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From Innovation to Markets and Back. A Conversation with Michel Callon

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Abstract

In this conversation, Michel Callon reviews the major events and questions that have marked his scientific career. He begins by presenting the personal and political path that led him, after completing his engineering studies, to join the Centre de Sociologie de l'Innovation at the École des Mines de Paris in 1968. His early work on the theme of innovation was conducted in a context where science and technology were the focus of multiple questions in economics and sociology. Michel Callon explains the collective research approach that led to the creation of the concept of translation, and the efforts to develop co-word analysis, an automated textual processing method designed to study the products of science and technology. It is from these questions on the processes of innovation, and from this long-standing dialogue with economists, that Michel Callon will develop, from the end of the 1990s, his work on markets. From research on the performativity of economic knowledge to the analysis of market agencements, this work has developed an original perspective on the dynamics of the economy, which goes beyond the criticisms usually levelled at capitalism by the social sciences. It invites us to develop a reflection on the plural roles that markets play not only in production and consumption, but also in the genesis of diverse social relations, in political organization and in crisis situations.

Keywords: Innovation; Science Technology and society; Economic sociology; Market agencement.

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Alexandre Mallard: Let's start from the beginning. You were initially trained as an engineer with a minor in economics. How did you become a sociologist? Are you a sociologist...?

Michel Callon: The project of orienting my career towards the social sciences unfolded progressively during my three years at the Paris School of Mines, which I entered in 1964. In high school, I intended to pursue research in physics. I eventually opted for the social sciences due to the influence of political debates at the time. French society was profoundly shaken by the events in Algeria. There were daily confrontations between the extreme right and extreme left; hatred transpired. This is how I learned about political debate, but also about colonialism. In primary school, I'd looked proudly upon the map of the French Empire that my teacher hung over the blackboard. The Algerian war sobered me for good.

A few years later, the discord was reactivated by war in Vietnam. When I entered the Paris School of Mines the United States had engaged militarily in the conflict; throughout Europe a wave of protests ensued. The premises of the Chinese Cultural Revolution emerged. As a result, there was a groundswell of intense intellectual activity in the Latin Quarter, spread through magazines, essays, books, and manifestos. Marx was at the heart of these debates and controversies of which I understood little. It was obvious that I had to read Marx since everyone else was clashing heads with him, but I also had to read the ethnologists since what was in question was the domination of what was then called "the West" over the rest of the world.

Reading Marx gave me a real intellectual jolt. To a young man with scientific training, the logic, coherence, as well as the simplicity of his analyses, at least those in *Capital* (Marx, 1890), possessed a *je ne sais quoi* that proved fascinating and convincing. However, the various and contradictory interpretations of Marxism that flourished at that time would soon reveal its weaknesses and make its theses questionable. I moved away from it. When I started fieldwork a few years later, I quickly realized that Marx's theory of labor value wouldn't get me very far. Paradoxically, it was by learning to disengage from Marx that I learned the most from him and was most influenced by his writings. In that period, I discovered French writers like Claude Lévi-Strauss, Maurice Godelier, Gaston Bachelard, and Jacques Ellul.

I was not particularly attracted to sociology. Nevertheless, Pierre Bourdieu's work left a direct and lasting impression, largely because he'd just published a brief and easily understandable book entitled *The Inheritors* (Bourdieu & Passeron, 1964). One of the effects of the book was to renew some purely economic analyses and claims by the students' unions, which had been organizing in favor of a student wage without considering the important impact of cultural inequalities. To me, the force of Bourdieu's work was its direct connection to social problems of the day. Moreover, when I was at the Paris School of Mines I participated in a study he directed of students attending France's elite universities, known to us as the 'Grandes écoles'. I was immediately struck by the distinctly Taylorist feel of the sociological work undergirding the study. Not only did this work distance the sociologists from the individuals they were interviewing, it also reduced the respondents to guinea pigs. My feelings were reinforced a few years later when I participated in a huge questionnaire targeting a legal profession, where I myself had the experience of laboriously forcing respondents to stick to the framework of the study. After 40 years in STS and following histories of objectivity by scholars such as Lorraine Daston and Peter Galison, and Isabelle Stengers, I now know there are many ways to practice science, and notably the social sciences. This is not only because there exist multiple theoretical frameworks, but because objectivity can be obtained by a variety of means.

A third encounter with Bourdieu would provide further material for disappointment and reflection. I was still a student when, within the context of the union for the *Grandes écoles*,

I was asked to organize student internships in what were then called "developing countries". For historical reasons Algeria had been chosen as one of the privileged partners. Given that Bourdieu had just published his work on Algeria I took up my finest pen to ask if he would consider a meeting with prospective interns. He sent a rather dry refusal, explaining that he was an academic and could therefore not get involved in politics! Retrospectively, the strategy he chose seems rather transparent. He accumulated academic capital that he would only convert into political capital far too late.

This may all seem anecdotal. For me it was not. In France, sociology was not yet a profession. The discipline of economics was equally fragmented. No pathway imposed itself, no epistemological imperatives guided my journey. I progressed by feeling my way through various terrains whose contours were never defined. I've never been a sociologist and I will never be one. I eventually took some sociology courses at Nanterre through distance learning during my military service. At the end of service, a friend made a providential call asking if I might be interested in joining the newly created Center for the Sociology of Innovation (CSI). As soon as I met the director, Lucien Karpik, I understood he was not the type to straightjacket other researchers with regards to theory. He was open to discussion, more interested in problems than in epistemological reflections and prerequisites. I accepted the offer without hesitation. This is how I became a full-fledged, yet faux sociologist. So faux, I immediately registered myself to do a Masters in economics.

AM: Before devoting your attention to markets, you were, for a long time, interested in scientific and technical innovation. The economic sociology you've developed is groundbreaking because it treats markets from the perspective of innovation. Could you explain to us the journey by which you brought innovation to bear upon markets?

MC: It took a while for me to recognize it, but I can now see the degree to which my concept of markets is tied to processes of innovation. I fell into the topic when I arrived at the CSI. Lucien Karpik had received a significant amount of funding for a unique and ambitious project on the politics of large tech enterprises. He would cover the full spectrum of the firms' activities: marketing, production, research and development (R&D), and finances, with emphasis on strategies for product innovation. For this project I chose to focus on innovation strategies. The topic was quite new, especially because at that time, most of the work was devoted to process innovations; product innovation was not of any interest in France. Later, in 1974, when I began to follow, in real time, a large-scale national program to design and develop a model of an electric car that could one day replace thermal vehicles, I got the feeling that others considered my project a bit exotic. I realized that no one discipline was in a position to grasp all dimensions of this type of innovation. Product innovation touched equally upon the organization of society, industrial dynamics, and social movements, as well as on the mass mobilization of science and technology.

Sociology offered little support. Analyses of consumption were only interested in the objects consumed insofar as these could unveil the contours of social structures. I would find what I needed in the work of Alain Touraine. The kind of sociology he proposed in *The Self Production of Society* (Touraine, 1973) was just right for me. He furnished the perspective I needed by insisting that social movements are not necessarily grassroots, but can be driven by the managerial class for whom innovation plays an essential role.

Touraine's interest in economics was almost nil. Karl Polanyi was the only reference he allowed himself. Yet, unlike the usual sociological studies of consumption, I soon realized the form of analysis he offered was perfectly compatible with numerous works on innovation that

were cropping up across the English-speaking world. In 1962, the NBER (National Bureau of Economic Research) published a reference book. In the preface, Richard Nelson offered a select bibliography of historical texts on industrial innovation featuring names like Jacob Schmookler, Edwin Mansfield, and Bela Gold. I threw myself into this literature, which was not breathtaking at a theoretic level but was essential for understanding the innovation process in industry. Science and technology were at the center of these analyses. The work discussed Schumpeter's ideas, renewing the analysis of long cycles and the role played by technology in different sectors. It inquired into the significance and role of competition, the respective roles of supply and demand, and the contribution of research to well-being; fleshed out a series of tools, measures, and indicators (such as bibliometrics, patent analyses, citation of scientific articles in patents, etc.) pointing out their limitations as it put them in place; and imported conceptual tools like the notion of a program of research, which Imre Lakatos put forward to reconcile Thomas Kuhn and Karl Popper.

