethnographic terms the meanings of 'rational', 'effective', 'competent', and 'profitable', all qualities, I was told with the scornful assurance of expatriates that seemed to be lacking in the African cadres. I saw clearly that these adjectives of combat and conquest did not result from any independent description; they were slogans, battle cries. If people hastened to invoke cultural dimensions, cognitive limits, 'black souls', and 'African mentalities', it was because their definitions for the work of thought were not sufficiently material and concrete. There was a flagrant asymmetry here: the Whites anthropologized the Blacks, yes, quite well, but they avoided anthropologizing themselves. Or they did so in a falsely distant, exoticizing fashion, by focusing on the most archaic aspects of their own society – communal festivals, belief in astrology, first communion meals – and not on what I was seeing with my own eyes, educated, it is true, by a collective reading of *L'Anti-Oedipe* (Deleuze and Guattari, 1972): industrial technologies, economization, 'development', scientific reasoning, and so on: in other words, everything that makes up the structural heart of the expanding empires.

From this arose the idea of applying the methods of the social sciences, ethnography in particular, to the most modern practices. In 1975, California seemed to be at the forefront of humanity's advancement, even described as its 'target-tracking missile'. A scientist friend from Dijon, Roger Guillemin (a cherished priest-uncle's former altar boy!), had suggested that I join him at the newly established Salk Institute in San Diego, if I could come up with funding. It took me only a few pages to write up the project of an anthropology that would provide an ethnographically based description of those who call themselves rational and modern. I still remember the astonished look of the consular agent who was to rule on my Fulbright grant application when he heard my confident claim that I was going to make anthropology *symmetrical* at last! And yet I found it perfectly normal to embed comparative anthropology in a trajectory that led from Abidjan to San Diego by way of the ancient cobblestone streets of Beaune, traversing three forms of modernity as different from one another as possible. Destination: the United States. Field: a scientific laboratory. With the help of the journal I have been keeping since I was 13, I sketched out in a few lines the project of comparing modes of truth, the first indication of a book that would appear only 40 years later.

Imagine my amazement when I discovered, in Guillemin's laboratory in 1975, located in a splendid Louis Kahn building overlooking the Pacific Ocean, that scientific work bore a strange resemblance to the exegesis I had left behind in Burgundy. As a good ethnographer, I knew that I should be skeptical of the abstract notion of ideas floating in the air, but I did not imagine that following the 'inscriptions' (Derrida, 1967), all the ideography of instruments, would provide such a fertile take on those ideas (Latour and De Noblet, 1985). And yet everything in the mysterious fabrication of facts became clear once I undertook to focus on the documents of the researchers in white lab coats who showed such interest – an interest that was at once totally obsessive and totally casual – and when I went on to follow the transformations of these documents step by step, it was as though the sciences could be embodied in fragile and apparently impalpable intellectual technologies (Lynch and Woolgar, 1990). It is true that I was helped not only by

Derrida but also by François Dagognet, whose little book *Écriture et iconographie* (1974) put me on the right track: I followed it like a hunting dog, nostrils flaring.

How could that form of materiality have disappeared from epistemology as completely as Biblical exegesis had disappeared from the preaching of Catholic dogma? How could one explain that, here too, the appeal to an abusive transcendence had been able to dissimulate the layer of texts, of documents, that had to be continuously rewoven in order to produce a truth that could not be based, try as one might, on a firmer foundation? Was it possible that scientific veridiction was as far away from Double-Click information as the latter is from religious truth – in which case we would find ourselves faced with *three types* of veridiction, each entirely distinct from the others and true in its own genre and its own fashion?

In San Diego, I fell into the habit of bringing the smartest people I met around into the laboratory where I was spending 12 hours a day. I hoped they would shed their wisdom on the enigma of an anthropology of the sciences that I did not yet know how to decipher. This is where I met many of the main characters of the field of science studies that was being invented at the time through a crossover between Mertonian sociology of science, ethnomethodology, and the emerging Edinburgh school. Steve Woolgar, Harry Collins, Trevor Pinch, and Karin Knorr, all came to the Salk Institute, meeting some of ‘my’ scientists together with the colleagues from nearby UC San Diego, such as Aaron Cicourel, Bud Mehan, and Joe Gusfield.

