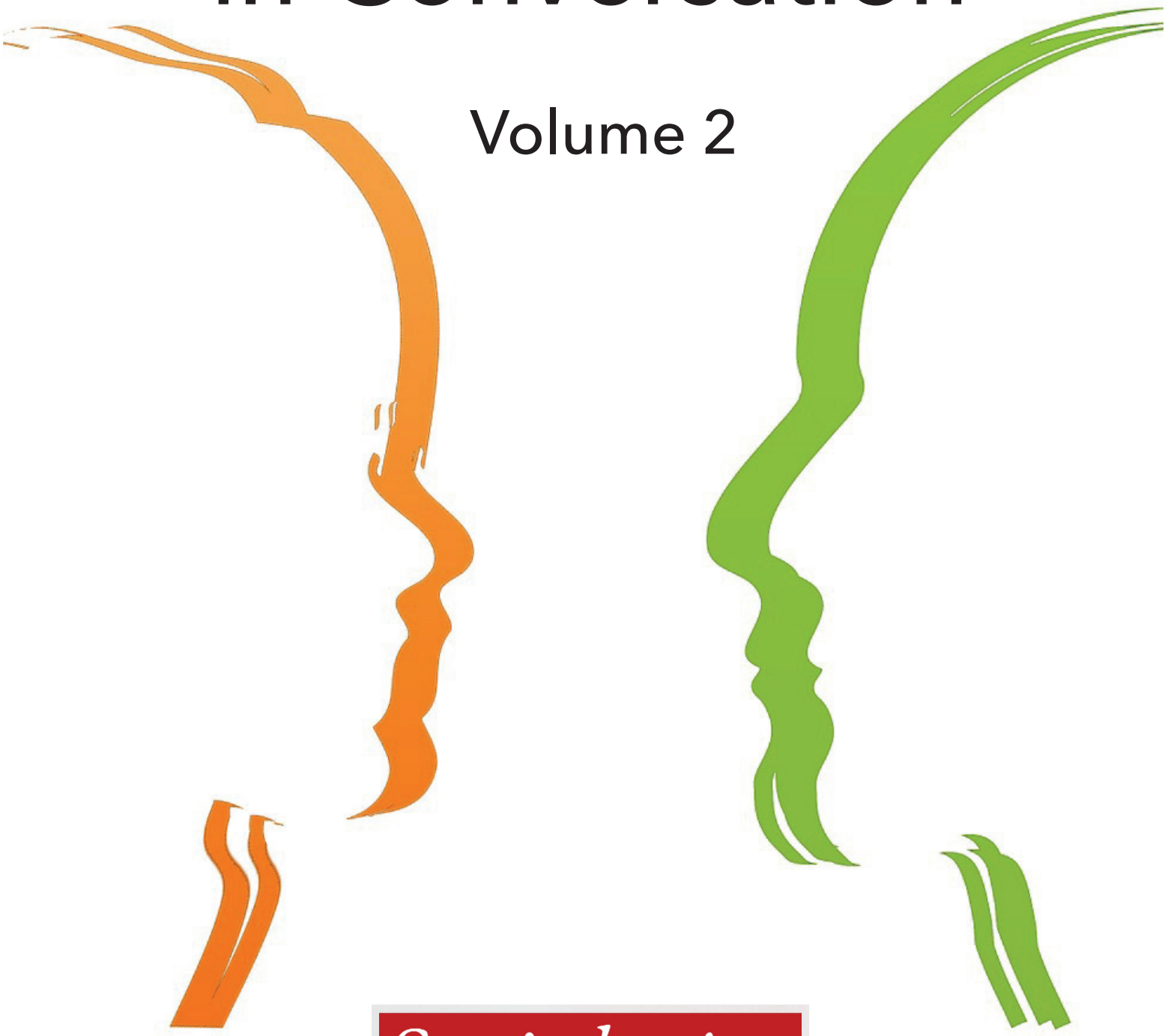
Sociologists in Conversation

Volume 2



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EthnoGRAPHIC: An Interview

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Abstract

The interview focuses on the book series EthnoGRAPHIC (University of Toronto Press) and the graphic novel *Lissa. A Story about Medical Promise, Friendship and Revolution*, the first book of the series. Four points arise from the interview with authors Sherine Hamdy and Coleman Nye, and with the filmmaker Francesco Dragone, who documented their research process. First, the problem of funding multimedia and innovative research projects, aimed to find new ways of communicating social research. Second, the question to what extent such projects are recognized and legitimated within the Academia. Third, the audience potentially interested in reading (ethno)graphic novels and, relatedly, their usability in teaching social sciences. Finally, the concerns and practicalities in putting together different narrative forms. This effort of combining several ways of representing social reality, also concerns the organization of the research itself as well as conducting fieldwork and the capability of thinking "graphically" from scratch instead of adapting textual data collected during the research.

Keywords: Graphic Novels; Collaborative Ethnography; Research Process; Medical Anthropology; Teaching Anthropology.

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Eduardo Barberis & Barbara Grüning (Q): *We'd like to know the origin of your project*, Lissa (2017), *and also of the book series* ethnoGRAPHIC. *Which project was born earlier:* Lissa *or the book series?*

Sherine Hamdy (SH): They were actually born simultaneously. I had written a piece, a blog post about how I began using graphic novels for teaching medical anthropology, and the editor at University of Toronto press, Anne Brackenbury, saw that. She gave me a call and she said "You know, I've been thinking about doing a series that brings together anthropology with graphic novels. Would you be interested in something like that?" Coleman [Nye] and I had already been working on a way to bring our research together so that was sort of the first birth of this project. As soon as Coleman finished her dissertation, we started working on the writing part — what we call "the script", that is the dialogue — and then we presented our vision for it at a Graphic Medicine conference in California. We were lucky to get funding for it and then Francesco Dragone, filmmaker, joined the team as our videographer/filmmaker. We got funding to go to Egypt and that's how it started.

Lissa was the first book in the series and then after *Lissa* came out I became the series editor along with Marc Parenteau, who is a comic artist that we met when we first did our presentation on Graphic Medicine, and so he gives us his evaluation on the submissions from the perspective of a visual editor/practitioner. We give feedback on the story together.

Q: *Where did you get funding for a project like* Lissa *?*

SH: A lot of the funding was from where we were at Brown University from the Watson Institute for International and Public Affairs and we also got external funding from the Henry Luce Foundation within a program called "Religion in International Affairs".

Q: *Did you have any specific reference in mind when starting your project? For example Brad Evans in this issue (2021), when he reflects on his* Portraits of Violence *(2016), maintains that it is not possible to work on a graphic novel on violence without knowing* Maus *by Spiegelman (1986). Is there any previous work that grounded yours?*

Coleman Nye (CN): Both of us were teaching medical anthropology classes and both of us have been teaching graphic novels in our medical anthropology classes. We had found them to be beautiful and effective at conveying embodied experiences of illness for patients, communicating the multiple different layers of communication and cultural dynamics at play in clinical encounters. At that time graphic medicine was really taking off: using comics to teach medicine in medical practice and to convey illness experiences. Both of us were really inspired by the works coming out in graphic illness memoirs and graphic medicine.

SH: I think *Maus* and *Persepolis* (Satrapi, 2000) are the two central works that really brought scholarly attention to graphic novels as a way to communicate really difficult topics. Of course, Joe Sacco's works on Palestine, too (2003). Especially *Persepolis* and *Maus* have had a very strong influence in graphic medicine, and I think for Coleman and me, because we were teaching medical anthropology. We were finding that there was so much that was memoir based — which is great because memoirs are personal and very moving — but we thought: how can we bring in our social perspective? How can we make this a larger story about social structure, about structural violence? What would that look like when our only models had been memoirs? So, *Lissa* was kind of a new formulation.

Q: Lissa *is a multimedia project, because it's not just a research and a graphic novel, but a*

film. What kind of audience did you want to reach and engage?

Francesco Dragone (FD): I can speak for the idea of filming the whole process. When I was contacted by Sherine, it was almost like: "We're doing something completely new, we don't know where we're going and so let's try to make an archival effort at least to be able to document this process, so that the rest of the series can rely on this as an opening path." And then the idea shifted a little bit, I believe, becoming almost like a multimedia, multi-layered effort. When we started to film the process, it became apparent that it was important to incorporate some of the graphic novel into the documentary, and on top of that also the archival effort made by the people living the revolution in Egypt. So we went back in time exploring this archival evidence, making it so much more powerful and impactful, having access to something that happened five years before we were actually able to go to Egypt.

To answer the question about the audience, I would say that the first thing that came to mind was getting closer to the readers, because the film is another layer of the story but, most of all what I got in the last couple of years by people that saw the documentary alongside the book was actually to engage in a different level of conversation with the students. The young characters are the first layer of interaction, just because they recognize each other when they see other kids going through school and exploring a different world. So Sarula [Bao, illustrator] and Caroline [Brewer, illustrator], which are the other two protagonists of the documentary together with Sherine and Coleman, are there and it's very neat to see that almost Anna and Layla [*Lissa*'s fictional protagonists] — even though they don't exist — are also the protagonists of the documentary. They kind of joined forces all of a sudden.

SH: And I have to say that it was all Francesco, that was all his amazing idea to think: "How can I document fictional characters?" Because we really did feel like when we were in Egypt, that they were with us, like we were seeing everything through their lenses. We knew their story so well and I think that was one of the most brilliant parts of the documentary, the way he was able to bring that feeling.

Q: *As for the book series, is there a specific audience/readership you had in mind, or is it more project-specific?*

SH: Anne Brackenbury again, who was the editor at Toronto University Press and came up with the idea for the series, had been running a Teaching Anthropology blog. So I think it was always an idea for the classroom — how to teach anthropology for students at undergraduate or maybe at a graduate level.

CN: Anne Brackenbury's daughter is a high school student, and she ended up reading *Lissa* as well and enjoyed it. So, I think one of the cool things about the graphic novel format is that it's really visually accessible. Because we wrote it also in the format of a novel it's also just a compelling story and so even my stepdad read it. My stepdad knows nothing about anthropological theory... he just appreciated that it was a powerful story, and he learned a lot about medical inequalities. I think the beautiful thing is that there are multiple layers that you can read into the graphic novel: there's the story, there's lots of information you can take away from the story that can appeal to all readers, with different ages and backgrounds.

But then I think there's something else, where you have a grasp of anthropological theory: there are different insights that you can bring with you: for example, here is how political violence or structural violence is lived. You can see anthropological concepts actually illustrated, coming to life.