I might have left it at that. I could have built a bridge between French sociology of social movements and Anglo-Saxon economics of (product) innovation to show how markets contribute to social dynamics and vice versa. The leading classes were establishing their legitimacy by mobilizing R&D and innovation for the sole purpose of satisfying consumers. But I was not fully satisfied; I didn't think this was enough. There were precise mechanisms being performed to make largely unpredictable adjustments between supply and demand. I wanted to trace these mechanisms. It is one thing to say that innovation is the heart of (capitalist) politico-economic machinery; it is another to understand why certain innovations find their public! The concept of translation was the answer to the question I was asking myself. Thanks to translation, I was able to retrace the complicated chains that linked different social worlds: markets, consumption, science and technology, political issues, the organization of society.

The notion of translation did not fall out of thin air. It was being carried in the wind. Economist Edwin Mansfield used it explicitly to explain why there was an almost instantaneous relationship between the laboratory and the market in chemistry, whereas in physics the connection took more time, requiring additional mediation and investment. It just so happened, *felix fatum*, that I was taking Michel Serres' courses on Leibniz, where he was elaborating various notions, notably those of interference and translation. Kuhn had used the same term of translation to explain how equivalences could be postulated between different scientific paradigms. And then, worth mentioning, there were the debates around the thesis of the indeterminacy of translation. The word was a natural choice for those who were trying to analyze the circulation and transformation of statements and things between different social worlds, and especially between laboratories and markets. Put in a nutshell, the notion of translation made it possible to re-embed the processes of market innovation in society. At the same time, it offered an answer to the more general question of the relationship between science, technology, and society.

At the beginning of the 1970s there was a flurry of work devoted to science and its ties with the social environment. But the vast majority of these works did not provide any precise answers to the questions I was asking. Sociology of science turned around conflicting norms, organizational adjustments, or professional claims-making. The analysis focused on the scientists and said nothing about what they were researching. In the hands of Robert Merton (1973) this approach proved its fruitfulness by bringing to light institutional mechanisms that had not previously been observed. In the hands of his less inspired disciples who frequently made do with rudimentary bibliometric tools, the model turned out to be quite sterile.

The sociology of science ran up against its limits when it was applied to industrial research.

Historians had not yet been seized by the topic, even if some like David Landes treated it indirectly. Researchers like me, disappointed by the sociology of science because we did not want to drain away the interesting part of sciences, that is, their actual content, turned to the history of science with hopes of finding answers to our questions. I discovered an entire continent by reading the books Alexandre Koyré devoted to Galileo and Newton and to what he called the scientific revolution. Because these topics were of the moment, I placed one foot in front of the other, following debates that pitted internalists against externalists, those historians who were interested in the content of science without regard for social context and those who strove to put content and context in conversation.

The detours I took confirmed to me that studying the role of innovation in markets and studying the relationships between science, technology, and society required the same theoretical tools. The problem to be solved was identical: through what mechanisms do scientific knowledge and technical artifacts contribute to manufacturing the social fabric? Although I started out with the idea that sociology of science and history of science would provide me with theoretical resources to solve the enigma of market innovation — how to explain the quite unpredictable matches between supply and demand, between what technoscience allows and what consumers desire — I ended up concluding something more. The concept of translation led me to consider innovation as a privileged site for accessing the analysis of a more general mechanism. In asserting that the strength of markets lay in their ability to design goods and services that allowed for a successful matching of supply and demand through gradual adjustments, I established a link with other currents of analysis that emphasized the creative dynamics of markets (such as evolutionary economics); I acquired the tools to trace and analyze these dynamics; and I was able to re-inscribe market dynamics in social dynamics. But I also came to the conclusion that the study of innovation mechanisms could get us out of the sterile opposition between internalism and externalism and would allow us to get rid of the conceptual divide between content and context once and for all. It felt like killing two birds with one stone! In addition, innovation offered an unparalleled entry point for those who wished to delve into the intricate entanglements of people with things. When innovation was treated as a nexus of ties, it became the premier terrain for studying networks of translation as they intersected in real time.

The trouble was that the word 'innovation' did not garner any enthusiasm in academic circles. It was judged too vague and too ideological just like the word 'change' being celebrated by then French president Valéry Giscard d'Estaing. It furthermore had the flaw of sounding like a hymn in praise of technological progress, which had already become the subject of virulent denunciation. While the technocracy was discovering innovation and glorifying it, others, like Ivan Illich, attracted incredible media attention by stigmatizing its nefarious effects. I wasn't worried! Personally, I could not have dreamed of a better situation. Not only did innovation delineate a space where economy, techno-science, politics, and ethics were intertwined, but it had the immense advantage of being polysemic and ambiguous. It was both desirable and detestable, it opened up innumerable terrains, and it revitalized established approaches to the functioning of markets.

AM: It follows from this period that you reflected upon the notion of translation to figure out the kinds of quantitative methods that might be used in studies. Could you say a few words about co-word analysis, which constitutes an important part of your work?

MC: Throughout the 1990s we devoted a great deal of time to quantitative methods at the CSI. The impetus came from theoretical reflections on the links between scientific research and

industrial innovation. We were searching for tools that would allow us to reconstruct networks of translation, analyze their architecture, and follow the evolution of relationships being woven between heterogeneous problems, be they scientific, political, technical, or economic. A small team comprised most notably of Jean-Pierre Courtial and William Turner was harnessed for this purpose, occasionally joined by Arie Rip and John Law.

By trial and error, we decided to begin by analyzing the content of texts related to the subject under study. For example, if we wanted to reconstruct translation networks that had progressively formed around putting electric vehicles on the road, through successive iterations we would gather together the documents that dealt with the different problems (be they scientific, organizational, or other) that needed to be solved to reach that objective. The constitution of such a database and the definition of its boundaries was not obvious in pre-digital times, but we were convinced that these difficulties would diminish with time and with the development of computing. Once this decision was made, we opted for a method of textual analysis.

For reasons explained in our 1983 publication we turned to the notion of co-occurring words (Callon et al., 1983). Contrary to multifactorial analysis, hierarchical classification tools, and other network analyses that were popular at the time, this method offered metrics specifically adapted to the notion of translation. Of course, focusing on words became the object of virulent critique. Could we not understand the absurdity of reducing a text to its words? How could we pretend to capture the description of problems by the proximity or co-occurrence of words? Google has long since shown that that our hypothesis was sound, but in that period Google did not exist! Confronted by an army of linguists leveling objections at our procedure, we needed some scientific backing. In a book about pragmatism I discovered Charles Morris, an author little known in France, a student of Margaret Mead. His semiotics emphasized the impossibility of separating semantic rules from practical rules. In justifying the fact that reducing a text to the combination of its words did not deprive it of meaning, he legitimized the choice of co-occurrences. I need no further reassurances. Let the linguists critique the respectable Charles Morris, colleague of John Dewey, before coming after us!

We named our method *co-word analysis*. While being aware of its limits and imperfections, we decided to apply it to a subfield of macromolecular chemistry. Several years prior, the *Délégation Générale à la Recherche Scientifique et Technique* (DGRST) had launched a concerted action to reinforce ties between labs and companies in this field.[1] An entire corpus of texts describing the context and the content of this program was available in hard copy: projects and researchers funded by the DGRST, scientific articles and patents describing the work carried out in the field. With the help of Françoise Laville, a skillful chemist, we enriched our method and developed tools that permitted us to follow and to characterize how networks of translation evolved (Callon et al., 1991). The method allowed us to determine if, and how, DGRST's funding had transformed and reconfigured the dynamics of innovation. It also offered an elegant solution to what seemed like an impossible reconciliation between internalism and externalism: indeed the content of scientific research could not be separated from networks of relationships that sustained it. The different social worlds taking part in this dynamic were linked together.