As luck would have it, just when I was asking my hopelessly difficult questions, I made the acquaintance of semiotics, thanks to Paolo Fabbri. I still recall my admiration when Fabbri, with his high-pitched voice and his lovely Italian accent, picked up a text that had emerged from the lab machinery – a text full of diagrams and chemical formulas concerning the discovery of a neuropeptide, the soon to be well-known Thyrotropin-Releasing Factor (TRF) – and calmly set out to produce a Greimasian analysis of it, as if he were dealing with a fairy tale (Latour and Fabbri, 1977). In Paolo’s capable hands, the varied figuration of the *actors* was no longer to be confused with the underlying detection of the *actants*. I suddenly understood that the nonhuman characters had their own adventures that we could track, so long as we abandoned the illusion that they were ontologically different from the human characters. The only thing that counted was their *agency*, their power to act, and the diverse figurations they were given.

A world opened up then that I have not finished exploring and that lent itself admirably, I have to say, to the principles of a comparative anthropology. Collectives – I did not yet have this word – were distinguished by the figuration they gave the actants, through the tests they had put their characters through, but not at all because some of these were realistic, rational, and real, and others symbolic, imagined, or mythical. The power of semiotics derived precisely from its sublime and radical *indifference* to the obvious realism of subjects and social actors. I had found the ideal condition for tracing the inventiveness of sciences that had been crushed by the task of mimicking the world, derailed by being so often confused with information about pitiful ‘matters of fact’ isolated from any ‘matters of concern’. Only the semiotics of scientific writings and inscriptions, freed from ordinary realism, could deploy this totally original mode of reference.

My excitement will not be hard to understand. I had a strong sense that this phenomenon, the circulation of scientific truth along chains of inscriptions, would not have an

easy time making a place for itself in philosophy because of the immense prestige of science. Since the path of inscriptions bypassed both knowing subjects and known objects, the mode of existence of scientific knowledge seemed to deserve a better habitat than the no-man's-land between words and things. I had no idea that it would be necessary to move heaven and earth to give it the place it warranted, and that, 40 years later, I would still be working at the task with pick and shovel.

The passion for semiotics – which cut its teeth as much on Biblical texts as on literary fiction – might have led to a simple ‘textualization’ of scholarly activity, if I had not discovered in Garfinkel’s (1967) research, around the same time, an entirely different way of breaking with the social realism that was so widespread in sociology (not knowing at the time that Mike Lynch was working along the same exact lines). The odd, jargon-laden genius of ethnomethodology comes from the discovery that every course of action, even the most ordinary, is constantly interrupted by a minuscule *hiatus* that requires, from moment to moment, an inventive act of repossession by the actor equipped with his own micromethods. A clumsy lab worker myself, I unintentionally multiplied experiences of ‘breaching’ that revealed by contrast the hard-won competence of my comrades in the laboratory. I was put off by Garfinkel’s style, but I understood that he was proposing to do for all reports or accounts what I had already identified in religious exegesis and what I was discovering on the lab bench in the exegesis of scientific texts: no continuity of a course of action is possible without an inventive repossession that gives the social actor reflexive capabilities, sources of innovation, and even sociologies and ontologies whose uses go far beyond the capacities of ethnologists. The investigatee always knows a great deal more than the investigator.

This, moreover, is why I still found the philosophy of my youth so indispensable: it alone was untamed enough to manage to follow, without too much astonishment, the inventiveness of agents. It was through metaphysics that one could hope to become a good ethnographer. The idea that actors need no longer be viewed as ‘cultural dopes’ resonated marvelously with the actants deployed by semiotics. Happily, protected by my raw ignorance of sociology, I could not know that Garfinkel was going to remain as radically unassimilable by social scientists as Greimas was by epistemologists (Lynch, 1985). Thus, nothing prevented me from using the terms ‘social’ and even ‘social construction’ to describe the adventures of the nonhuman beings that were beginning to populate collectives (Latour and Woolgar, 1986). I had no way of knowing that it would take me a quarter of a century to get myself out of the misunderstanding created by the use of the word ‘social’ and from all the complications that turned out to be attached to it, to my great surprise (Latour, 2005). Although since my happy childhood in Beaune, I had not budged an inch from the most solid realism, and although we were the first to describe the materiality of the sciences with precision at long last, I suddenly found myself accused of an apparently abominable crime, committed inadvertently: calling scientific objectivity into question through ‘relativism’.