Also, for the artists' side there's all this symbolism in the book: what is the role of the cats, for example? What is the role of the eye?

So, there are lots of different layers because of the combination of image and text that can make it differently accessible for a wide range of audiences.

SH: Because it brings people in at so many different levels, I think that was part of why for us filming was also very crucial. I recently taught it in a medical anthropology class and I showed the film. Students said to me: "I read the book and I enjoyed it — I liked it and I understood it but it wasn't until I saw the film that I thought — 'Wow! You did so much work!' "

CN: For students, watching the film lets them realize that Sarula and Caroline [*Lissa*'s illustrators] were also undergraduate students at the time. They like to watch people who are basically their peers going on this trip and doing anthropology for the first time, and then translating that into visual language. I think it's exciting and inspiring for students to see people their age doing this incredible work.

Q: *When we talk about graphic novels, we talk about a content that has to be translated in comics language, and this implies also a question of style, of aesthetic features. How much such design features are important for you — but also for the readership — to provide an interpretation of different meanings of social and graphic research?*

SH: It's important, but at the time we had already picked the artists before we had any idea of what the pictures would look like! We were somehow locked in, and then we were pleasantly surprised because we liked their artwork.

But since then, in my role as series editor, I've gotten a lot of submissions and now I appreciate how important the aesthetic dimension is, because I get submissions where I just immediately like or don't like the art. Sometimes the style really turns me off, and I don't even know necessarily the words to describe it. Then Marc Parenteau, the visual editor, helps me — for example: "It's because these dark lines are very aggressive and don't match the story". In understanding which style is good there's a lot of art, and we were lucky that our artists were very well trained in their illustration program — to learn how to match their style with the mood, the tempo and all of these different things.

CN: You can really see that in Francesco's documentary as well. You can see us all sitting in a hotel room in Cairo and having this conversation. There's Sarula asking questions like: "Well, what is your character like?", and we discuss different ways to think about characters' personality, or "How would you visually represent that or visually convey that?". For example, she pointed out that there's a lot of graphic violence that we might want to simplify and make a little bit more cartoony, because it could be too explicit if you visually depict graphic violence.

SH: in that scene of the documentary, when the illustrators are explaining to us this point, they said: we have to know what the whole arc of the character is before we design the character.

So, for example, in reference to the story of Anna: she was burdened as a child with this very heavy load — her mother's illness and then her mother's death — and she was put in an adult role so early. The way that Sarula shows that is by making her hands and her feet too big for her body — that's the way she used to show this adult role that she's been thrust into at too young an age.

Q: *We'd like to explore a bit more the relation between the cartoonist and the social scientist. Collecting the different experiences on making social science graphic novels, we saw an array of different levels of autonomy and coordination between illustrators and researchers, and different degrees of awareness about each other's job. How did it work for you in the making of* Lissa *— in the production process, so to say?*

CN: When we started, we were really separated. Sherine and I wrote the story and then

we gave the first draft to Sarula and Caroline and they started to thumbnail, that is to draw tiny sketches of each of the pages. Then, we looked at the pages together and started to have conversations around it.

Nevertheless, it wasn't really until we got to Cairo and all of us were together where we really started to work together to translate across our different disciplines. For example, they had to teach us what visual language means and how you can convey ideas via symbolism — and then we had to show them how we do our work. In this respect, Sherine did incredible work, for instance bringing us into interviews with doctors who had been on the ground providing care to injured protesters. Here the illustrators started to see the anthropological process with us, and to learn firsthand from people who had been there. They also met local Egyptian cartoon artists, spent more time looking at Egyptian graffiti and so they became part of the anthropological process with us.

So, we collaboratively went through the script together to really think through the kind of visual narrative that we wanted to tell, the important components of the story. Francesco did a beautiful job documenting this process as well

FD: From my perspective we understood while we were in Cairo the importance... for example Coleman was referring to the places, walking through the actual neighborhood so... That made a lot of sense, because when you need to place a character in a space it's impossible to imagine that just through photos, or a narrative told by someone. You need to experience that and so being there. Not only do you know the pollution in the air in Cairo, the situation, the pressure, the military and all that, but also seeing one neighborhood against another neighborhood, you know, one is more refined and rich and clean and the other one where everyone else is just living. So, these contrasts really came to life for Sarula and Caroline and for all of us while we were in Cairo. So, it was like a fundamental piece.

Q: *It was a kind of knowledge transmission that you need to have through experiences, but at the same time you need to know how a social scientist or an anthropologist thinks and how "graphic novels' language functions", so to say.*

SH: Exactly, and when I was in Beirut — I think for a conference — and I was telling Lina Ghaibeh, who's a well-known comic artist, and she's the chair of graphic design at the American University of Beirut... so I was telling her and her husband, who is also a very famous cartoonist, about our idea, and they said: "What! You are working with American students on a story that takes place in Egypt!??" and we said: "Yeah!" and they said: "But what about all the visual references?" and I said: "Oh well we know those, we're watching movies" and they said: "No, no, no! You have to take them to Egypt!". I didn't think it would be feasible before, because we had a short timeline and we didn't have the money, but it all worked out in the end, and we really needed to have gone! I know that a number of anthropologists are working now in their fieldsites with artists, but we didn't do that: we had completed our research before doing the graphic novel, and then we built the graphic novel based upon the insights from our research, and then wrote a story to piece it together. But I like that a number of anthropologists now are starting from the ground up, where they're undertaking research from the beginning in collaboration with local artists to try to undertake the research collaboratively, with the aim of making a graphic novel, and I think that looks very different if you start from that stage.

Q: *Turning back to the book series, how does the programming work? Do you receive submissions, or do you invite people to submit? Is there a market interest? And how much are scholars interested in publishing a graphic novel — in the frame of this "publish or perish" game, that may*

rank publishing a graphic novel quite low in academia.

SH: We do get a lot of submissions. When I give talks about *Lissa*, I always encourage people to think about, to think of their work in that way, but all of our submissions have been unsolicited. The second graphic novel after *Lissa* is called *Gringo Love*(Carrier-Moisan & Santos, 2020) and it's about sex workers in Brazil. Actually, before that, Lochlann Jain, a professor at Stanford, produced one called *Things that Art* (2019), which is not really a comic in the sense of a story told through panels, but more of a juxtaposition of images with text. And then the next one we have, I'm very excited about, it's called *Light in Dark Times* (Waterston & Holland, 2020), a very cool story. Alisse Waterston was the president of the American Anthropology Association and she was giving her inaugural lecture, and it was right after the election of Trump, and so she was talking about how to find light in these times that look so stark. And there was an anthropologist in the audience, Charlotte Corden, who happens to be a good artist, and she was just doodling while Alisse was giving her lecture. Then she went up to her hotel room and just stayed up all night drawing, and the next morning she met Alisse and said: "I have to show you what I've been doing!" And that was the birth of that project and they completed it really fast! I was impressed.

And just to the "publish or perish" point... it definitely puts people in a difficult position, I think, especially for young scholars because there's just a big question mark off what is the weight and what is the value of a graphic novel in academic metrics and so... if we get submissions from young scholars early in their careers, they often have big ambitions, and they don't have a good idea of what it takes to get there. So, I think it's easier for senior established scholars to be able to pull something like this off, but you know I hope we continue to get submissions from everybody.

Q: *This is very interesting. I think the point is to what extent this kind of work is considered, so to say, "scientific". Maybe younger people fear that if they do something like that, their work is not scientific enough for the disciplinary community, for example. I don't know if you have this impression, if some of your colleagues are not interested in doing something like that, because it cannot be considered scientific, because it's in a different language.*

SH: Yeah precisely, that's the big concern. There are a lot of funding issues, I think, connected to that as well, like it's very hard to get funding for artists in the social sciences. So, the ways in which grant structures, also don't necessarily make it easy to produce graphic works or collaborative projects between artists and ethnographers.

CN: I think it depends on programs, too. I have a student right now who's defending her dissertation and the entire dissertation is a comic. But I don't know what's gonna happen in the academic job market for her, but I'm feeling hopeful. I think we are seeing a shift now in the social sciences and humanities toward more multimodal forms of scholarship, so I'm hopeful!

Q: *The articles in our section show that something is moving around — ERCComics,* Sociorama *— most are very recent publications, popularizing social sciences. What do you think about this market?*

SH: I just want to say, but not to discredit any work, but I think it feels very different when you have a completed work and then it's translated by professional artists. It's a completely different feel than having the artists who from the beginning document what they're learning, and I think, when you look at some of the *bandes dessinées* in *Sociorama*, that it feels like a translation to me, it's a different process.

Q: *So, do you think that the value-added of your work is basically that you have you to work together. Thus, the point is not only popularizing, but also constructing something with a goal.*

CN: We think it's really helpful for anthropologists — as we did venturing and having worked with this PhD student, who's making a comic. I think there's something pedagogical, for social scientists as well, to learn how to think visually and to really approach the comics form itself as an invitation to show rather than tell, like all the potentials that are embedded in the page. I think it's really exciting and I think it opens up a whole range of different possibilities for how we can draw connections and we can think. So, I just think that there's a lot we can learn from artists about how to convey ideas outside of text, it's exciting!

FD: it's difficult to explain but I felt like the anthropologists and the artists established a common table to open a conversation. So, it's really like not simplifying the knowledge of the ethnographic methods and the ethnographic research that has been done, but making it more accessible to a broader audience. So that's what happened there, in my opinion.

CN: We have different two-page spreads in the book , which are two full-page illustrations side by side, and we talk about how we're able to convey a whole bunch of interconnections globally between health and politics and economics and environment with no text, with all visual language and the kinds of connections that you can draw that would take many pages in an ethnographic monograph! and then you also give your readers an opportunity to fill in the gaps and for them to kind of meet you and do their own analytical work which I also think is a really generous reading practice.

Q: *Which difficulties do you see in continuing your projects, and also which are your hopes about?*

SH: I think finding funding has been the biggest difficulty. These projects also take much more time, I think, because of the multiple components, and so the publishing timelines are not always amenable to the kind of timeline that it takes to produce a good work. Plus the cost is higher.

CN: I think we live in an exciting moment, though. I'm junior faculty, and I don't have tenure yet, but I do feel very supported by my department for *Lissa*. I think because the academic series is at University of Toronto Press, it is validated within larger academic circles as scholarship. So, I feel confident that *Lissa* counts toward tenure and I think the more projects that we have like this, you know, the more we're going to produce the conditions to make it easier for junior faculty to do this kind of work, and to shift some of the expectations around what academic publishing can look like.