In developing *co-word analysis* we were effectively contributing to the development of a little known subfield called *scientometrics*, founded by Derek Solla Price in a book that has since become a classic (De Solla Price, 1963). However, the uncontested chief practitioner was one Eugene Garfield quite simply because he founded a society that digitally polled scientific publi-

1. The DGRST was a public agency whose mission was to stimulate cooperation between academic research and industry.

cations across the world (name, author, citations, etc.).[2] Garfield's stroke of ingenuity was that he understood citations were significant before anyone else and that it would be meaningful to track them systematically. In that period in France, scientific organizations were happy to index articles without thinking about citations, while the journals sat dormant in the cupboard waiting to be photocopied! Our method seemed promising because it allowed us to identify strategic themes that were likely to develop in the future and consequently offered the possibility of providing assistance, not only to researchers, but also to science policy makers. The person to win over was therefore the enterprising Eugene Garfield. So we headed to Philadelphia to present our work. We met him during a small party organized by a colleague. The man was all business, his judgment toppling upon us like an ax: "Your method is interesting but too difficult to lead to commercial applications." Having spoken these few words, more than enough in his view, he returned to munching on cold roast beef with mayonnaise and guzzling his wine without bothering to speak to us again.

After the failure of our American campaign, we returned to France and started lobbying like mad. First, we published articles in popular magazines presenting the main bibliometric tools that were available and giving examples of their potential application to the French system of research and innovation. Within a couple of weeks we wrote a *Que sais-je?,* a small textbook, presenting the most important bibliometric methods, including co-citation and co-word analysis, which could be used as complementary tools for mapping science and technology.[3] Our idea was to show the potential of these instruments, emphasizing the merits of relational analyses, and in particular, of the co-word method, as well as the necessity of building a solid information infrastructure to support the execution of these studies. We were also given the opportunity to participate in a government mission to establish an observatory of science and technology (Observatoire des Sciences et des Techniques, or OST). Our efforts to create the observatory were successful, but our attempt to impose relational scientometrics as the privileged methodology ultimately failed. The method won over neither researchers in STS nor science administrators.[4] Garfield got it right: too complicated! The OST would settle for counting publications and sociologists of science would become specialized in the use of traditional bibliometric indicators and the well-worn critiques of their use.

Until recently, when facing these relative failures, I had the impression that I had wasted my time and that of my colleagues. Today, I am partly reassured. More than thirty years after their publication, references to these methods appear in many projects, articles, and books. Not only are they being applied, they are also being constantly enhanced. The development of large databases and their digitization is a partial explanation of this growth. But I am also convinced of the enduring relevance of the original questions the method sought to answer: analyzing the dynamics of the translation operations that underlie problem networks.

AM: As we can see through the events you invoke, while becoming interested in science, you were also in dialogue with the work of economists from quite an early stage.

MC: Economists in the Anglo-Saxon world had carried out extensive work, both theoretical and empirical, on the ways in which scientific research contributes to economic innovation. Explicitly or implicitly, the analyses were based on the hypothesis that scientific knowledge had

2. The ISI (Institute for Scientific Information), which is a forerunner of the Web of Science.

3. Henri Small is the inventor of a very clever method, called co-citation, which gives a visual representation of the intellectual structure of any scientific research field. This tool was later developed in association with co-word analysis by Dutch colleagues who applied these methods to research evaluation.

4. On the unfortunate divorce between STS and scientometrics see Bowker (2020) and Cambrosio et al. (2020).

the special status of what they called a public good. Nelson and Arrow crafted the canonic formulation: scientific knowledge is non-rival and non-excludable. This implies that everyone can appropriate scientific findings without diminishing their utility. And this is why, for reasons of economic efficiency, basic research must not be subjected to market mechanisms. Mertonian sociology of science supported this analysis. It showed that over time and outside of any economic calculation, scientists conceived of norms that made it possible to preserve the status of science as a public good. The norms encouraged researchers to produce new knowledge and ensured its evaluation (Callon, 1994a). In short, neoclassical economics and the sociology of scientific institutions mutually endorsed the same vision of science. As many authors have shown, this vision requires that scientific knowledge be reduced to reliable information that, in accordance with standard economic theory, permitted agents to make rational decisions (Foray, 2004).

Nevertheless, Mertonian sociology, which had so seduced standard economics, was progressively showing its limitations. Merton himself recognized these limits as early as 1982 when he gave a speech at one of the first 4S conferences as the recipient of the J.D. Bernal Prize. He'd just read *Laboratory Life* (Latour & Woolgar, 1979), which he'd much appreciated, and likely also the works by Karin Knorr and Mike Lynch. Merton suggested a new name be assigned to this new genre of studies entirely devoted to scientific practices in place of institutions. From the audience, Harry Collins impishly called out, "scientology"! Laughter rang out across the auditorium, but even so, Merton couldn't stop himself from reacting. In all seriousness, he protested.

Merton's eye for other approaches was not shared by his followers whose main objective was still to show that science was an institution like any other.[5] For their part, mainstream economists uncritically adopted a Mertonian conception of science and even converted themselves, becoming its most ardent advocates. The 1994 article in *Research Policy* by Paul David and Partha Dasgupta perfectly illustrates the long lasting alliance between sociology and economics, and the explicit claim of disinterestedness in scientific content. I had seen a draft of the article in 1993 when I was a visiting fellow at the Princeton Institute for Advanced Studies. I had just completed a survey of the sociology of innovation and of the economics of innovation which allowed me to ascertain how little each discipline knew about the other. The draft made me understand that rebutting the hypothesis of science as a public good, the hypothesis tightly shared by neoclassical economics and Mertonian sociology of science, was a priority. I hunkered down to demonstrate that non-excludability and non-rivalry were never qualities attached to scientific knowledge as such, especially when it was emerging and seeking acceptance (Callon, 1994b). My main argument was that to understand how the free circulation, but also the free duplication and sharing of knowledge are possible, we must take into account the construction, maintenance, and extension of specific socio-technical infrastructures. I did everything I could to make this perspective compelling to economists. I accepted every invitation to present my work, and I even wrote a second paper on the topic (Callon, 2002). Good grief! Economists, especially those who came sold on the idea that science was a public good, would not budge. They simply ignored me. My first encounter was a fiasco.

I would find happier conversations with economists later on. David and Desgupta's article contributed to the development of a new field, the economics of science (Stephan, 1996). Dominique Foray and Dominique Guellec were excellent French specialists. They were also very close colleagues. They generously gave their advice and comments whenever I ventured

5. Martha Poon, personal communication.

into the territory of economics. They often disagreed with me but never refused to converse. Foray and I edited a special issue of a French journal called the *Journal of Industrial Economics*, devoted to the economics of science (Callon & Foray, 1997). In the issue's preface each of us expressed his point of view. I also managed to write an article with Guellec that presented an alternative to the Mertonian hypotheses. The journal's editor-in-chief rejected the piece on the grounds that it contributed nothing new. Foray and I immediately resigned from the editorial committee in protest.

I can count on one hand the collaborations I've since had with economists: an edited volume on the concept of network with Patrick Cohendet, Dominique Foray, and François Eymard-Duvernay (Foray et al., 1999), and a recent article cosigned by Alvin Roth on the role of economics in formatting the economy (Callon & Roth, 2021). I enjoyed working with these colleagues because they were open and respectful, and I tried to be too. They understood my efforts to be in dialogue with their discipline. Their attitude was in stark contrast with the majority of their colleagues who were arrogant, bordering on dismissive. Every time I've had to share a flight with economists someone has snarked, "Michel, I hope the plane you're boarding isn't a social construction!" Economics is by no means a dismal science, but too often economists make it one.

Through these encounters, so filled with incomprehension, it hit me that the very definition of economics was in play. There were so many things to question that went beyond pounding on *homo oeconomicus*, his unrealism or his vices and virtues. For instance, in contrast to evolutionary economics, mainstream economics was entirely based on an unrealistic definition of goods that was strikingly limited when applied to scientific knowledge. In asserting that scientific knowledge was intrinsically (by nature) non-rival and non-excludable, mainstream economists implicitly recognized that they were completely uninterested in the associated milieu of goods, that is to say in everything that gives them the capacity to be useful and consequently to be used. Economists did not realize that without an associated milieu a good is not a good. A scientific statement airlifted over the Gobi desert has no other fate than to dissipate into the sands because it is deprived of the socio-technical environment that gives it meaning and utility. Likewise, without the infrastructure that allows it to take off, navigate, and land, without the fuel supply contracts, control towers, and air traffic controllers, without the insurance companies, international regulations, and the legal agreements, an Airbus 380 remains grounded. A Nespresso capsule in the palm of George Clooney's hand, without its dedicated machine or a supply of running water, is as useless as a car on an uninhabited island lost in the middle of the Pacific Ocean. A thing is not born a good, it becomes one; a thing is not born a public good, it must become one too.