Back in France in 1977, looking for colleagues, I found myself on the premises of the Direction Générale de la Recherche Scientifique et Technique (DGRST) on the rue de Varenne, on the strength of the summary of a contract to study the evolution of biomolecular chemistry in which the author, a certain Michel Callon from the École des Mines, calmly explained that he was not going to submit his analysis to preliminary checking by

chemists because he wanted to explore an approach that would be *independent* of scientific authority. Ah! I was impatient to meet the bold fellow who professed to be talking about science with such freedom! Our meeting was a stroke of luck that ended up allowing me to work for a quarter of a century in the tranquil bubble of the Center for the Sociology of Invention (CSI).

It was thanks to Michel Callon that I was introduced to industrial field studies. As we saw things, the technological arrangements we had to trace from the standpoint of innovation (innovation was fashionable at the time, and money to study its origins was plentiful) presented, as we saw it, a form of realism that the notions of efficiency or profitability could not exhaust. In the course of our investigations, we reconstructed the way engineers had to design an entire world in order to succeed in maintaining their riskiest innovations just a little longer. Here again, I fell upon a course of action that no continuity, no transport of necessity, and no somewhat solid causality could explain. But the hiatus peculiar to the new technologies – by definition, it is always a matter of breaking with existing practices through innovation – was astonishing because, in the end, when everything was in place, when the arrangement was actually working, a *detour* had occurred through the intermediation of objects whose status became quite strange; these were the technological objects whose ‘mode of existence’ – this was the first time I had heard the term – had been proposed and explored by Gilbert Simondon (1958).

Just as sciences, grasped in their practice, could not be contained within the narrow framework of epistemology, neither could technologies, especially the most advanced, most modern ones, be contained within the simple idea of effective action on matter. They had to do with magic, religion, and philosophy; they had their own world, full of organizations, negotiations, calculations, metaphysics, and even morality; they represented a complete challenge to ethnographic or sociological description in that they upset the borders separating themselves from human subjects (Latour, 1988b). But in addition, and more radically, they populated the collective of nonhuman actors that brought to bear on the human actors, by delegation, as it were, a dizzying number of unanticipated consequences, which we were so delighted in tracing with our colleagues in technology studies (Bijker et al., 1987). The technological infrastructure was, or so it seemed to Callon and John Law (1986), the most ‘social’ element in a given society, provided that we returned to the etymology of the adjective and enabled ourselves to follow all the *associations* required to extend the network, especially if we added to it the intellectual technologies that we had learned to follow by studying laboratories and that turned out to be mixed up everywhere with technological organizations (Callon, 1981). To machines, we had to add offices; to gears and cogs, we had to add accounting technologies; and to the resistance of materials, we had to add bureaus of standardization.

And yet in the eyes of our colleagues from the sciences legitimately called ‘social’, the social did not seem capable of absorbing the multiple and labile connections that we had designated by the word ‘translation’, purposefully borrowed from Michel Serres (Callon, 1986). We attended Serres’s seminar every Saturday, in the smoke-filled amphitheater in the Sorbonne (people smoked in classrooms then!), profiting every time from the boldness with which Serres developed his ‘anthropology of the sciences’ based on the very fertile principle of exegesis according to which the single metalanguage of a text – a poem, a fable, a memoir, or a scientific treatise, it hardly mattered – could always be

found in the text itself. All one had to do was look for it, a lovely methodological lesson for following the ‘actors themselves’ and an approach compatible with both semiotics and ethnomethodology. Describe, describe, and then describe some more. Explanations and contexts were much less important than connecting in a single tight interpretive network – a text by Livy, an argument by René Girard, and a topological theorem. The explanation would come later, if there was time.