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Lucy Suchman in Conversation with Ana Gross

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Ana Gross (AG): Can you tell me about your research experience at Xerox Palo Alto Research Center (PARC)?

Lucy Suchman (LS): I came to Xerox PARC as a PhD student in anthropology immersed in studies of face-to-face human interaction, with its extraordinary choreography, the moment-to-moment co-construction of our mutual intelligibility. At PARC I encountered other disciplines seemingly asking similar questions, but this time about human computer interaction, or interactive machines. And so that was a kind of immediate hook for me to think about. At that time the idea of interactive machines was, you know, quite surprising: you didn't hear technologies being characterized in that way, but also of course it resonated so much with my own background. And so eventually my doctoral dissertation ended up being a study of interaction at the interface, taking seriously the idea that human machine interaction was interaction in a kind of comparative sense to the way that we then understood how human interaction worked. My study of human machine interaction happened in the context of an expert systems group at Xerox PARC, which took me into the realm of AI and cognitive science, and associated ways of trying to conceptualize communication, understanding, mutual intelligibility and so forth. There was a small but significant number of my co-researchers who found my study interesting, it was really thought provoking for them at that time. So it was through those apparently shared interests in interactivity and cognition that we came together, although as an interactionist anthropologist I was never directly engaged with cognition, say in the way that Edwin Hutchins (1995) was. I was always thinking about human machine interaction much more relationally,

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and already in a Science and Technology Studies (STS) kind of way. But still there were many resonances, and I think apparent points of similarity and actual differences, which created a very generative space for me to work in. So it was an entry into this world, and then looking for places where I could work in that world that were interesting to me and that I could make interesting to people there. I think this is a long-standing anthropological move. Part of what comes out of doing ethnographic work involves reframing the problem. Looking at the dominant way in which things are being framed and thinking about how that can be interrupted and reworked, and I think that's a really productive way to enter into interdisciplinarity. And that kind of reframing is one of the strategies for, again, working across similarities and differences.

AG: What prompted you to move from Xerox PARC to Lancaster University?

LS: For me it was primarily the changes in the industry that occurred towards the end of the 1990s. There appeared to be a kind of embrace of a lot of the work that I and my research group, the Work Practice and Technology area, had been doing — at that point we were in a laboratory called Knowledge and Practices so we would seem to have made huge headway. But actually, at the same time that that kind of language was embraced and, in some ways, appropriated, there were differences between our understandings and those differences became less and less acceptable. I felt that there was less room or space for difference or commitment to long term research projects: it was more about the short term of returns to shareholders and what would be the next billion-dollar app. My immediate manager at the time accused me of being "resistant to change," and I realized that I was resistant to getting in line with the corporate programme, which was the change that was increasingly being imposed on us as researchers. When I moved into a halftime position at Lancaster University, I found it to be enormously liberating. It wasn't until I left Xerox PARC that I realized how much I had had to fold myself into particular shapes to fit in there, and so going to Lancaster was an incredible sense of opening. It was building on work that I had done before but now being able to do it in a space where thinking critically, opening up problems, trying to understand things in a more nuanced way was celebrated and supported. The department of Sociology at Lancaster was particularly interdisciplinary, with scholars working on STS, feminist theory and gender studies, so it enabled me to expand the critical side of my work and return to an early interest in anti-militarism as a focus. The interdisciplinarity of our department was crucial: I would never have gotten a job in an Anthropology department. While I love Anthropology it tends to be quite orthodox, I mean it can be very radical in many ways but there is a kind of disciplinary orthodoxy there. And if I had had a career in academic anthropology only, I probably would have been quite frustrated.

AG: How do you see the future of social science and industry collaborations beyond the in-house anthropologist model? And how can academic ideas translate better into other worlds?

LS: It is really important to emphasize what a distinctive place Xerox PARC was in my early years there. It was a very open, academically oriented research centre, founded by academics, largely populated by academics who of course also had some government contracts, particularly with the Defence Advanced Research Projects Agency, at that time. Xerox PARC was created under the auspices of economic prosperity, in line with other great industrial research complexes like IBM or Bell Labs. However, as the years went by and the computing industry grew, and Xerox's place in the computing industry became more uncertain, that open research space started to close down. There was an increasingly intensified focus on quarterly business analyst reports, short term returns, etc. And this is really antithetical to research; that is when

collaborations between academia and industry become really difficult. I guess I cannot be terribly optimistic about the spaces within industry where there is genuine investment in any kind of long-term research, or anything that requires ongoing engagement over time. Although there is an opening to the social sciences within industry for the most part it is extremely instrumental. And at the same time of course it is so important for sociologists, anthropologists, people working in STS to actually understand what is going on in industry as a site for research, as a way of looking for opportunities to learn how things work from the inside, I think that is really valuable. But I would not have huge expectations of finding really generative places to do research within industry at the moment. In terms of translation I've long felt the need to write for multiple audiences, in multiple genres. What I like most is writing academic papers, because I love scholarship, I love citation, and I like reading and thinking with other people, which is how I think of the best sense of academic papers. The careful findings of the antecedents of what you want to write about and then building on those arguments and starting new conversations. I love that kind of writing and it is quite important as a space for us to be able to do challenging critical thinking. Then the question is how you take what you learn there and voice it in different modalities. In the past few years, for example, I've become involved with the International Committee for Robot Arms Control, which is part of a larger effort, headed by Human Rights Watch, against autonomous weapon systems. I've spoken at events in the UN and engaged in these movements for arms control and disarmament, which is incredibly important but also really limiting. You have to accept the idea that all of these countries, all of these states, are going to continue to be militarized, and then in a way you are trying to avoid the worst, which would be to automate the identification of targets. I have also been following closely the developments of the National Security Commission on AI, which is a collaboration of Silicon Valley promoters like Eric Schmidt, former Google Alphabet CEO, and technophiles in the US military, to promote AI investment. The National Security Commission came up with a seven-hundred-page report and I wrote a blog post for the AI Now Institute entitled "Six Unexamined Premises Regarding Artificial Intelligence and National Security" (Suchman, 2021a). Things that were absolutely assumed and unquestioned in this report that I thought were the fundamental things that we needed to talk about, and I also did a podcast[1] on that. I think it is important to challenge ourselves to expand our voices and collaborate with other people in advancing academic ideas in other areas. Telling your story in different ways is crucial and generative, both to different audiences and to yourself. In some spaces it is very difficult to do that work of reframing. For example I was invited to a workshop convened by the Defense Innovation Board, advising the Pentagon on a set of AI Ethics Principles. And I found it difficult to engage in that discussion, because everything that I wanted to talk about was outside of the frame. So, yes it was hard. That does not mean that we don't need to keep working at it. But sometimes it is difficult to find a voice in those conversations without being appropriated, without adding legitimacy to a project you don't agree with. I was also invited by Microsoft to be an ethics adviser to their Integrated Visual Augmentation System (IVAS) program (Kipman, 2021), and for a moment I thought, what a fantastic ethnographic opportunity! But then I asked what the terms of my engagement would be and basically was told that I would never have been able to say anything publicly about it. So it requires careful consideration to avoid being one of the ingredients in the recipe "add a social scientist and stir"!

AG: It seems to me that current AI development is being shaped and informed by cogni-

1. https://techpolicy.press/ai-and-national-security-examining-first-principles-a-conversation-with-lucy-suchman/

tivist understandings of interaction, meaning-making and context-making. The AI field has been mainly informed by cognitive theory when it comes to interaction and communication. Why do you think the AI field has failed to incorporate social models of interaction? Do you think this is somehow a disciplinary battle that social scientists lost?

LS: I think it has partly to do with the history of the behavioural sciences, where psychology, which was folded more closely into the biological and behavioural sciences, put the focus on the individual cognizer, and was in this sense more aligned with the scientific framings of the computational modelling of the brain and the neuro-scientific modelling of the computer. When I came to Xerox PARC, when people talked about interdisciplinarity they meant, well we have, you know, computer scientists, we have computational linguists, we have physicists, and we have cognitive psychologists. The cognitive psychologists were mainly working on information processing, so there was already such a strong alignment between computer science and individualist or psychological models as ways of understanding cognition. Social scientists came to work on the periphery of such alignments, they worked around the edges. There is, however, some progress being made in trying to introduce critical social scientific sensibilities into the AI field. If you go to the Computer Human Interaction (CHI) Conference and the burgeoning conferences on human-centred AI, there are increasing numbers of young researchers who are quite conversant with and inspired by different aspects of critical social science and humanities, who are really not satisfied with that superficial add-on version of social science.

AG: I'd like to touch upon your current work on AI based military systems and your reformulation of the concept of "situational awareness". Your work suggests that human interactivity with a given environment and other types of agencies is paramount in generating context, meaning-making and more importantly, in the shaping of moral and ethical decisions in the battlefield. Does your work fundamentally suggest that machines will always be incapable of such moral and ethical accomplishments? Or does it suggest that it is a matter of building the right machines underpinned by the right anthropological and sociological models?

LS: You are asking a question which is at the heart of the work I have done and which I am very much still thinking about. When I first started my engagement with human computer interaction and AI, I was careful not to make programmatic arguments about the essential nature of humans and machines. My argument has always been that it's not about diminishing the machine's capacities but letting machines be the specific artifacts that they are, not aiming to have them be approximations of humans. Social understandings of human context-making are quite different to the behaviourist models of perceptual abilities, for example, that have been used to build AI, based on input/output information processing exchanges between the environment and the cognizer. Conversation analysis and ethnomethodology brought this crucial shift from thinking about the environment or world as out there to worlds as ongoing accomplishments. The much more radically interactive propositions coming out of interaction studies, and also the propositions coming out as part of the poststructuralist, performative turn in the social sciences, challenged this notion of context or environment as a mere container or closed world view. Karen Barad says that we are not in the world, nor are we of the world, we are part of the world's differential becoming (Barad, 2007, p. 91). This is quite different to the ways in which different disciplines — cognitive psychology, computational science, neuroscience, and related disciplines — advanced individual cognitivist models of information processing and awareness based on the assumption of closed worlds. I've just finished writing a paper entitled *Imaginaries of Omniscience: Automating intelligence in the US Department of*

Defence (Suchman, in review), looking at the history of cognitive models of situational awareness in the military, and more specifically the Observe, Orient, Decide, Act framework. Even in the more cybernetic versions, the assumption is that there is a kind of whitespace out there, a closed world loop. You will never get from those models of situational awareness the kind of openness, contingency and indeterminacy that characterizes and forms the basis of human interaction. And it becomes increasingly clear that in order to make AI systems operate in the openness and contingency of human worlds it is necessary to reengineer these worlds as closed worlds. I worry about this in relation to automation of weapons systems and military training. If you look at military training simulations, arguably what is happening in that field is that it is done in the name of simulating the world out there that the soldiers will encounter, so that when they encounter it, they can recognize what is going on. But the other way of reading simulation is that it is about generating a closed world, which the soldiers can take with them wherever they are deployed. This in some way will make them impervious to any kind of openness to what is going on around them. And you can see that even more in this IVAS HoloLens project coming out of Microsoft, with is the idea of a head mounted display for frontline infantry, where basically their engagement with the world will be mediated through an information processing interface. These technologies are being developed in the name of expanding situational awareness, when in reality they are closing awareness to maintain closed worlds. The thing that radically interrupts the whole computational and AI project is the openness and contingency of human interaction, you can see each round or iteration of AI as an effort to close worlds. I am always looking to be surprised, and I monitor developments in AI and robotics to see whether something radically changes this argument, but so far nothing has.