An associated milieu can be the outcome of evolution: air is a good on earth because life developed in relation to it; it is a public good because living organisms developed organs that support the continuity of life (lungs, bronchioles, etc.), and because oxygen is available in large quantities. In other cases, an associated milieu and a good become complementary through the mediation of shared infrastructure and built environment. Without receptors to capture and decode them, public radio waves are lost in space and lose their use value. But certain situations, that are becoming more frequent in today's world, are blurring these boundaries. Emissions from the factories that produce aircraft do not prevent planes from flying, but air pollution can make the areas surrounding these plants unlivable. As associated milieus come into conflict, goods that have become public might quickly turn into public bads.

Permit me to add another observation. Curiously, the entire critique by sociology, political science, and anthropology has fixated on economics' conception of agents. The obsession

has brought about important modifications like the introduction of various forms of limited rationality or network effects. But the analysis of goods has remained weakly evolved. Indeed, goods continue to be conceived of as isolated entities whose material and relational qualities are of little importance.[6] In associating goods to the techno-economic networks that allowed them to circulate, I have sought to provide deeper explanations of their economic properties. The qualities of goods should not be considered as intrinsic attributes, but as the evolving outcome of relational adjustments. As long as I continue to hear that science is a public good, as long as economists and sociologists invoke non-rivalry and non-excludability without asking about the processes that makes these properties emerge, I will remain convinced that there is vastly more research left to do than what has been done!

AM: The edited volume *The Laws of The Market* (Callon, 1998) was an important step in the articulation of the different faces of your reflections on innovation and markets. It also crystalized a line of inquiry into the performativity of the economy. Could you recall this moment for us?

MC: While working on innovation, I was struck by mainstream economists' role in shaping and developing the categories used by governments to frame and measure the innovation process. They were actively involved in clarifying and legitimating the radical distinction between invention (discovery) and innovation (commercialization), which was the basis of intellectual property law, in particular patent law. Economists transformed a slogan, "Science discovers and industry applies," into a scientific truth. These same economists had also constructed an apparatus of surveys and statistics entirely based on the hypothesis of science as a public good (e.g. Frascati manual — OECD, 2015 — and later the Oslo Manual — OECD/Eurostat, 2018). The affirmation that science is a public good is obviously neither true nor false (because it depends on the state of translation networks), but this does not prevent the affirmation from producing tangible effects. The more relevant question was: how does the concept of public good contribute to structuring the actual innovation process by pushing to draw a clear line of demarcation between science and markets? To what extent and through what mediations does the concept do this work? I was simply reviving an earlier proposal that Latour and I had made about sociology (Callon & Latour, 1981; see also Law & Urry, 2004). There was no reason not to include economics, considered in all its diversity, in a more general reflection on the performative character of sciences and techniques.

I had to admit, however, that I was sorely lacking in empirical proof. The solution was an edited volume (Callon, 1998a). But with whom? Authors prepared to defend the thesis were not banging at my door. Crafting the introduction was a delicate balancing act. Without Donald Mackenzie, who took the thesis seriously, the book would probably have disappeared. *The Laws of the Market* was like a flying saucer crashing down unexpectedly on the manicured lawn of economic sociology. The book managed to elicit a certain amount of curiosity because the performativity program, as it came to be known, offered an escape from the cold war that had deadlocked sociologists and economists for decades. With performativity the game was no longer to argue with economists, but to evaluate the effects they produced.

One of the first critiques leveled at the introduction to the volume took aim at the privileged position I conceded to neoclassical economics. I'd chosen this economic model because every one knows of it, or thinks they do. All scholars have heard of *homo oeconomicus*, a creature whose sole reason for existing is to maximize profits and satisfaction. And just about everyone

6. The only significant exception is the work done by evolutionary economics on irreversibility

is wondering whether the creature truly exists. I wanted to show that this extremely traditional epistemological issue, which people like Milton Friedman tried to resolve, was of limited interest: from the very beginning, empirical research had shown that this supposedly mythical being did exist, but could only live and prosper with the aid of technical and cognitive prosthesis and equipment. This became obvious to me when I read Marie-France Garcia's (1986) article in the French journal *Actes de la Recherche en Sciences Sociales*. Her description of strawberries auction markets showed how a well-conceived material environment, supported by rules, legal apparatuses, and compelling justifications, could transform both small produce farmers from Sologne and their counterparts, the grocers, perfect *homo oeconomicus*. Garcia's own analysis was weakly convincing because she'd relied upon Bourdieu's idea of a 'theoretical effect', which severely underestimates the importance of material devices in processes of economization. In contrast, I wanted to highlight the role of the material devices that she so clearly captured in her writing. The example was all the more convincing because it showed ordinary working people, whose existence was ignored and sometimes disparaged in swaths of economic sociology and anthropology, metamorphosing into *homo oeconomicus*.

I considered neo-classical economics an exemplary case because of its outsized influence. Indeed, an equivalent demonstration applies to all currents of economics: evolutionary economics had considerable impact on innovation and public policy supporting innovation; behavioral economics now dominates; and neuro-economics, still fledgling, will soon produce some impactful outcomes. Performativity would proceed even more fluidly were we to acknowledge that doing it is one of the explicit purposes of these kinds of specialties.

As I have had occasion to point out on several occasions, when I speak of economics I do not limit myself to the academic definition of this discipline. In my view, economics includes all the organized and equipped knowledge or know-how that contributes to what Koray Caliskan and I called the economization process. This includes the obvious disciplines, such as accounting, marketing, and the management sciences, that for decades have been designing categories and tools that contribute to the functioning of the economy. With the growing grip of platforms and big tech companies on economic activities, but also with the concurrent rise of ecological concerns, yet other knowledges and technologies will also participate powerfully in renewing processes of economization. Just consider how computer science and mathematics are being applied to the design of algorithms or to the formulation of prices; or how the laws of physics and chemistry are currently mobilized, following the pioneer example of scholars like Nicholas Georgescu-Roegen, to elaborate macroeconomic models intended to better manage limited resources. These examples are still emerging, but they will soon become dominant. And in order to understand them, and possibly interfere with them, the notion of performation or performation struggles is inescapable.

No matter what, socio-technical devices matter. In their absence, economic agents and economic analysts are disabled. Alongside Donald Mackenzie (2009), this approach led me to take the materiality of markets into account and to introduce the notion of a market agencement.

AM: You developed the notion of market agencement in *Markets in the Making*, which appeared in translation in 2021. The subtitle refers to 'innovation', but also to 'competition' and 'goods'. All of this reflects the uniqueness of the economic sociology you practice, which is that you've never rejected the discipline of economics. You take up its concepts but give them a new definition. Yet doesn't this position carry the risk of confining your reflections to the very terrain the economists have already staked out?

MC: The first chapter of *Markets in the Making* was entitled "What is a market?" (Cal-

lon, 2021). Of course, there was no obvious answer since the term is so polysemic. Specialists will sometimes propose definitions, but these will vary depending on the analytic framework they're using. Most often the word is used as if it doesn't need to be defined, and many use it interchangeably to mean 'capitalism'. It was not my objective to put forward a more true or realist definition as if there is a form of a 'market' that must be discovered. Nor did I want to enrich the typology. Staying true to my ways of doing scientific research, I departed from the formulation of a problem rather than from a quest for a definition: how can we describe and analyze the processes that lead to the establishment of a singular commercial transaction? The transaction in this case is defined as a permutation of property rights through some medium of monetary payment. Answering this question also answers a question that has been haunting the social sciences: how can we explain the emergence and satisfaction of what are commonly called needs? The notion of a market agencement is not a new way of conceiving of or defining markets. It seeks to bring together the elements of an answer to a question the concept of markets carefully sidesteps. Yet it would be silly and vain to eliminate the term markets from our vocabulary when it is used by just about everyone. The term is irreplaceable. Moreover, its multiple meanings should be preserved because they are a potential source of new ideas.