The discovery of technological detours and delegations added to my list a new mode whose ontology was very inadequately accounted for by the notion of ‘materiality’. I was beginning to wonder whether I might not have to change my philosophy, when I was lucky enough – always a matter of luck – to get a phone call from an anthropologist in California, with an invitation to attend the first colloquium for specialists in *Papio anubis* monkeys, which were beginning to be studied systematically. She needed an observer of controversies among scientists! Thirty-five years later, the shock of my encounter with Shirley Strum, along with primatology, ethnology, the Kenyan savannah, and, above all, monkeys, has not faded. I was to discover, first of all, that an intense social life – that of the baboon troops that Shirley had been following for 7 years and is still following in 2012 – was perfectly compatible with an extremely limited use of technological tools (Strum, 1982).

While baboons manifest an unimagined degree of social complexity, wholly worthy of Garfinkel, they still use only their brains and their bodies. Here was something that offered a fine confirmation to the intuitions Callon, Law, and I had had about the technological fabrication of society: what characterizes humans is not the emergence of the social, but detours, translations, the enfolding of all courses of action into more and more complicated – but not necessarily more complex (Strum and Latour, 1987) – technological arrangements. A few months after I returned from fieldwork in Kenya, in 1979, we drafted the foundational text of the actor–network theory (ANT), ‘Unscrewing the big Leviathan’ (Callon and Latour, 1981). The social theory it proposed was open enough to absorb the associations between humans and nonhumans – in particular, by making *change of scale* a consequence of using organizational as well as material technologies. The performativity of the social by the sciences, including the sciences of economics, legislation, and management, was thus opened more broadly to empirical research.

In shifting from the social to associations, the analyst benefited at last from as much freedom to maneuver as informants had; we could break out of the narrow framework of the ‘social dimension’ of scientific or technological phenomena whose content was presumed to escape the analyst entirely. The expanding sociotechnological networks were the ones to follow. We began to proclaim this from the rooftops, in a timely and untimely fashion. We must have been unbearable, but, after all, we were young and passionate, and besides, we were right! History – I mean the history that ecology was about to force humans and nonhumans to take into account – was about to prove this. Here, at least, no one could take us by surprise; equipment in hand, we were waiting for this new world or, to put it better, we were waiting for it like the servants in the Gospel, our lamps already lit.

And yet it was not the invention of the ‘sociology of translation’, as important as that may have been, that I retained from my long association with Strum and soon with her husband, David Western. No, the key discovery was the association in a single mode, one

totally surprising to me, of living organisms left to their own devices. I knew laboratories, of course; I was beginning to measure the artificial aspects of experiments – artificial in the good sense of the term; I knew perfectly well that there was nothing natural about the countryside (especially, the perfectly aligned vineyards of my native Côte d’Or), but how was I to qualify the space created by baboon troops that were being followed by researchers? *Followed* and not *preceded* by them, this says it all: how could I not be overwhelmed by these troops of monkeys whose path was crossed by leaping gazelles, by zebra or buffalo herds, and occasionally by a pachyderm slipping soundlessly by? No, this was not nature untamed, not the celebrated ‘wildlife’. Or rather, yes, it was all that, but it was something quite different as well: it was a segment in the trajectory of phenomena left to themselves, without the intimidating presence of human subjects. These later were pushed off into the wings. And yet these researchers capable of following and not dominating their object of study were producing science, and very good science at that (science I was assimilating as fast as I could by teaching a course with Shirley on the evolution of technologies and ecology at UC San Diego almost every year from 1979 to 1992). The various practices of primatology, from macaques tied down to chairs for torture sessions in laboratories to chimpanzees imprisoned in zoos to baboons followed for a month at a time, day after day, by enthusiastic doctoral candidates, produced a magnificent lesson in philosophy: it contained all possible postures of knowing subjects and known objects. The passion it inspired in Donna Haraway, whom I had the good fortune to meet in 1981, is understandable (Haraway, 1989). As we followed the baboons on foot, Shirley, as invisible in their midst as Athena at the heart of a battle, speaking softly, explained the astonishing complexity of their societies to me even as she kept on taking notes. I began to imagine other relations between the course of knowledge and that of the known world. But to get there, I needed the opportunity to acquaint myself with ‘the other metaphysics’, that of William James and A.N. Whitehead.