AG: When did you kind of come out as a feminist or how did feminism encounter you? What do you think are the most ground-breaking feminist ideas at the moment, both at a political and theoretical level?

LS: I'm not someone who really identified as feminist in the 70s, or even in the 80s. I began to read feminist writing and realized that I was a feminist, becoming increasingly aware of the long and complex history of feminism in all of its different aspects. I realized how increasingly important it became to mark one's work in relation to feminism, rather than just appreciating or acknowledging other people's work as feminist. I can't remember exactly who I first read but I think for me the greatest resonances were possibly Judith Butler and Donna Haraway along with the post structuralist move within the social sciences, which of course resonated very much for me as well with my more ethnomethodological background. The whole idea of the performative agencies of discourse, the inseparability of the material and the semiotic, the idea that social structures have to be understood in their ongoing reproduction, rather than as given. And the fact that such structures have to be reproduced, but that there are also always slippages in that cycle of reproduction and that those slippages are points of potential intervention for transformation. One of my earlies feminist papers, first published in 1988, was called *Computerization and Women's Knowledge*, which I co-authored with my colleague Brigitte Jordan, where we compared childbirth and office work (Suchman & Jordan, 1988). We talked about how in both cases, technological projects like Western biomedicine and high tech childbirth, and office automation shared a failure to recognise the longstanding knowledge that informed childbirth and work practices respectively. The comparison also served to recognise the limits of the formalisation of computation and other kinds of so-called technological improvements. Feminist technoscience is an incredibly exciting and burgeoning field. Some of the most exciting intersections for me are between feminism and post or decolonial

thinking. Michelle Murphy[2] and Max Liboiron[3] are, for example, working at the intersections of feminist, environmental, indigenous, decolonial thinking and they are shifting the frames of reference in terms of literatures in very exciting, challenging ways. In the area of militarism and demilitarization there is really interesting work on questions about how identification of civilians and combatants is done, and the gendered and racialized aspects of that, like the work of Christiane Wilke (2017), which is really pushing the boundaries of what it means to be a civilian.

AG: Can you tell me about your methodological toolkit? You have written from a methodological standpoint about the videotape; the prototype; the face-to-face interview. What has been your methodological proposition throughout your career?

LS: Having a disciplinary background in anthropology there's certainly for me an appreciation of ethnography, in the sense of being present over extended periods of time in the worlds that you are trying to understand. I believe this allows you to get an understanding that you cannot get in any other way. In some ways I could think that I did a 20-year ethnography at Xerox PARC, and this is probably the place that I know best from the inside out and that has been a tremendous resource for me. There was certainly a period of time when I was also strongly influenced by interaction studies, conversation analysis, ethnomethodology, video analysis and so forth. And again, I think I learnt things from these methods that I could not have learned any other way, in particular about the interface or human machine interaction, and for my own dissertation that proved to be extremely generative. We used video analysis in our work practice studies at PARC, and co-design projects, and we found video incredibly valuable in the sense of capturing unremarkable and mundane choreographies that people would not be able to tell you about. However, video is also limiting as there are things that you miss given that you have a really tight frame on things, so the contextualization required is incredibly important. I also managed get involved in some really rich projects concerning prototyping, for example a project that we did with the California Department of Transportation with a team of civil engineers where we were able to do ethnographic work understanding their work as civil engineers, do some close studies of their work at their workstations, and actually create a prototype document system for them and test it out. This was a multi-methods project, and this is what made it so rich. So, I think being at Xerox PARC was an opportunity to explore and work with a really extraordinary range of methods that for me came out from anthropology, interaction analysis, the field of computer supported cooperative work; participatory design; STS, etc. And yet now my writing and research is more and more reliant on secondary sources. Working on the military, I've not been able to figure out a way to do research about this field ethnographically for a variety of reasons, but particularly reasons of access. So I am finding myself, for better or worse, more reliant on a range of diverse secondary sources and archival materials, which are also very generative. I am very grateful to have had the opportunity of working with so many different methods and the contexts that made that possible. The main point would be that I am against any kind of methodological orthodoxy. I think it is important to craft the problem you want to engage with and where you engage it, then think about the kind of methodological toolkit that you can draw on to do the work that you are trying to do.

2. https://technoscienceunit.org/

3. https://civiclaboratory.nl/

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Karin Knorr Cetina: An Interview with Alex Preda

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Abstract

Karin Knorr Cetina was already a well established figure in the Sociology of Science and Technology when, in the mid-1990s, she changed the focus of her ethnographic investigations to financial markets. What followed was a series of edited books and journal articles that made significant contributions to the Sociology of Finance. In this interview, Karin, who is the Otto Borchert Distinguished Service Professor at the University of Chicago, reflects on the intellectual drivers of her research interests in financial markets, on the evolution and challenges of the field since the 1990s.

Keywords: Sociology of finance; sociology of science and technology; markets; economic sociology.

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Alex Preda: Karin, it's a great joy to have this conversation with you, especially since we've known each other now for over 28 years. I've been briefed about the main topics of this interview, and one of them is to reflect upon the mid to late-1990s, when sociological interest in finance arose again. We had this interest before in sociology, in the mid 80s. We all remember the work of Patricia and Peter Adler. But after the mid 80s, there was a pause I would say, and then in the 90s, there was renewed sociological interest in finance, and you had this interest too. So, my first question would be for you to reflect upon, what awoke your interest in finance, as a sociologist of science?

Karin Knorr Cetina: This is always a multi-dimensional question. But it certainly mattered that I had the Cern study, the study of high energy physics, finished and I was not looking to do another laboratory study, I wanted to do something else, really the study of high energy physics where practically I did all the high energy physics part myself was time consuming, and highly interesting. But also demanding in the sense that you wanted to do something else afterwards. And there was one thing left from the Cern study that haunted me, and that was the global nature, high energy physics had become very global, in an institutional sense, in the financing sense, during the last part of my study, and it was too late for me to do much about that, in the book that I wrote, the book epistemic cultures. It was also a very big topic.

So I left the topic, more or less, or at least to the degree to which I would have been satisfied with it, I left it aside. But I wanted to continue to understand what happens on a global level. And so that was one thing that pushed me in the direction of financial markets. There's always also I think, when one delves into a new topic, there's some preparatory things that have happened before, even if that didn't happen intentionally. And in my case, you know, I'm an avid reader of the New York Times. And on the front pages of the New York Times for years, for many, many years, even while I was doing the Cern study, there was a lot about what I after the fact recognized as finance issues. I didn't even know that in the beginning. I am not trained as an economist, and what we knew about finance in sociology came from Marx really, but often Marx was no help in explaining what I read, because Marx talked about the production economy, and what I was reading was about takeovers and investments and financial markets. And that was a secondary economy.

So these topics always fascinated me because of the imaginaries around the financial stories in public media. And then the rest that explains my interest was really opportunity. Two major opportunities. I think one was that you joined us in Bielefeld at the time, and you had an interest in the economy. And remember that you had that even when you came. You were not completely naive about economic issues, meaning unknowledgeable, you already knew quite a bit, you must have observed some of it. And you had that interest. And then the other thing that happened, I think you may have been part of the session, that was Urs Bruegger came to give us a talk. In our colloquium, I think it was in our lab studies colloquium that we had at the time. And Bruegger was at that time still in Zuerich working in financial markets, in one of the two big banks in Switzerland in Zurich. At the same time, he wanted to do a PhD and then maybe do something else than being a trader afterwards. He had heard about my interest in financial markets from a student of us. She told him about my interest. And then I invited him to that colloquium. And, you may remember, we submitted a grant proposal in economic sociology? Yes. You were on it, and I was on it. And I'm not completely sure whether that happened before or after, I believe maybe it happened before Bruegger's talk. So I was very interested in that talk. And I became fascinated by the talk. And then the talk led to our teaming up, Bruegger and I, and the next thing that happened is I was trying to do a study

of financial markets on the trading floor in Zurich.

The talk fascinated me but what I always need is a site visit. And at that site visit I went on the trading floor with Urs Bruegger, he introduced me. And, and I was immediately hooked again, I thought, okay, this is exactly what I was looking for, to study that. The looks of the floor brought up another memory from my study of high energy physics, the floor was a highly, high tech setting, obviously, has always been in financial markets, though not in banks, generally, very high tech. And so there was a continuity to the high tech setting in which I had worked before where the detector was the high tech thing. But it was a completely different, a completely different version of it. And a version of it, which I somehow sensed was also much more future oriented or was, you know, it seemed to be more a setting that pointed to the future of our capitalism and of society generally. So these were all these impressions and sometimes these contingent experiences are epistemically efficacious, you know, they stimulate you and they enforce an epistemic desire to understand a particular area better.

I have to add that at that time, first of all, I did not know economic sociology well enough. The version that existed in the German speaking countries, I really never found a way into it because it was mostly industrial sociology. It was not economic sociology in the American sense. And at the time sociologists did not talk about finance. And that's a very general thing that continued until the financial crisis in Germany, then they talked about finance. They didn't really look at the financial systems, but at the consequences of financial markets, for the economy, for the population, you know, and in terms of power, in terms of who occupies what power, for what reason, in terms of politics, but they never opened up the system, they never looked inside the system was my impression, I have to say my impression, because I haven't systematically studied the literature, but I think it's not an unfair take on the German situation. And I knew American economic sociology to some degree, but not sufficiently, I think, I was not well versed in it. So really, my entry into that area of financial markets was not so much stimulated by some scholar, or some connection I had to economic sociology.