An analysis of market agencements and how they work is not dependent on economics or its models. No field has a monopoly on terms like good, innovation, competition, price, supply, or demand. They are part of the worlds that each and every person inhabits. Through market agencements their meaning is profoundly reshaped without nullifying their importance, which is what maintains the link with common experience. My way of conceiving of market activities does not separate us from the world; it draws us closer to it. This is why I had to multiply the available references to detailed case studies. For me, the indicator of success comes from this movement, from diving into practices instead of taking distance and privileging abstraction. I could not have made it to the end of the endeavor without the ethnographic work and the conceptual tools forged therein. The people I've had the pleasure and luck of working with are dear to me. They include Madeleine Akrich, Franck Cochoy, Sophie Dubuisson, Antoine Hennion, Hans Kjellberg, John Law, Alexandre Mallard, Fabian Muniesa, Vololona Rabeharisoa, and Geneviève Teil.

The approach I've proposed does not simply redefine some of the central notions of economic theory in depth, and it incites us to abandon several of them. One good example is the notion of 'market exchange,' which is so commonly employed despite its absurdity. I've made no attempt to trace the origins and genealogy of this strange expression. But what we can see is that the term participates in making the practice of exchange the cornerstone of social life. Humans must exchange to survive and the market is simply considered as the most advanced answer to this universal necessity. Adam Smith defended this very thesis in *The Wealth of Nations* (1776). Like many of his colleagues, Alfred Marshall saw money as a tool for managing a complex network of transactions such that market activities merely prolonged the exchange of goods in a more sophisticated form. Most recently, in the rather successful book, *Reinventing The Bazaar*, economist John McMillan (2002) deploys reasoning that would have markets and their spread emerge directly from the social nature of human beings. Because of this supposed anthropological necessity, it seems completely natural to speak of market exchange. Once this step is taken, all that is left is for anthropologists, treading in the footsteps of the conquistadors that preceded them, is to rediscover forms of exchange that differ from the ones to which they are accustomed. The alternatives get labeled 'non-market'. A binary that encompasses all cases but explains nothing is in working order. Yet who would dare pretend that when I buy a bottle of olive oil from the grocery store in my village I'm engaging in exchange? No, I pay, I buy, I

acquire, but I do not exchange. That would be like saying that theft and pillage are forms of exchange. I have to concede, we can say this, but at the cost of swimming in total confusion.

So-called *market failures* are another example of how we might radically redefine a central concept for mainstream economics. Externalities and their production, positive or negative, have been a topic of sustained attention for over a century. Now, because of climate change, everyone speaks of externalities. But this concept is tightly linked to an unrealistic vision of market activities. Externalities are not market failures. Quite the contrary, without externalities a market cannot function. This necessity is the reason why I introduced the notion of framing-overflowing which considered the externalities studied by economists as a specific case of a more general mechanism (Callon, 1998b). All action is framed, formatted by socio-technical devices, that assemble and coordinate heterogeneous elements such as contracts, machines, legal texts, rules governing the circulation of information (patents, commercial secrets, copyright, etc.), inscriptions, embodied knowledge, and so on. I have tried to show that these devices frame, and simultaneously with framing, structure overflows. Some overflows are visible and even predictable; others go unnoticed because their very existence is not well documented or simply because instruments for detection and measurement are lacking. To spot overflowing in market transactions, the best strategy is to scrutinize the five framings I presented in *Markets in the Making*. These framings are the main source of overflows, of matters of concern, of which I gave examples in passing. Nothing and no one can mop up all the problems and issues created, or stop overflowing once and for all. Taking one matter of concern in hand leads to the implementation of new framings, which in turn are sources of more overflowing. *No markets without failures*. Markets work *because* they fail.

The concept of a market agencement does not enrich the notion of a market as conceived of and demarcated by economics. It does something quite different. It proposes a new approach to the organization of market activities permitting us to understand how human lives are intertwined with goods, while bringing new meaning to the categories we use to describe the economy in which we live such as competition, innovation, consumption, production, or needs.

AM: Schumpeter's "creative destruction" is another interesting, potentially provocative economic concept for analyzing innovation and markets. By making the positive aspect of creativity a companion to destruction it appears to justify the violence of innovation. This is a pressing issue as innovation proliferates in our own times. From the perspective you've been developing, what interpretation of Schumpeter's message would you give?

MC: Schumpeter is an unavoidable author. At the very beginning of my career I read *Capitalism, Socialism and Democracy* (1942) as well as *Imperialism and Social Classes* (1989). Yet is was my much later reading of *The Economics of Industrial Innovation* by Christopher Freeman (1982) that made me conscious of the importance of Schumpeter's more technical pieces and of so-called evolutionary economics. Freeman gave a clear and elegant presentation of Schumpeter's theses, and also convincingly reworked Kondratieff's long cycles. There was a real craze at the time for long cycles and their explanatory power. Among others, works like Gerhard Mensch's (1975) *Stalemate in Technology* proposed impressive frescoes that drew together scientific progress, technological innovations, profits, and inflation. I guess Freeman wasn't convinced by these macroeconomic models even though he emphasized the future importance of information technology and biotechnology. He challenged the mechanistic aspect of Schumpeter's theses and the sequence they implied: phases of innovation, followed by periods of imitation, and then ruinous competition. These assertions were not supported by the

more detailed analyses Freeman conducted at the OECD.

At this point let me make a comment. Today, sociologists and political scientists talk a lot about imaginaries and anticipations, but without making any substantial reference to these works. Yet they had the immense advantage of giving a place to notions such as irreversibility or lock-in. Social sciences, especially when they are focusing on macrostructures, are not really equipped to analyze the long-term structuring effects of technology. They struggle to analyze the processes by which sociotechnical assemblages are formed and evolve; they tend to revive the division between the technosciences and the social (or institutions) while neglecting technical contents. I remember a colloquium organized by the STS program of the CNRS (French National Centre for Scientific Research), attended by a handful of reputable sociologists and philosophers. In that period, we had started to speak of biotechnologies and their impact. Most of the celebrities in the room joined their voices, to argue that all of these claims were empty promises by scientists who thought technology ran the world and that the speculative bubble would soon implode. Isabelle Stengers, who was obviously following the story, gave a presentation that sent a chill over the room: "In twenty years everyone will be doing biotechnology and you'll be blindsided." She was not mistaken. Yet she found herself on the defensive because of her supposed technophilia. Here is the golden rule the other scholars wished to impose on us: "Scientists have the right to speak as long as they remain within their expertise. Social scientists will take care of all the rest, impact, representations, public acceptance, but without getting involved in the content." The purpose of this intellectual and political Yalta was to erect a wall between two empires: on one side content, on the other concerns.

I'm recalling this situation because concepts like 'the gale of creative destruction' caricature the events associated with the emergence of new technologies. They also underestimate the gradual progression and reconfigurations that underlies processes of diffusion, as well as the multiple adjustments and hybridizations these entail. Only after the fact, and seen from the point view of Sirius, does it become possible to speak of rupture and deploy a vocabulary that invokes a state of war. Destruction is considered as the price paid for reconstruction. When the shells rain down, reducing a city to ruins, we imagine the investors and public works entrepreneurs rubbing their hands in gleeful anticipation of the business about to come their way. This simplistic vision is deceptive. It leads us to believe that everything changes in a single blow. To the contrary, building and reshaping infrastructure take time, a lot of time. The new gets mixed with legacy structures to create unanticipated forms.