At the time, I had no words to convey the impression made by my collaboration with Shirley and the ethnologists other than *irreduction*. This term had been the object of a little ‘politico-scientific tractatus’ – a curious philosophy without readers, a somewhat odd mix of network theory, then-current Nietzscheism, and an attack on epistemology, all this against the background of the Cold War (Latour, 1988c (published in French in 1984)). It blended a spot-on intuition – the distinction between relations of power and relations of reason makes both reason and power incomprehensible – with a total contradiction unnoticed by me at the time, since I claimed to use the same metalanguage, in terms of translation, networks, and entelechies, for all associations. If I have always had a soft spot for this acerbic, youthful book, it is because I now know that it had to do with a particular mode of existence, and not, as I thought at the time, with an irreductionist philosophy – the mode that allows one to deploy networks of heterogeneous and unanticipated associations without letting oneself be intimidated by distinct domains. I did well to demonstrate its effectiveness in a historico-semiotic study of the discoveries of the French national hero, Louis Pasteur. As a mode, network analysis is indispensable to investigation; I went on to show this once again in the delectable case of an automated subway system (Latour, 1996). But, like all modes, it tends toward hegemony and tends to misunderstand the others. Still, up to now, if someone had asked me, ‘What is your philosophy?’ I would not have known how to respond except by saying ‘Read

Irreductions'. (Not to worry: no one has ever asked me that question, since the tumultuous quarrels over relativism and the science wars have in the meantime turned me into a mere sociologist, adherent of a 'social construction' according to which 'everything is equal', objective science and magic, superstition, and flying saucers.)

To understand how things finally came together, two other encounters have to be noted – one's thinking seems to be a matter of decisive encounters whose effects one pursues in total solitude. (Without solitude, nothing happens; without encounters, nothing happens either.) No sooner had I returned to Paris than Paolo Fabbri put me in touch with Françoise Bastide, an outstanding physiologist and semiotician with whom I had the great pleasure of working until her premature death in 1988. Françoise, with all the seriousness of a single woman and a Protestant, applied to texts the same absolute respect that she had shown to study the countercurrent multiplier of kidneys when she was in a laboratory at the Collège de France. A specialist in and author of scientific texts, she thus knew very well that semiotics, despite its claim never to depart from texts, in practice actually never ceased to rely on what was happening outside them. The dilemma was to figure out how to approach this 'outside' without falling back on clichés about human subjects caught up in a social and material context. The intuitions of semiotics had to be *extended* beyond its original framework – Biblical texts and literary fictions – without sacrificing the independence of semiotics in relation to ordinary realism (Bastide, 1990, 2001). Greimas, whose shiny pate tended to disappear behind the smoke produced during his seminar, encouraged us in this project with a smile. (Cigarettes probably killed him, too, as they did Françoise.)

This is where we perfected a little piece of machinery based on the theory of enunciation. Fiction need not be too concerned about this: once an utterance has been produced within the frames of reference of a text – for it is almost always a question of texts – the itineraries of their actors (or better, their actants) are easy to follow. Yet this cannot be the case for at least two regimes of enunciation: scientific instruments and technological arrangements. For these, without question, the processes of shifting out have to be followed with care. The nonfigurative characters in a scientific text may well travel like fictional beings, but they have to return in order to *bring back* something that finds itself in the hands of the enigmatic enunciator, the one whose presence is without importance in a fictional text, since no one asks Flaubert whether he really has Madame Bovary's birth certificate, to show as a proof of his claims (Latour and Bastide, 1983). Einstein and his little relativist characters served as tests that allowed us to identify the weirdness of this fiction on the path to gradual verification (Latour, 1988a). But we had the most trouble with technological objects, because there we had to explode the textual framework. And yet the problem was not materiality, but, here again, the particular role of the enunciator capable of absencing herself, since the object stayed in place without her.

In fact, as we soon noticed, the very possibility of the famous shifting out of levels of enunciation came from technology. The absence of a flesh-and-blood narrator in a fictional narrative is not a semiotic property of fiction but of *books* as technological objects: without books, the narrator would be a storyteller as little absent from what he is uttering as the manipulator of marionettes in a *bunraku* performance. Françoise and I in fact had the idea that it would be possible to compare *regimes of enunciation* – this is the term I used at the time – by going from one to another by way of the attention paid to the

respective roles of sender, receiver, and utterance. In 1986, I drafted an initial text, AMI, for 'Angel, Machine, Instrument', using a common vocabulary to establish the comparison. (It took me only 26 years to get from AMI to AIME (An Inquiry into Modes of Existence)) Unfortunately, this project came to a halt with Françoise's death, for she alone had the semiotic technology required to develop this model (Bastide, 2001).