I think both Donald MacKenzie and Michel Callon, who I think are the main anchors of economic sociology in the science studies area, if MacKenzie worked on that, at the time, I didn't know it. I didn't speak with him about it. So he was not an original influence. Of course, I got to know his work on finance later on very well, and continue to follow it and appreciate it, highly appreciate it. But I didn't know it at the time. And Callon published his piece I think in 98, so we're relatively late. And I didn't immediately read it. And it is not that well written. The introduction to that book he edited. It's not a bellwether piece. Or even when I did, I wasn't particularly how should I say, taken by it? But I didn't have that either. At the time you Alex were still a student originally, but then progressing towards the dissertation. Students have always been important to me. I didn't have to deal with all by myself, I had someone to talk to about this interest and to work with as we did on some of the grant proposals.

AP: Thank you. I want to linger a little bit on the mid 90s. If we look at how the sociology of finance emerged, we had for instance Mitch Abolafia or Charlie Smith working in New York City. And of course, New York City is a big financial centre, and we had Michel Callon and his group working in Paris which in the European context is a significant financial centre, and then Donald McKenzie working in Edinburgh, which we can make the case is still a significant European financial centre, but we were in Bielefeld. Bielefeld, no matter how we praise it and no matter how much we appreciate it, is not a financial centre. So we cannot say that in Germany the sociology of finance emerged in Frankfurt, or that it emerged in Munich, or that it emerged in Cologne. Some of the major contributions made by you were coming from Bielefeld. Do

you think that being in Bielefeld, in a setting that was not a major finance centre, was more of an advantage or a disadvantage?

KCC: Well, at a first sight it's a disadvantage because you are not having anything close by that you can work with. And I remember well that in that first attempt to look at the economy, we looked at banks and one had to travel, you know, it wasn't really working to look at banks in Bielefeld. So you have to travel to these banks and to try and get access, to go to the places where they are located. So it was not an advantage. But on the other hand, Bielefeld had many advantages. You know, one advantage I had used during the study of high energy physics was the chance you could switch between teaching and research. So you could sort of get out of teaching for one term and doing more research and then teach more the next term, and that sort of thing was an advantage. And in Bielefeld, you have a micro sociological tradition. And you have a systems theory tradition. And both of these traditions offered the possibility to talk with people about things encountered in financial settings. The surrounding supported a particular approach. And it was a good thing at the time. Sociology was a faculty in Bielefeld and not just a department, as you know, it was much larger and had a much smarter architecture, it had political science, it had anthropology. And so it wasn't just sociology, it was much more. It offered a strong support because of its tradition, for a particular methodology, ethnography, which I taught at the time in Bielefeld. And so the intellectual milieu of Bielefeld was stronger, I would say, I hesitate to do that to my Frankfurt School colleagues, but at the time in the mid 90s, my sense was that Bielefeld's intellectual climate was stronger than Frankfurt's, which had a good tradition in political theory. But I'm very happy that I didn't buy into that tradition, in which case, I wouldn't have gone into the system of financial markets fully, but would have turned them into a topic of, of, you know, resulting deprivations for the population, or a topic of capitalist interference with economic development. That is not what really interests me about markets.

Again, I have to refer to the study of high energy physics. I was launching this study from Bielefeld. And of course, there's no high energy physics laboratory in Bielefeld. But there is a national particle physics research center in Hamburg, DESY. Before I went to CERN, I did consider researching DESY and because it's always a question of access, and as it might have been easier to get access to DESY via German financing agencies, I debated that with myself but did not go down that line. Mainly because, actually because the really top level appeared to be Cern in terms of research at the forefront, and they had strong German participation. It was a conscious decision, an intentional decision to go to the top level, to the main European institutions instead of DESY, and I went to CERN. So, I was not afraid to take on a research site in Zurich quite a bit away from Bielefeld, because of my previous experience.

AP: Thank you. I want to talk now a bit more about the relationship between science and technology studies on the one hand, and the sociology of finance, on the other hand, simply because at least in Europe, we had three sites, Paris, Edinburgh and Bielefeld. All with a very strong reputation in the science and technology studies. And all of them got interested in finance, do you think that any of the debates which were going in the 1990s in science and technology studies had a relevance for sociology of finance?

KCC: The thinking in science studies, the conversations we had, all of that should have had an impact. In my case, I know that I wanted to go on the trading floor and follow the practice of finance, rather than study the institutions, the banks, or study the industry, or study the consequences. I think that's directly linked to science studies and the continuity of conversations we have partly at colloquia and workshops and conferences. But also partly, in smaller

meetings that we had when someone came and gave a talk, the idea that it was important to go to a lab, for example, and not to simply interview scientists, somewhere, you had to go to the site where things are happening.

The site question was part of our conversations, and that conversation started more than 20 years before the sociology of finance. It started at Cornell really, the first social studies of science meeting of Cornell in 1976, and it led to and implied an internalist gaze at the practice of science And the internalist view I took of financial markets comes directly from how productive it was in science studies. The other linkage is the one I mentioned before, it comes really from my experience at CERN, is the global question, the globalisation question. I was thrilled to be able to go into an area that was global in nature, currency markets. So, that certainly has a personal science studies background. Symmetry thinking as a general principle was for me also important in the sense that you are not approaching finance from a political economy or critical theory standpoint, but from a standpoint of more detached observation and that you investigate participants' actions and practices. You are also not limiting yourself to particular temporal presuppositions, or to a particular perspective, that was also important.

The concept that became important in social studies of science later is performativity. I just didn't have that concept at the time, and I have been critical of it later on. So, I am not a performativity theorist. And MacKenzie's work is, of course, completely down my alley, because he is opening up many black boxes and looking at their inside and seems influenced by his own previous research and is bringing a science studies sensitivity to finance. And, you know, in my case, that sensitivity was also to the technological environment. So I didn't see knowledge as usual immediately relevant when I went on the trading floor, but I did see technology, and what I knew from studying science raised questions about the epistemics of information knowledge. So there was the continuity of seeing the object world, for example traders' continuous interaction with screens, as relevant to and having to be written into a social science understanding of financial action, one couldn't simply stay on the human or human firm level, for example. And now, with the takeover of financial markets by quants and algorithms, there is a whole other level of relevance of science and technology that in a way confirms the intuition that drew me as a student of science to the study of finance.

AP: Staying within the topic for a little while, we can make the argument that the sociology of finance has benefited from science studies contributions, we can make the argument that science studies has opened up to finance nowadays, we see regularly articles on finance being published in Social Studies of Science. Before, that wasn't the case, we can argue at least not to the extent that it is the case today. How do you see the future of this relationship? Can we make the argument that this contribution is to be continued in the future?

KCC: Yeah, I am not sure about that. Because the question is, what are the science studies goals? And what have science studies today that is useful for the sociology of finance? The concepts that had a lot of influence, like performativity, are not that new. They are 20 years old, and have been criticised by sociology of finance scholars in the US for example. It really depends whether you can see STS as a coherent discipline.

Now, as it was a while ago in the 80s and 90s, at that time STS stimulated a number of directions of research, six or seven. But it was relatively coherent in that these groups of people talked to each other, met each other at conferences, and shared, I would almost say, certain kinds of interests and principles. And I'm not sure whether you can say that of STS today, it's a much more diversified field. There is not that coherence any longer. It hasn't progressed in a way where I see it necessarily, you know, as future oriented to the degree to which finance appears

to be, is also oriented to historical developments, it's oriented to working with computational data. It does a lot of things that are very interesting. But whether it provides for background interests and understandings on which we can constantly draw in, is another question.

Also, economic sociology in the US has its own strong tradition. From the 1980s on, economic sociology exploded in the US, not immediately in Europe, certainly not immediately in Germany, where it's continued to be more of an industrial sociology, even here in the US, this was the case. But the field of economic sociology grew, and has some fantastic, excellent scholars in there. And I can't see how, you know, whether it would need a lot of STS. I see a split, a split between what's called Social Studies of Finance, which in Europe is often linked to performativity. And, on the other hand, the economic sociology in the US, which is not linked to performativity and has its own traditions and its own dominant topics often centered on embeddedness, for example, the relationality of economic action, that's a strong tradition in economic sociology, and there are other traditions, an institutionalist tradition, which is important. There are some people who bridge some of these gaps, who on the on the one hand take something over from the social studies of science, on the other hand, continue the tradition of an economic sociology in the US.

AP: You talked about the links between the sociology of finance and economic sociology. Now, before I ask you this question, I want to make the following case. You know, I work in a business school, the finance department is separated from the economics department, which is separated from the management department, marketing and so on and so forth. How should we see the sociology of finance, as a subset of economic sociology, or as a separate domain?

KCC: I see it more as a separate domain. Also, for cognitive reasons. When you look at what the people investigating the primary economy are doing, you know, even the notion of a market has to be revised, when you look at financial markets, it's not a producer market, it's a different kind of market. So I see finance really as a separate domain. When we try to understand an economy, then there is consumption, there is production. And there is the distribution of goods, which is associated with primary markets. These are three pillars, but then there is finance which should be seen as a separate pillar. I've always seen it as a separate pillar, because it has to do with credit, doesn't it? It's not consumption, it's not production in an economist's sense.

In the 1970s, as you know, there were a number of theories being produced by finance scholars in academia, theories that had to do with financial markets, Black and Scholes, the efficient market hypothesis, all of that was produced around in the 60s, 70s. For financial markets, not economics. There are some overlaps but in terms of what they are doing and what theories they are using and which direction they are working and what communities they are in, finance is clearly a separate specialty. Economic sociology and the sociology of finance should be seen as separate specialties too. I do not see myself as an economic sociologist, I see myself as a sociologist of finance, an anthropologist of finance, if you wish, but not of the economy generally.

AP: I want now to shift my focus to micro sociology. You taught me its importance. Without any doubt, what place do you see for micro sociology in sociological investigations of finance?

KCC: There's a continually important place for it whenever you are looking. Think of real markets, they operate, in my view, on a micro sociological level, they may also operate on an international level, especially over the counter markets, they are located in big banks, the

first tier markets that trade the highest volumes. What happens on the trading floor is oriented to what happens on another trading floor in another country, and mediated by scopic media, and trading is interactional, whether with another trader in conversational trading or with the market on screen directly.