An example... While everyone carries on about digital society and economy, platform economies, and central bank money, the little mole rat called biotechnology, with its many faces, diligently digs its underground passages, changing the landscape as it stirs up the soil. The transformations biotech is bringing about, the raising stakes, seem somehow secondary to the growing influence of Big Tech and climate change. Yet the mole rat's work is perfectly traceable, as traceable as China's growing hold over the surrounding seas. One day we will wake up to find entire swaths of the economy, as well as of political and moral reflection, taken over by these new biotechnologies through the economy of digital platforms to which they integrate themselves bit by bit. There's one way to avoid being surprised: forget about revolutions. Follow the mole rats in real time as they construct galleries, patch them up, branch off, and sometimes abandon their work when it ends in impasse. The ethicists seem to be the only ones concerned with the emergence and development of genetic engineering that will one day invite itself to everyone's table and impose its own menu.

The idea of creative destruction lacks finesse. It leads us to be disinterested in processes, in the discrete slow movements we could actually imagine acting upon, and that lead to the

construction of the new infrastructures we will end up living with no matter the cost. I am interested in what the mole rats are doing rather than the magicians who pull revolutions out of their hats, each rupture being more dramatic than the last, piling them up one after the other like pastry chefs laminating mille feuilles dough. If you're distrustful of these magicians, and rightly so, then read Alberto Cambrosio. For more than thirty years he's been following a mole rat called biomedicine, living with it, feeling his way alongside it, documenting the topography of the galleries it builds. I've learned never to listen to experts or scholars who claim to be less blind than mole rats, or who put on airs about their powers of prediction. The only superiority a researcher can claim is the immense power of modestly while professionally following the winding expansion of galleries and the new assemblages being produced.

Having worked extensively on long cycles, Schumpeter understood these difficulties. The invention of the character of the entrepreneur provided him with a solution that seemed realistic. He made him a kind of hero, capable of upsetting the most well-established structures thanks to his energy and ingenuity. Yet between the individual entrepreneur that he characterizes and the waves of creative destruction he believes he is observing, sit an ensemble of mediations that he did not give himself the means to see or analyze. Instead of looking at the relationships that knot together technologies, regular entrepreneurs, and economic cycles, he pursues general hypotheses he can neither confirm nor deny. I don't think there is any betrayal in concluding that he stopped midway and invites us to pursue the course.

Research consecrated to sociotechnical lock-in and lock-out, as well as to entrepreneurship and techno-economic networks, allows us to advance our knowledge of these complex processes and the diverse trajectories they form (Garud & Karnoe, 2012). Without recourse to these analytic tools there can be no progression in our understanding of transitions. The concept of irreversibility is central in the description of these mechanisms. We should read and reread Schumpeter, particularly his monumental history of economic thought. But we should force ourselves to forget the idea of creative destruction because it not only obscures the dynamic of innovation networks, it also disappears the mobilization of non-humans and financial circuits. What I said about Marx applies equally to Schumpeter; it's by dismantling the work that we learn the most from it.

AM: The definition of capitalism is another, ever-burning question. It bears little interest to you, except, perhaps, in the 1997 article co-authored with Bruno Latour called "Thou shall not calculate!". Today's debates are about platform capitalism. Do they at all inspire you?

MC: I must admit that I'm not particularly moved by the question of finding a general definition of capitalism or making distinctions between the forms it can take (monopolistic, state, digital, cognitive, German, Chinese...). Must we speak of capitalism before the industrial revolution? Can we distinguish between different varieties of capitalism? If yes, what criteria should we use? Should we locate differences in structures, human behaviors, institutions, or all three at once? These questions are primarily interesting in the hands of historians who exhume some fascinating materials. The most important findings give evidence of a variety of managerial tools, equipment, accounting techniques, organizational forms and legal statuses of businesses, forms of intellectual property, and land title.

As a general rule I think we need to be wary of abstract categories. Using surgical tweezers, we should prudently handle expressions like "capitalism allows progress while fully respecting individual liberty," or the opposite, "capitalism will kill the planet"; likewise, "capitalism spurs the creativity of entrepreneurs," or the opposite, "capitalism ignites the pursuit of individual profit"; and this one, "capitalism is the accumulation of capital," no, "capitalism is creative

innovation." When I apply myself to understanding how market devices function I am confronted by the interlacing of identities and relationships these macro statements shamelessly paper over. It is the intricacies that allow us to understand what is going on and to grasp the evolutionary processes at play.

The enigma of capitalism echoes, in many ways, the enigma of the gift. Both concepts are obscure and polysemic. The usual procedure of pitting capitalism against gift giving sews confusion, engendering or inspiring a series of distinctions that hinder thought and condemn us to binarism and all the opposites that feed it: market exchange vs. non-market exchange, interests vs. disinterestedness, reciprocity vs. individualism (Guyer, 2004). Mauss doubtlessly bears some responsibility for the rise of binarisms in economic anthropology, but he's not the only guilty party. I have read and reread *The Gift* discovering each time a wealth of ideas, suggestions, and elements of reflection that had previously been hidden (Mauss, 1925). Maurice Godelier (1999) perfectly expresses the same experience in the introduction to his book *The Enigma of The Gift*

> With the "Essai sur le don," I felt as thought I had suddenly emerged onto the bank of an immense tranquil river bearing along a mass of facts and customs plucked from a multitude of societies, stretching from the Pacific islands to India, from British Columbia to China, and springing from the most varied epochs, from archaic Roman antiquity to the present that Mauss knew, that of Boas' Kwakiult fieldwork before the First World War, or Malinowski's stay with the Trobrianders during it. (p. 6)

Mauss would probably have been surprised had he read Claude Levi-Strauss's introduction to the French edition of his essay. Levi-Strauss considerably weakened Mauss's text, and in my view imposed an overly simplistic reading. Instead of contrasting gifts and market transactions in multiple ways, his commentary stomped out nuance in favor of a rather obscure, universal, and general category of exchange. I mentioned earlier that I think the notion of a market exchange should be abandoned. Unfortunately, for Levi-Strauss, as for many social scientists including economists, it is the fundamental notion.

The force of Mauss' essay is that it connects phenomenon that others are committed to separating. It is free from spheres, sub-systems, and other fields that inconveniently enclose sociological and anthropological analyses into conceptual frames and binary logics. It allows us to become more attentive to meaningful differences. Common sense rings true. Everyone knows that it's not the same thing to donate blood in exchange for a sandwich or to place a can of stew we just purchased into the box for the Food Bank at the exit of the supermarket. All of the scholars who are interested in the practices we habitually label "donation" have documented the variety and the complexity of legal, organizational, educational apparatuses, and so on, that allow these practices to exist and become hybridized. In contrast to Levi-Strauss, an orthodox philosopher in search of a universal designated by a single word (exchange), vernacular language has multiplied the expressions that do justice to the diversity of interactions: offering, giving a gift, freeware... The same vernacular could be found for commercial transactions. Semantic richness is a treasure that should not be squandered even in the name of structural analyses.

Bruno Latour and I felt it would be more enlightening to explore similarities and differences than to obstinately repeat the same oppositions *ad nauseam*. Gifts and commercial transactions have a number of traits in common. Capitalism is often characterized by calculative practices: to maximize profits, optimize return on investment, increase dividends, and so on. We know from Mauss that what gets labeled the gift is equally marked by calculative practices, a

point underscored by the potlatch and the reciprocal gift. There are many similarities between the telethon counter that announces the accumulating pledges of TV spectators and the display at the auction market in Sologne; but there are also important differences. Calculation is omnipresent but does not capture the same reality. In our article we sought to characterize differences. Of course, there is calculation in both cases, but what differs in the two transactions is what gets rendered incalculable. The expression 'to give without counting the cost' is no more true than the expression 'good accounts make good friends'. Both statements are simply incomplete, remaining silent on what is being excluded from calculation. In capitalism, and more generally in market transactions, externalities are largely ignored. This is what allows final payment to occur: the partners separate and are quit. Strawberries change hands, the buyer and the seller go their separate ways. And all of this is only viable on condition that all of the possible overflows of the transaction are not taken into account. (Consider the effect of pesticides on health, plastic bottles polluting the environment, etc.) With gifts, closing calculations is extremely difficult or even impossible no matter how they are defined. There are many apparatuses that prevent definitive calculation from being carried out; connections remain and cannot entirely be unraveled. Our article sought to draw attention to the importance of these mechanisms and the sociotechnical apparatuses that enact them. Our approach was a contribution to liberating analysis from binaries. Gifting is neither the opposite nor the negation of market exchange. In *Markets in the Making*, I suggested a conception of the gift that avoids these crude oppositions and places the impossibility of closing calculations at the center of the analysis.