If my readers have the impression that this new book on modes of existence comes in the wake of work in the sociology of science and technology, as if, after undertaking empirical studies, I had returned to philosophy at a more advanced age, this is simply an optical illusion. *Science in Action*, a book that I wrote in between these two phases, was published in 1987; at the very moment when I was writing up the investigation begun in 1986 into the various regimes of veridiction. By following the circulation responsible for the production of facts and the construction of machines, one can read *Science in Action* not only as an application of ANT – which it undoubtedly is – but also as a detailed study of three regimes of truth: scientific reference, technological machinations and, opposed to both of them, the Evil Genius of Double-Click information. In fact, two distinct events occurred: on one hand, my encounter with Isabelle Stengers and, on the other hand, the unexpected success of the so-called ANT. This success and the ensuing disputes delayed the publication of the other project, although I continued to pursue it.

It is to Stengers, whom I had known since 1978, that I owe the constant disruptions that she imposed on all the social explanations – even those improved by the ANT – that Callon and I kept on producing. She challenged all my sociosemiotic developments with a vigorous 'I know, I know, but even so ...' and making a characteristic rapid circular movement with her right hand, demanded that something be brought to the surface in the analysis, something that would be the world but grasped differently. Even Pasteur's microbes; even Aramis's magnetic couplings, the automated subway system; and even Michel Callon's famous scallops, all of them undeniably present, actants and movers, glittering with reality, still did not offer, in Stengers' eyes, a sufficient guarantee that we had pulled ourselves away from the text, the social and the symbolic. To manage that, we would have had to grasp the world without dragging through its human subjects and their obsession with knowledge conceived as the relation between words and things.

I am almost certain that it was in 1987, during a conversation by the swimming pool at Fondation Les Treilles, in Provence, that Stengers shared with me an astonishing quotation from Whitehead, who was even less well known at the time than Gabriel Tarde, about the risk taken by rocks – yes, rocks – in order to keep on existing; it must have been the famous passage about Cleopatra's needle on the Charing Cross Embankment in *The Concept of Nature* (Whitehead, 1920: 165–166). In August of that year, stretched out in the sun on an island across from Gothenburg, in Sweden, I could not stop running my fingers over the rough red surface of the rocks as if to find out whether Whitehead could have been right! At that moment, everything – what I had discovered in Kenya and what the principle of irreduction had hinted at obscurely – became clear. There exists a completely autonomous mode of existence that is very inadequately encompassed by the notions of nature, material world, exteriority, and object. This world shares one crucial feature with all the others: the risk taken in order to keep on existing. Thus, the hiatus that I had detected very early on in exegesis and that I had found in the study of scientific inscriptions, in the disjointed itineraries of courses of action, and in the surprising detours

of technologies, the same hiatus was here as well, here in the first place, in the apparent continuity of being-here. An epiphany linked up with all the others, and especially the one whose scenario I had developed in *Irreductions*, when ‘for the first time, I saw things unreduced and set free’ (Latour, 1988c: 163). There was nothing inevitable, nothing definitive, and nothing irremediable in the tribulations of subject and object. One could think differently (Stengers, 2011).

From that starting point, everything quickly fell into place. In June 1988, when I got off the plane that had brought me to Melbourne for two precious months of total, blessed solitude, even in the fog of jet lag I was able to chart in one fell swoop the regimes that I was going to have to investigate more systematically (Latour, 1998). A few of the regimes or modes were still missing, but the essentials were in place, especially the principle of comparison on the basis of a metalanguage that has no goal but to keep ontological pluralism from being crushed by the subject/object schema. In particular, the little framework – call it semiotic, theoretical, philosophical, whatever – was no longer opposed to the deployment of fields of inquiry. I could be, without contradiction, a philosopher *and* an anthropologist *and* a sociologist: everything led to the investigation and everything stemmed from it. Here began the adventure that the readers of the book on modes of existence are invited to prolong today, by participating, if they so desire, in the research themselves.