A financial market is on a fundamental level an interactional social form. It is not an organisation. That's reflected in the line institutional economists draw between markets and hierarchies. And therefore micro sociology will be useful, it is hugely important to understand markets. If you look at markets that are global, many financial markets are not local, but currency markets are by nature transnational is always one currency against another. The fascinating combination we have here is that we have something going on that's very large scale, not in terms of numbers of people involved, but in terms of effect and impact and volume of trading as well as geographical scale, that's very large scale and really global, but it can and should be studied from a micro sociological perspective because that's uniquely adequate. With macro sociology, you shift the perspective, you may ask, for example, what does the volume of foreign investments do to the economy in a particular country, then you look at volumes of investment and you look at the economy of a country and you are on an aggregate level, but as long as you are interested in the market as a system of practice and as a particular structural form in society, you need micro sociology to study that both methodologically and theoretically.

AP: Thank you. Now, I am going to ask you a question in preparation for the grand finale, so to speak. You've made so many conceptual contributions to the sociology of finance. Global microstructure and scopic system, synthetic interaction and synthetic situation, post social relationality immediately come to mind. This is just what I spontaneously enumerated. If you were to evaluate your own conceptual contributions, which one do you think is the most important?

KCC: From the ones you mentioned, I think it's still the maybe the notion of scopic system and scopic media, we'll switch to media. The switch to media didn't come naturally to me. I haven't studied media, really. But I had to somehow come to grips with the technology I saw in financial markets, and that's a story in its own right, and this coming to grips took me long enough. But the notion of media was important there, you know, scopic media, I think, is still very important. At one point in time, I thought, with the switch to algorithms maybe the screens are going black. Because algorithms don't operate on screens, you know, they don't need screens, they operate on an infrastructural level. But that hasn't happened at all, those screens have multiplied, you have now sixteens in front of you, and not six. And it also leads to the idea of a synthetic interaction, to have these algorithms as synthetic actors. An algorithm in my view is an energetic medium, it's a media structure that has acquired agency. These synthetic actors, if you call them that, they may need a full set of information infrastructures, they need a whole infrastructure to be able to work. So I think the scopic system notion leads to the synthetic actor notion or is combinable is with the synthetic actor notion via this notion of media and agentic media, and therefore in terms of a whole line of research and further concepts emerging from one, that is probably the most important in the set you mentioned.

AP: Thank you. And now, as I said, the grand finale, which is a set of several questions, I know that you have been writing a new book on finance, tell us more about it.

KCC: Well, this book is a much delayed one, on the currency markets I have studied. But it's also a relatively theoretical book in the sense that I had to come up with a notion of a financial market. I needed to differentiate between financial markets and producer markets, I had to

understand the role of sociality or what sort of sociality global markets financial markets have or can have, when these people do not even know each other. And when you cannot see them, you cannot say that they work together, or that they meet each other all the time. Some of them do at times, but mostly they don't. And so it was important for me to work out an idea of sociality, that's captured by the notion of a regime of attention and attentional integration, it's really the role of the human observer in these markets, all these participants observing the market simultaneously and deeply, it's a coercive regime of attention. And I link this to concepts of temporality. It is done now, but it took me a while to understand some of these things, especially now in the last part.

I had the problem of also experiencing the transition into algorithms, which happened mostly since 2005. So relatively soon, you know, after we started looking at financial markets, algorithms took off, they were no longer are invisible, they became very visible on screens, and they changed these markets structurally, completely, where they have taken over trading, and even where they are trading alongside human traders, they are changing the structure of the market. In that sense, you know, it's, it's been a long journey for me, but one that is rewarding. And only now, as only in the last year or two, I have the impression that I've also managed to approach the question of algorithm innovativeness, which leads to semi-autonomous markets. And this notion of semi-autonomy interests me, because it reflects back on the role of human beings, in a future when there will be more and more algorithms around. So the long journey is a long engagement, whether it ends in a book or not, is in a sense, not the most important, but it has been a long engagement. On the level of intellectual understanding, of intellectual fruitfulness, if you wish, it has been worth it. And it has prepared me for a next step into something else.

AP: Actually, I find, personally, that slow writing is much better with fast writing. It's this analogy with slow cooking versus, you know, fast food. And I always keep in mind that it took John Rawls, for instance, 10 years to write A Theory of Justice. So we have a good example here in Rawls's work. It takes time to write a good book.

KCC: Yes, yes. I have also learned a lot, I have to say, I have learned a lot from several of my original students. Certainly in the area of financial markets, I've learned most from you. I don't know whether this is relevant to an interview like this. But it's been productive to have that sort of partnership for me. It's very lonely, if you don't have anyone discuss things with, except colleagues in your department, who have all been recruited in order to complement each other and don't work in the same area in which you work. I was happy to have the opportunity to work with students in Bielefeld at the time before you came, and then with you when you came and also later in Constance with you, and to have the opportunity to discuss complex systems like financial markets. It's important to meet occasionally, and that one could talk. For example, if you do a book, if you continue your work on synthetic capitalism, I would like to be informed about that. And I think that's a very interesting, a very interesting project.

AP: I borrowed the concept from you.

KCC: I wish I had it myself. And it could be a very interesting, a very interesting undertaking. You have to put some of the semi-autonomy stuff in there. Because it fits, I think. One of the joys of such conversations is that you get insights from them. I get insights from talking to you, I get insights from my work, but also from that of others, especially yours. Aha, this how I should have thought about this. It might sound very egotistical, but it's very productive to have great interlocutors in one's area of work.

AP: I look forward to reading your book. Do we have a title?

KCC: Well, I still have the old title. I had the old title Maverick Markets. Really, I think of financial markets as a social form, but I don't simply think of them as networks, I think of them as this weird social form. And so I used the word "Maverick." I don't know whether this is going to survive the editor's gaze. But if you have a subtitle that is very descriptive, like "Currency Markets as a Global Social Form", this might work. I have completed the last chapter. I'm now going through chapters at the moment, the last two chapters in terms of bibliography and things like that. It's really finished. And that's unbelievable, even to myself, but it is the case. It's still a lot of work as you know, but it's good to be in this stage. It's good to be in this stage.

AP: This great news, I truly look forward to reading your book and get ideas from it.

KCC: The problem with these books is, as you progress over time towards the end you have new, interesting ideas and then you think you should put that in the beginning. But if I do that, I will make sure that I don't have to reconstruct the whole book around these questions. It would take me another 10 years if I did that, and I want to go on to something else.

AP: I didn't have this question on my initial list, but I'm going to ask anyway, what's your next project? I know you're not going to stop now.

KCC: Yes, now the next project is actually already a finance project. It is in collaboration with the University of Siegen and financed by the German National Science Foundation. We have an SFB there, and my part is really looking at algorithms as agentic media generally, and to also try and bring into the picture some of the producers of these algorithms and what they are doing. We are looking at three different kinds of what we call agentic media. The one is bots, communication media. We want to look at drones and autonomous flying machines too, how they are engaged by the worlds that receive their images, by the public who see them, how they are engineered on some level, and what role algorithms play in these things. We are really interested in semi-autonomy as a social form and what it implies. Agentic media and semi-autonomy are the top notions in that project.

AP: It's sounds fascinating to me. I can follow every single bit of it. I can see into it, almost literally see. Karin, thank you so much.

KCC: Thank you for the questions. The questions one gets are very important. And they got me to think.

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Margaret Somers in Conversation with Daniel Hirschman

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Abstract

Margaret R. Somers is a leading comparative historical sociologist and social theorist specializing in law and political economy, citizenship and rights, and the work of Karl Polanyi. After pathbreaking work early in her career on the origins of modern citizenship rights as well as on the logic and practice of comparative historical sociology, historical epistemology, and narrative analysis she turned to problems of escalating social exclusion, statelessness, and the threat to citizenship rights in the context of intensifying neoliberalism. Author of multiple articles and books and winner of numerous prizes, Somers is Professor Emerita of Sociology and History at the University of Michigan. Strongly influenced by the writing of Karl Polanyi, she has been a key contributor to debates on English legal history; dedemocratization and the rise of neoliberal authoritarianism; the political economy of predistribution, moral worth and market justice, and the political power of knowledge cultures and ideas. She also writes about contemporary social policy for a broader public in *The Guardian*, the *Washington Post*, *Open Democracy*, and other venues.

In this interview with Daniel Hirschman, conducted between 2021–2022 in a multiplicity of synchronous and asynchronous formats befitting the pandemic moment, Somers discusses her intellectual and political trajectories and how they shaped her intersecting research programs, including her latest work on moral economy, predistribution, and the contemporary authoritarian moment.

Keywords: Karl Polanyi; law and political economy; capitalism; comparative historical sociology; citizenship rights; moral economy.

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1 Becoming a Sociologist

Daniel Hirschman: How did you get interested in sociology? How did you end up studying it in graduate school? What connection, if any, did it have to the political movements of the time (my hunch, from knowing you a bit! is that your political and academic commitments have always informed each other, but I'd love to hear more about it!).

Margaret Somers: I was born a natural sociologist thanks to my Quaker upbringing, the result of the union between my Southern high Episcopalian mother and my Jewish immigrant father. More of a culture than a religion, Quakerism nurtures dissenters, social justice advocates, and anti-authoritarians. By the time I was 15 I was already protesting the Vietnam War, and my first year of college I participated in the famous March Against the Pentagon. Then, in 1968 the world turned upside down. MLK was assassinated; so was RFK. In horror I watched the Chicago Democratic Convention and, with the "whole world watching," the police bludgeoned anti-war protestors in Grant Park.

It was a bleak moment—time to get serious about the revolution. I left Vassar College and transferred to Merrill College, the "College of Social Change in the Third World," at the University of California, Santa Cruz, and became a Sociology major. Fortuitously, my undergraduate advisor was Wally Goldfrank, a student of Immanuel Wallerstein and Terrence Hopkins at Columbia, and thus by extension, of Karl Polanyi. What a lucky inheritance I had stumbled into!

I became an avid New Left feminist, and in 1969 organized "Bread and Roses"—the first feminist conference in California; started a daycare center for welfare mothers; worked with Cesar Chavez's Farmworkers' Union; with the Black Panthers in the "Breakfast for Children Program;" and built an anti-war Café for young people in Santa Cruz. All the while, I protested the war in Vietnam.