I'm sad to say that in spite of all the wonderful work demonstrating the complexity and variety of gifting practices the concept continues to be solely applied to mean the opposite of market exchange. The bad habit is in full force with the emergence of digital platforms. I think it's a mistake to assert, as some authors do, that platforms combine donations and commercial exchanges. This observation is important to me: to avoid falling back into binarism in platform studies, it is better to talk about zero prices and just speak of commercial transactions than to invoke gifts. In my own work, a concept of the platform is central to the analysis of market activity. In the entrenched model of markets as interfaces, whose weaknesses I've documented, the goods, including services, must play the role of the platform, brokering a connection between supply and demand that somehow pre-exists. But in my model of market agencement, what is now commonly called a platform is core to the organization of market encounters and the exploration of identities. In my approach, what we call a digital platform is only one incarnation, one avatar of the platform, among many others. Yet, it is undeniable that digital platforms have deeply expanded what market agencements can do through their ability to put a large number of heterogeneous actors in touch, their power to generate attachments and singularize transactions, their hold over their partners, and their financial strategies (Pais & Stark, 2020; Stark & Pais, 2020).

Let me go one step further in exploring the economic description of digital platforms. Rather than reviving the distinction between gift and market exchange, rather than arguing that personal data is given over without compensation to consumers, it is more appropriate to resort to the classical notion of the asset, or better, as a growing number of scholars have insisted upon, to the idea of assetization (Birch & Muniesa, 2020). Consider Amazon, the most well known cases of a digital platform. One of the numerous problems these platforms pose is precisely the economic status of the data they manufacture, accumulate, treat, and re-inject into their activity to singularize market transactions. These data are simultaneously an intermediary outcome and an input in a complex process that includes, among others, the

elaboration and mobilization of algorithms, logistical innovations, sophisticated strategies of attachment and of prices formulation, and so on. Most importantly, from Amazon's vantage point, data can be considered an asset for at least two reasons. First because it could feed the process of developing new services that will generate further engagement with the company and create the continuous revenue (think Amazon Prime Video for on demand streaming or Amazon Luna for gaming); second because the data might eventually be purchased for very specific reasons by economic agents having access to the digital infrastructure necessary for its exploitation.

Studying platforms makes it clear that the notion of a resource is devoid of explanatory power, and so is the notion of the production function. This point is not new. In the early 1980s, evolutionary economists and technology management specialists, pushed instead for the notion of assets and complementary assets. In their approach each agent (and in particular firms) are characterized by a singular combination of specific assets that ensures it a monopoly position. Assets have a double identity. They constitute active entities, which, combined with each other, contribute to the process of commodification and singularization of goods; but under certain conditions they are themselves susceptible to being transformed into commodities. It could undoubtedly be shown that the probability of a combination of complementary assets being valued on one or more secondary markets is linked to the capacity of these assets to feed and develop the production of a flow of goods that are the object of commercial transactions. This property is perfectly illustrated in the case of Amazon: the higher the growth of its sales of goods and services, the higher the value of its shares, as well as that of its various complementary assets (data, logistical innovations, pricing algorithms, etc.). From this perspective, it is no longer appropriate to use the notion of gift to describe the activities of digital platforms.

Assetization will give rise to extremely interesting and promising work because it is bound up with commodification. Commodification and assetization are two faces of a common process. Platforms do not introduce discontinuities in the general dynamic of market agencements. Quite the opposite: by the simple fact of enlarging the list of entities being transformed into assets, they constitute a powerful means of extending the empire of merchandise. Looking forward, the work that needs to be done is to describe the process of assetization, that is, the set of mechanisms by which combinations of entities are rendered (a) active, and able to generate identifiable, predictable and controllable effects; (b) durable and capable of being permanently reenacted; (c) appropriable and accountable. In short, we must do with assetization the same work that I did with commodification in *Markets in the Making*.

AM: The pandemic we recently endured demonstrated how urgent it is to reimagine the relationships between economy, society, science, the environment, and so on. In your opinion, what role do markets play in crises?

MC: In hectic and uncertain situations it's difficult not to resort to markets, at least in part, as a means of designing goods and services that will suit those who prove inclined to buy them. The problem, which is political, is the choice of how to organize these markets. In my view, by providing a realistic description of market activities, and in particular, by emphasizing the role of socio-technical infrastructures (framing devices, management tools, etc.), the notion of market agencements will broaden the range of possible interventions. Instead of being obsessed with hypothetical notions of efficiency and optimal allocation of resources, thinking about market agencements allows us to follow the dual movement of framing and overflowing, the dynamic of market activities.

Whatever its causes, the Covid-19 pandemic was not a straightforward consequence of com-

mercial activities. Yet there is no doubt that the global turmoil it triggered had a strong impact on the economy. Entire sectors have been severely affected, such as tourism, catering, culture, transport, real estate, and so on. Services delivered to homes by digital platforms have exploded. The organization of work has been profoundly modified. There's so much more to say. Yet none of these consequences tells us much about the specific role, if any, played by markets. For example: Did markets contribute to fracture or, to the contrary, did they help contain it?

To answer this question, we must first remind ourselves of Janet Roitman's (2013) thoughtful reflections on the notion of crisis. In her book *Anti-Crisis*, she explained why the word must be handled with care. Crisis is not the inflection point for systemic change but a moment when long processes of interrelated adjustments get set in motion. The crisis expresses itself in the crushing rise of widespread anxieties, the interventions of experts or critics of all kinds, sometimes unexpected claims, social movements, and so on. This is exactly what happened with the Covid pandemic and its long-lasting settlement. Matters of concern have exploded in the public space. Some of them were a direct challenge to the functioning of economic markets. Patent law, especially in the pharmaceutical industry, was criticized yet again; the distribution of roles between the state and private companies was yet again the subject of heated controversies; the formulation of vaccine and drug prices reinvigorated debates that had already been going on for several years; the modalities of support for entrepreneurial initiatives, especially tech startups, were also re-discussed. But we should not overlook the fact that there is almost general agreement on the effectiveness of the market institution, which has demonstrated its capacity to innovate by developing new vaccines in record time. The pandemic created an interesting situation by imposing questions, not directly created by markets as such, but to which markets could provide answers (Callon, 2009). I would not be surprised if there were intellectual property reform projects or profound changes in the organization of the pharmaceutical industry. The pandemic may well have accelerated these restructurings by giving even more weight to concerned groups, such as patients organizations, which have been trying for several years to intervene in the functioning of market agencements by taking on issues such as price fixing, start-up funding, and compulsory licensing. When faced with such turmoil, and to get the most detailed picture possible of what market contributions might be, one of the first tasks should be to map out the terrain. This means building as exhaustive as possible a table of connected matters of concern and the relationships that hold them together, while also remembering to identify the actors that carry these concerns. Claude Lefort (1981) assured us that in an uncertain world one cannot do without democracy. I would add, nor without well designed market agencements.

AM: To finish, could you tell us about the questions that interest you today?

MC: In *Markets in the Making* I insist upon the driving role played by matters of concern in the dynamic of market agencements. In Chapter 8, I suggest that it's important for researchers to work on the inventory and formulation of these concerns, as well as on clarifying the reframings they require. This kind of work could easily be done in collaboration with economic actors. It could also be extended to the dissemination and implementation of the most relevant responses. I hope to illustrate this process by studying forms of economization that are compatible with ecological concerns, and that simultaneously capitalize on the ability of market agencements to design and deliver tailored goods by closely cooperating with those for whom they are intended.