Before I conclude, it may be useful to recall the influence of these studies on the nature/culture schema for philosophical anthropology. Not for a moment had I forgot the shock of Africa, of neocolonialism, and of the advance of the modernizing frontier. How can one practice a truly symmetrical anthropology? While I was in Melbourne, I prepared a lengthy review of Shapin and Schaffer’s seminal book on Hobbes and Boyle, *Leviathan and the Air-Pump* (1985), which had just been published. This led, thanks to the work on regimes of enunciation, to a significant outcome in symmetrical anthropology: by providing a realistic description of the sciences at last, by showing their equipment, and by bringing the chains of reference to the foreground, it became possible to detach the representation of Nature both from the work of the sciences and from the movement of beings left to themselves, a movement that Whitehead had taught me finally to respect. An anthropology of the Moderns became possible, one that would transform the nature/culture schema used up to then by anthropologists as an indispensable resource into a topic that had, on the contrary, to be explored. (Once again, ‘the resource becomes the topic’.)

The result was not negligible, for it made it possible to detect the immense abyss between the modernist representation of history – that of a modernizing frontier – and real history – that of an entanglement between humans and nonhumans that is ever more intimate and ever larger in scale. But above all, it opened up, with other collectives – a term I used from then on to replace the overly anthropocentric term ‘society’ – the possibility of making comparisons, at last, that would be less biased by the idea of a modernizing frontier capable, in the long run, of extending to the entire planet. The ‘others’ are not really modern? So much the better: we have never been modern, and they never will be. An entirely different history awaits us. Outlined in 1991 (Latour, 1993), the search for the parliament of things, 20 years later, has only increased in actuality. Modernize or ecologize: we were going to have to choose.

As I saw it, the chief interest of *We Have Never Been Modern*, the negative version of an argument whose positive version I am offering now, is that it initiated a much closer collaboration with anthropologists, the genuine ones, around the ontological pluralism of collectives. It was no longer a matter, for Philippe Descola, or Eduardo Viveiros de Castro, or Marilyn Strathern, of comparing cultures against a background of Nature but of contrasting more and more sharply the ontologies of which just one, ours, uses the schema of mononaturalism and multiculturalism. Having been a servant of philosophy, anthropology is becoming, if not its mistress, at least its colleague; by becoming local or regional, ontology has become correspondingly deeper. For the science of being has more than one trick up its sleeve, it seems, and the end of the restrictions imposed by the notion of 'symbolic representation of a material world' opens up an all the more fertile program of research.

Between the science of being-as-being, the venerable discipline of ontology, and the science of being-as-*other*, anthropology, new bonds can be woven. All the more so in that the people Descola (2005) sees as naturalists, the Whites, frenzied users of the nature/cultures schema, in practice do something else entirely, which complicates their description a little bit more, as I see it. This is not insignificant, since the more and more pressing irruption of ecological questions requires us to pay closer and closer attention to the relations between cosmology and science. The singular term cosmology, a property of the exact sciences, and the plural term cosmologies, used in a somewhat casual fashion by anthropologists to describe diverse worldviews, are now converging within an enclosure that has become the new political world, that of contemporary cosmopolitics (Stengers, 2010).

In the end, the mystery as to what these Moderns have been up to remains intact. What has happened to them? If it is not Nature that they have discovered through the fog of their cultures, if it is not Reason that has finally shined light into the darkness of representations, what has in fact happened? Of what are they the heirs? To answer these questions of philosophical anthropology and of regional ontology, we need a method that provides an adequate depiction of the situations to be described. How many sensors are needed to do justice to the values deployed by the Moderns? I have been struggling to identify these sensors, in the hope that this brief return to the origins of my investigation will spur some readers of the modes of existence book to help me carry it out.

Acknowledgment

A version of this article has appeared in French "Biographie d'une enquête – à propos d'un livre sur les modes d'existence" in *Archives de philosophie*, Vol. 75 n° 4 pp. 549–566 (2012). Translation by Cathy Porter.

Funding

This research has been funded by the grant No. 26956 from the European Research Council (ERC).

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