Rage and frustration set the stage for a fateful decision I made in college at age 19. The Vietnam war accelerated with Nixon's invasion of Cambodia, which led to the murders by the National Guard at Kent State in May 1970. The idea circulated that the student anti-war movement was pointless; only organizing the proletariat into a general strike would sufficiently pressure the government to change policy. Convinced, I was recruited into a Maoist Marxist-Leninist political organization called the Revolutionary Union (R.U.), a break-off from the defunct Students for a Democratic Society, which required me to admire Stalin and target practice under the Monterrey Freeway in preparation for the revolutionary "armed struggle." I can't overstate how that brush with delusion produced in me an aversion to orthodoxies of all kinds and set me on a path towards an affinity with Polanyi and other left intellectuals (such as E.P. Thompson and Hannah Arendt) whose life work embodied principled stands on the left against dogmatism.

After graduating, I became a teaching assistant at UC Berkeley for the wonderful young sociologist, Arlie Hochschild. I joined the editorial board of the new journal, *Socialist Revolution* (S.R.), which despite the overblown title was a journal of political economy and history founded by the late James Weinstein, John Judis, Eli Zaretsky, and others. S.R. articulated a democratic socialist-feminist politics, and for the first time I realized that I could not only make politics but also make and think ideas.

I then participated in establishing the New American Movement, the democratic socialist successor to SDS, and with Katherine Johnson wrote what became the founding document of the new organization. "Behind every Sexist stands the Boss: A Socialist Feminist Manifesto"

(Johnson & Somers, 1972)[1] argued that the site of *reproduction* was equally essential to capitalism as that of paid labor, and it needed to be recognized for its economic value to give meaning to the slogan that the "personal is the political." We made clear that the subordination of women is a thoroughly political phenomenon—not a "natural" one—that must be challenged through politics.[2] This early critique of "social naturalism" became an intellectual theme of my life's work.

When I moved to the Boston area I joined with Silvia Federici and Selma James in the "Wages for Housework" movement.[3] At the time, we were vilified by other feminists who believed the movement would keep women at home and disempowered. Our theory posited exactly the opposite: Wages for Housework stipulated that because women *already* generate economic value from reproductive housework, it was imperative to recognize and remunerate the worth of this work to the macroeconomy, as acknowledging women's full economic value was the *precondition* for our empowerment. It is hard to believe just how controversial this movement was in the early 1970s, as its claims have been demonstrated so consistently by mainstream economics.[4]

2 Graduate School

DH: So, now in Boston and thoroughly ensconced in the social movement scene, you then entered graduate school. What was that like? What were some formative experiences then? Who were your most important mentors? Interlocutors?

MS: Let me set the scene for you of grad school and left academia in the mid to late 1970s. As the truth about the Chinese cultural revolution was slowly coming to light and the Soviet Union was revealing its increasingly post-1968 (invasion of Czechoslovakia) tyranny, the atmosphere was filled with an exhilarating antipathy for "vulgar Marxism" in favor of a more critical and culturally inflected post-Marxism, one implicitly sensitive to feminism and race (although insufficiently) and especially to the state. The anti-Stalinist Marxists became heroes—the "early Marx" himself; Lukács (despite his dark side); The Frankfurt School—Horkheimer, Adorno, Marcuse, and, with mixed feelings, Habermas; and especially the "British Marxist historians"—E.P. Thompson, Hobsbawm, Hill, Hilton, Raymond Williams—and their fellow travelers, Ralph Miliband and Doris Lessing.

In graduate school at Harvard, I immersed myself in political sociology, social theory, and history, with Theda Skocpol and Daniel Bell my primary mentors, as well as John Brewer (English history), Peter Hall (political science,) Sally Falk Moore (legal anthropology). Skocpol was *the* rising star of the discipline and I owe much of my early career to our collaboration in

1. In preparing for this interview I only now discovered that our 1972 Manifesto is discussed by American historian, Sara M. Evans, *Tidal Wave: How Women Changed America at Century's End* (2003, p. 161), and discussed in depth in a recent Syracuse Masters' Thesis, Chris DiCesare's (2019) "A growing excitement that 'something was happening' ": A Rhetorical History of Gay Liberation and Socialist Feminism in the New American Movement between 1970 and 1980."
2. And while we didn't specifically address sexuality in that paper, later work on sexual politics and gay liberation perceived it as opening the potential for sexuality to be considered both within the sphere of production and the sphere of reproduction (DiCesare 2019, p. 79).
3. See Federici & Austin (2017) and Federici (2020).
4. Indeed, Silvia Federici was written about at length in the *New York Times Sunday Magazine* just a few months ago: https://www.nytimes.com/2021/02/17/magazine/waged-housework.html?searchResultPosition=1

what became a flagship article in the new subfield of comparative historical sociology (Skocpol & Somers, 1980), which first conceptualized three different methods of historical comparison. I studied social theory with Bell, who became my dissertation advisor when Theda was fired (temporarily) from Harvard. It was an odd match: I was a flaming New Leftist and Bell a well-known neoconservative. In truth, Bell was never as conservative as his peers such as Irving Kristol, and while he worried about detecting a Bolshevik hiding inside every student's backpack, his background as a Trotskyist arguing politics in the alcoves of CCNY in the 1920s and 30s was irresistible. To everyone's surprise, Bell was my biggest cheerleader.

DH: What was the intellectual space like at the time? What were the big questions that shaped your approach to sociology and history?

MS: I spent most of my graduate career at Harvard's Center for European Studies, where I happily shared an office with my fellow graduate student David Stark. The atmosphere was dominated by three formative debates.

"Bringing the state back in": The first was over the "role of the state," a question that surfaced in the 1970s with the anti-economistic tide in political science and sociology and a renewed interest in Weber, and made most famous by Skocpol (Evans et. al., 1985) herself. The main antagonists originally were both Marxists—Ralph Miliband versus Nicos Poulantzas, an Althusserian structuralist, who accused Miliband (1969) of "instrumentalism" (aka vulgar Marxism) for his linking the capitalist class directly to the structures and functions of government. But it quickly evolved into a much wider debate over the state's "relative autonomy" from capitalism, and reflected the anxiety of progressive academics not to be too closely associated with that dreaded "vulgar Marxism."

Feudalism to capitalism: Then there was the question of the "transition from feudalism to capitalism." Although the terms of debate were set by an earlier generation—Maurice Dobb (production) versus Paul Sweezy (circulation)—the entire discussion was transformed by the appearance of three new critical texts, Perry Anderson (1974), who centered his analysis on the "absolutist state," Immanuel Wallerstein (1975), who introduced "World-systems theory" into the social sciences, and Robert Brenner (1976), who brought attention back to the centrality of class. This debate became central to my dissertation research on the origins of English citizenship rights and working-class formation.

Structure versus agency: The third debate was between E.P. Thompson and Althusser, in which Thompson (1978) accused Althusser of a theoreticist structuralist dogmatism. When I started grad school Thompson's *Making of the English Working Class* (1965) was the bible of young New Left academics captivated by his focus on the power of human agency. He famously argued that the English working class "made itself" and was not a mere pawn of history. Thompson was more than a historian; he was a political activist, anti-nuclear activist, and most importantly, seen as the moral leader among the group of British historians (including Christopher Hill and Rodney Hilton, founders of the journal *Past & Present*) who resigned from the British Communist Party in 1956 to protest Stalin's suppression of the Hungarian revolution. Braiding together this moral stance with his culturally-oriented history inspired extraordinary devotion. Arguably, without Thompson, there never would have developed the next fixation of my generation, the "structure-agency" problematic (most associated with Anthony Giddens), which aimed to capture Marx's famous edict that (to paraphrase) "Men [sic] make history, but not under conditions they choose."

DH: Your approach to these three debates was shaped by your engagement with another

Karl, not yet named: Karl Polanyi. Tell us a bit about that. How did you first encounter Polanyi? What drew you to his work?

MS: I first engaged *The Great Transformation* (1944) (hereafter GT) in depth in the German historian Mary Nolan's graduate seminar. Polanyi's depictions of the cultural devastation inflicted by industrialization, commodification, and the New Poor Law on English working-class communities transfixed me. It seemed he had an almost spiritual connection to the soul of English working people and his descriptions of humans being tossed about and treated as disposable "fictitious commodities" were astonishingly evocative. From Polanyi, I learned that when writing about the depredations of market society such rhetorical affect was (and is) not only justified but *required*.

My new interest in Polanyi came to a lucky head when Theda invited Fred Block and me to write a chapter on his work for *Vision and Method in Historical Sociology*, the book that kickstarted the subfield of comparative historical sociology, which became Block & Somers (1984, 2014).[5] It's hard to remember just how few people knew of Polanyi in the late 1970s. We argued that his relatively neglected status among canonical theorists was explained by his theoretical liminality—he fell between the cracks of the dominant schools of Marxism and economic liberalism. Our title, "Beyond the Economistic Fallacy...", taken from Polanyi's (1977) own words, captured his rejection of their mutual overemphasis on "economistic" interests. Economic institutionalism—a focus on rules, policies, practices, and power, rather than naturalized systems—was/is the Polanyian antidote to the economistic fallacy.

DH: I'm sure you'll have more to say about Polanyi. But is there anything else you want to add about your time in graduate school, and your reflections on it from the present moment?

MS: Let me end with a paradox. Alas, escaping the economist fallacy was an aspiration deeply out of place with its time. I began grad school in the mid-1970s amidst an optimistic post-Marxism. But as we rejected the Soviets, embraced Eurocommunism and the "relative autonomy" of the state, and increasingly centered culture and institutions, by the 1980s the Thatcher and Reagan revolutions were turning the world into a Marxist caricature of the economistic fallacy: While we valorized "new social movements" and cultural discourses, the state *really* was becoming the "executive committee of the ruling class" and unions *really* were being decimated. Like ships passing in the night, throughout the 1970s and 80s neoliberalism was turning the *real* economy into a market fundamentalist nightmare, while many academics obsessed only on culture. It's not that this was wrong: Culture, power, and discourse *were* essential weapons in the war on social equality, racial justice, and gender equity. The mistake was that progressive academics too often framed them as alternative and superior perspectives to that of political economy, rather than as the modalities through which the neoliberal backlash was channeled. My generation came of age in liberal post-war America. It's hard to convey the shock of Reagan's election to the presidency, a shock we tempered by deluding ourselves that it was an aberration from what we had learned was the inextricable normative coupling of "democratic capitalism." How wrong we were.

3 Dissertation

5. The chapter was translated into Chinese shortly after its publication to become the Introduction for the first Chinese version of *The Great Transformation*.

DH: Having set the intellectual and theoretical scene, tell us a bit more about your empirical work. How did you come to write a dissertation about English working-class formation?