References

Birch, K., & Muniesa, F. (2020). *Turning Things into Assets in Technoscientific Capitalism*. Cambridge, MA: The MIT Press. https://doi.org/10.7551/mitpress/12075.001.0001

Bowker, G.C. (2020). Numbers or No Numbers in Science Studies. *Quantitative Science Studies*, *1*(3): 927–929. https://doi.org/10.1162/qss_a_00054

Bourdieu, P., & Passeron, J.-C. (1964). Les héritiers: les étudiants et la culture. Paris: Éditions de Minuit. https://doi.org/10.1515/9783112319161

Callon, M. (1994a). Four Models for the Dynamics of Science. In S. Jasanof, G.E. Markle, J.C. Petersen, & T. Pinch (Eds.), *Handbook of Science and Technology Studies*, (pp. 29–63). London: Sage. https://doi.org/10.4135/9781412990127.n2

Callon, M. (1994b). Is Science a Public Good?. *Science, Technology and Human Values*, *19*(4), 395–424. https://doi.org/10.1177/016224399401900401

Callon, M. (Ed.). (1998a). *The Laws of the Markets*. Oxford: Blackwell.

Callon, M. (1998b). An Essay on Framing and Overflowing: Economic Externalities Revisited by Sociology. In M. Callon (Ed.), *The Laws of the Markets* (pp. 246-269). Oxford: Blackwell. https://doi.org/10.1111/j.1467-954X.1998.tb03477.x

Callon, M. (2002). From Science as an Economic Activity to Socio-economics of Scientific Research: The Dynamics of Emergent and Consolidated Techno-economics Networks. In P. Mirowski & E. Sent (Eds.), *Science Bought and Sold: The New Economics of Science* (pp. 277–317). Chicago, IL: University of Chicago Press.

Callon, M. (2009). Civilizing Markets: Carbon Trading between in Vitro and in Vivo Experiments. *Accounting, Organizations and Society*, *34*(3-4): 535–549. https://doi.org/10.1016/j.aos.2008.04.003

Callon, M. (2021). *Markets in the Making: Rethinking Competition, Goods and Innovation* (O. Custer, Trans.). New York, NY: Zone. https://doi.org/10.2307/j.ctv1mjqvf7

Callon, M., Courtial, J.P., & Laville, F. (1991). Co-word Analysis as a Tool for Describing the Network of Interactions between Basic and Technologicl Research: The Case of Polymer Chemistry. *Scientometrics*, *22*: 155–205. https://doi.org/10.1007/BF02019280

Callon, M., Courtial, J.P., Turner, W., & Bauin, S. (1983). From Translation to Problematic Networks: An Introduction to Co-word Analysis. *Social Science Information*, *22*(2): 191–235. https://doi.org/10.1177/053901883022002003

Callon, M., & Foray, D. (1997). Nouvelle économie de la science ou socio-économie de la recherche scientifique?. *Revue d'économie industrielle*, *79*: 13–36.

Callon, M., & Latour, B. (1981). Unscrewing the Big Leviathan: How Actors Macrostructure Reality and How Sociologists Help Them to Do So. In K. Knorr & A. Cicourel (Eds.), *Advances in Social Theory and Methodology. Toward an Integration of Micro and Macro Sociologies* (pp. 277–303). London: Routledge & Kegan Paul.

Callon, M., & Latour, B. (1997). "Tu ne calculeras pas!" ou comment symétriser le don et le capital. *Revue du MAUSS*, *9*, 45–70. https://doi.org/10.4000/books.pressesmines.2024

Callon, M., & Roth, A.E. (2021). The Design and Performation of Markets: A Discussion. *AMS Review*, *11*: 219–239. https://doi.org/10.1007/s13162-021-00216-w

Cambrosio, A., Cointet, J.P., & Hannud Abdo, A. (2020). Beyond Networks: Aligning Qualitative and Computational Science Studies. *Quantitative Science Studies*, *1*(3): 1017–1024. https://doi.org/10.1162/qss_a_00055

Dasgupta, P., & David, P. (1994). Toward a New Economics of Science. *Research Policy*, *23*(5): 487–521. https://doi.org/10.1016/0048-7333(94)01002-1

De Solla Price, D.J. (1963). *Little Science, Big Science*. New York, NY: Columbia University Press. https://doi.org/10.7312/pric91844

Foray, D., Callon, M., Cohendet, P., Curien, N., Eymard-Duvernay, F. (1999). *De la coordination*. Paris: Economica.

Foray, D. (2004). *The Economics of Knowledge*. Cambridge, MA: The MIT Press. https://doi.org/10.7551/mitpress/2613.001.0001

Freeman, C. (1982). *The Economics of Industrial Innovation*. London: Frances Pinter.

Garcia, M.-F. (1986). La construction sociale d'un marché parfait. Le marché au cadran de Fontaines-en-Sologne. *Actes de la recherche en sciences sociales*, *65*(1): 2–13. https://doi.org/10.3406/arss.1986.2347

Garud, R., & Karnoe, P. (2012). Path Creation as a Process of Mindful Deviation. In R. Garud & P. Karnoe (Eds.), *Path Dependence and Creation*. New York, NY: Psychology Press. https://doi.org/10.4324/9781410600370

Godelier, M. (1999). *The Enigma of the Gift* (N. Scott, Trans.). Chicago, IL: University of Chicago Press. (Original work published 1996)

Guyer, J. (2004). *Marginal Gains. Monetary Transactions in Atlantic Africa*. Chicago, IL: University of Chicago Press.

Latour B., & Woolgar, S. (1979). *Laboratory Life. The Social Construction of Scientific Facts*. Beverly Hills, CA: Sage.

Law, J., & Urry, J. (2004). Enacting the Social. *Economy and Society*, *33*(3): 390–410. https://doi.org/10.1080/0308514042000225716

Lefort, C. (1981). *L'invention démocratique. Les limites de la domination totalitaire*. Paris: Fayard.

MacKenzie, D. (2009). *Material Markets: How Economic Agents Are Constructed*. Oxford: Oxford University Press.

MacMillan, J. (2002). *Reinventing the Bazaar – A Natural History of Markets*. New York, NY: Norton.

Marx, K. (1890). *Das Kapital. Kritik der politischen Ökonomie. Erster Band. Buch I: Der Produktionsprocess des Kapital*. Hamburg: Otto Meissner.

Mauss, M. (1925). Essai sur le don: forme et raison de l'échange dans les sociétés archaïques. *L'année sociologique*, *1*, 30–186.

Mensch, G. (1975). *Stalemate in Technology. Innovations Overcome the Depression*. Cambridge, MA: Balinger.

Merton, R.K. (1973). *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago, IL: The University of Chicago Pres.

National Bureau of Economic Research. (1962). *The Rate and Direction of Inventive Activity: Economic and Social Factors*. Princeton, NJ: Princeton University Press. https://doi.org/10.1515/9781400879762

OECD. (2015). *Frascati Manual 2015: Guidelines for Collecting and Reporting Data on Research and Experimental Development, The Measurement of Scientific, Technological and Innovation Activities*. Paris: OECD Publishing. https://doi.org/10.1787/9789264239012-en

OECD & Eurostat. (2018). *Oslo Manual 2018: Guidelines for Collecting, Reporting and Using Data on Innovation, 4th Edition, The Measurement of Scientific, Technological and Innovation Activities*. Paris/Luxembourg: OECD Publishing. https://doi.org/10.1787/9789264304604-en.

Pais, I., & Stark, D. (Eds.). (2020). *Power and Control in Platform Monopoly Capitalism*. *Sociologica*, *14*(3): 43–193.

Roitman, J. (2013). *Anti-crisis*. Durham, NC: Duke University Press.

Schumpeter, J.A. (1942). *Capitalism, Socialism and Democracy*. New York, NY: Harper & Brothers.

Schumpeter, J.A. (1989). *Imperialism and Social Classes* (P.M. Sweezy, Ed.). Fairfield, NJ: Augustus M. Kelley. (Original work published 1951)

Smith, A. (1776). *An Inquiry into the Nature and Causes of the Wealth of Nations*. London: W. Strahan & T. Cadell. https://doi.org/10.1093/oseo/instance.00043218

Stark, D., & Pais, I. (2020). Algorithmic Management in the Platform Economy. *Sociologica*, *14*(3): 47–72. https://doi.org/10.6092/issn.1971-8853/12221

Stephan, P. (1996). The Economics of Science. *Journal of Economic Litterature*, *34*(3): 1199–1235.

Touraine, A. (1973). *Production de la société*. Paris: Seuil.

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