MS: A combination of Marx's Chapter X of *Capital*, "The Working Day," E.P Thompson's *The Making of the English Working Class,* and Polanyi's *The Great Transformation* catalyzed my love affair with English working-class history. But it was a love affair animated by the *history of the present.* That present, of course, was Reagan's sudden and brazen attack in the 1980s on the social state. Chapter X of *Capital* is Marx's analysis of how in the heyday of laissez-faire the government passed the Ten Hours' Act in 1847, limiting the hours of women and children's factory labor. How had such a powerful constraint on the power of property succeeded? Marx's answer didn't satisfy me, as it focused exclusively on elite interests. I was interested in the workers' Factory Movement: Why did they articulate their demand for limited hours in the language of legal rights? Why did they target the law's obligation to protect their rights and *expect* their rights to be honored? Why did working people fight economic tyranny in language of obligation between what I came to call "the people and the law"?

It was reading Polanyi that convinced me that to answer these questions I needed to investigate the *institutional roots* of the solutions they articulated. That led me to the new research in "protoindustry." Pioneered by German economic anthropologists (Kriedtke et al., 1977), this research broke pre-industrial European national economies into regions divided by soil types (arable versus pastoral), which indexed not only what kinds of livelihoods were supported but also distinct demographic, family, and inheritance patterns. Ninetheenth-century factory movement activists came almost exclusively from eighteenth-century protoindustrial wool-growing regions of the English countryside, which nurtured different economic and familial cultures from those of agricultural laborers in the manorial countryside. But what this could not explain was why they expressed themselves in the language of legal rights, unlike similar European protoindustrial regions. It was then that my research turned to mercantilism and the law (Schmoller, Bucher, and the German Historical School) and especially the long-forgotten English and Swedish historical institutional economists (Ashley, Cunningham, and Heckscher).

Unique among European nations, the English state extended national labor statutes across town and country; elsewhere, labor regulations were confined to urban areas (controlled by guilds) leaving the countryside to manorial law. Intended to be forces of domination and control, thanks to England's unique participatory legal apparatus, woolen and textile workers were often able to turn these laws to their advantage (e.g. turning *maximum* wage statutes into *minimum* wages in 1603) and create a tradition of labor rights tied to the rule of law. When I looked at the varying effects of the transactions between these two forces—the protoindustrial cultures and the English law—I found that only certain measures of the law, and only certain distributions of power and public participation in the law, allowed public spheres to be transformed into quasi-democratic arenas in which early market capitalism was contained by a political and legal culture of rights (Somers, 1986, 1993, 1994b, 1995c).

The whole process was an exercise in Polanyian institutional analysis. Modern capitalism was not born of the liberation of markets from state regulation. On the contrary, the industrial revolution developed directly from the institutionalized economies in the protoindustrial regions. Capitalism revealed itself to be a political and legal social institution in which property and the labor contract were thoroughly constituted through law, power, and social relationships (Somers, 1993, 1994b, 1995c). As "new" institutionalisms have come and gone over the last decades, it's puzzling that so few have returned to Polanyi's economic institutionalism (but see Cangiani, 2021).

4 Citizenship

DH: How did this historical work on the English working class set up your later research on citizenship?

MS: Only at the end did I realize that my dissertation was the story of the institutional origins of citizenship rights and identities, a topic long disappeared from the social science agenda.[6] These were claims based on the *rights-bearing* status as "freeborn Englishmen"—citizenship identities and rights-claims derived from regionally-specific institutional nexes of the law/state, civil society, and market dynamics, all mediated through contestation in the public sphere (Somers, 1993, 1994b, 1995c).

Attention to citizenship rights formation inevitably raised broader theoretical questions about a missing sociology of rights—an absence based on the social science aversion to the normativity associated with rights. But this was and is nonsensical: Social movement actors, among sociology's favorite subjects, display no such qualms expressing the centrality of rights, so neither should sociologists, as I argue in *Genealogies of Citizenship* (2008; and Somers & Roberts, 2008). In theorizing rights, institutionalism once again comes to the fore: However much we conceive ourselves as "rights-bearers," from a sociological perspective rights are not individual possessions; after all, they come and go when we cross borders or move through time. Instead, rights are better understood as the *subject positions* we occupy temporarily in shifting institutional and social arrangements, and it is this variable positionality that we need to foreground in rights analysis.

DH: Perhaps your most iconic empirical analysis of citizenship is your discussion of the aftermath of Hurricane Katrina in *Genealogies of Citizenship* (2008). Can you talk about that, and about how that analysis centers race?

MS: Hurricane Katrina teaches *in extremis* what social exclusion and rightlessness look like in the face of the de facto loss of meaningful citizenship rights. The book argues that in 2005, three decades of market-driven governance had already transformed growing numbers of rights-bearing citizens into socially excluded internally stateless persons. Their statelessness was based on social exclusion from any meaningful *membership* in the polity, a condition that long preceded the hurricane. So while they had all the *de jure* rights of citizenship—the right to vote, to assemble, and access the law—they were powerless to act on their citizenship rights since they had effectively been thrust outside of the circle of the power of the regulative state. I analyze this condition of stateless citizenship to be in part a result of the "contractualization of citizenship," in which the authority of the market transformed the meaning of citizenship from one of noncontractual shared fate to conditional privilege—making rights, inclusion, and moral worth dependent on one's market value in a quid pro quo contractual relationship. Deemed to be without market value, African Americans were excluded from recognition as moral equals by others. Just as Arendt demonstrated that absent state-centric citizenship, having nothing but one's human/natural rights was tantamount to death, so too were the Black stateless self-defined "refugees" of Hurricane Katrina left to their deaths.

But the contractualization of citizenship was only half the story; the other half was centuries of racial exclusion, apartheid and terrorism in the U.S. combined with grotesque accusations of Black citizens as morally unworthy "welfare dependents" and "welfare queens." "Hurricane Katrina" is what erupted from the toxic mix of neoliberalism, white supremacy, and

6. See Somers (2008, Chs. 1, 4), where I trace and explain the fall and rise of social science studies in citizenship.

the false universality of color blindness that created the conditions of internal statelessness and rightless citizens. Without *de facto* citizenship or recognition as moral equals, no other rights are possible—legal, civil, social rights are meaningless conversation when one is abandoned on a rooftop in a flooded city.

Against this perilous mix of white supremacy and turning citizenship into a market contract, *Genealogies* advances an alternative view of rights as necessary public goods rooted in an alliance of public power, political membership, and social practices of equal moral recognition—in short, what Arendt called the *right to have rights*.

Almost twenty years after Hurricane Katrina the situation—and the racism—has worsened. White supremacy and accelerating neoliberalism easily transform yesterday's "welfare queens"—already robbed of their moral worth and their right to be recognized as "real" Americans—into today's accused "voting cheats" and justifies the alarming speed by which the democratic rights of communities of color (not only Black, but also Latino, Asian, and Native American) are being dismantled. Populist authoritarianism colludes with reactionary state legislatures to use violence to disrupt, delegitimate, and criminalize the institutions and the procedures that facilitate the democratic citizenship of those they name as moral outlaws and threats to the body politic (Somers, 2022).

5 Narratives and Identities

DH: In addition to setting up your later work on citizenship, your work on English history led to perhaps your most influential paper, "The Narrative Constitution of Identity: A Relational and Network Approach" (1994a). Can you tell me a little about how this paper came to be written, published, and received?

MS: It was my historical subjects who made me aware of the high price we pay for the fact that much of what we observe in the world (or find in history) is incongruous with how our theories compel us to talk (Somers, 1992, 1996b, 1997). Lacking a vocabulary, we often misread social reality—seeing positive claims for legal rights, for example, as negative examples of "failed class consciousness," a case of what I have called looking through the lens of an "epistemology of absence" (Somers, 1989, 1996b). Finding a poor fit between available concepts and my historical findings, I started transgressing boundaries—between disciplines, between theory and history—and appropriating and reformulating concepts once alien to the social sciences. This led me to jettison the language of categories and attributes and instead to make *relationships* and *narrative* the central axes of analysis (Somers, 1992, 1994a, 1997, 2008 Ch.7; Somers & Gibson, 1994).

Polanyi's "economy as instituted process" (1957), which focuses on the causal effects of varying social arrangements and the *place* of the economy relative to other social institutions, catalyzed my interest in relationality, institutional connections, and networks. In lieu of the holism of "society" I conceived the concept of a *relational setting* (Somers, 1992, 1993, 1994a, 1994b)—a matrix of institutional relationships among economic, social, and political practices and institutions, making institutional configurations and social relational networks the analytic core of my methodology. It allowed me to disaggregate categorical entities such as "the economy" and to reconfigure them as institutional clusters through which people, power, and organizations are contingently connected and positioned in empirically shifting relationships.

I did the same kind of work on the concept of agency. My English historical subjects explained their actions in the arc of historical time and memory, yet in the 1980s the concept

of narrative in the social sciences was derided as devoid of theory and excluded as the "epistemological other" (Somers & Gibson, 1994). I nonetheless appropriated and transformed it into a staple of identity analysis and dubbed the concept of *narrative identity* to capture actors' spatially-variable "place" in networks of (breakable) rules, (variable) practices, binding (and unbinding) institutions, and the multiple stories of family, nation, or economic life. While a social category approach imputes to actors internally stable properties, narrative identity embeds historical subjects within relationships that shift over time and space, and discerns the meaning of events and behaviors only in temporal and spatial relationship to others (Somers, 1992, 1994a, 1997).

Over time I've continuously used the generativity of narrative in several conceptual innovations—*narrative justice* in explaining the plasticity of law (Somers, 1993, 1994b), the historical method of *causal narrativity* (Somers, 1996a, 1998), the naturalization of narrative assumptions to explain the gatekeeping epistemics of *knowledge cultures* and *metanarratives* (Somers, 1995a, 1995b, 1999, 2008 Chs. 5, 7), and the concept of the *conversion narrative* (Somers & Block, 2005; Block & Somers, 2014), to describe a rhetorical tool used to convert people from one belief system to another by telling causal stories that change perceptions of reality.

6 Historical Epistemology

DH: Beyond your turn to narrative, you also published several influential articles on the logic of historical research. What inspired this line of work? Does it also start with your dissertation and your deep engagement with the English working class?

MS: Yes, another dimension of my thinking that evolved from my English historical research falls under the rubric of what I dubbed *historical epistemology,* a term I use to capture the idea that the history and development of a thing (and not just the logic of its construction) can tell you something fundamental about its nature. Since collaborating with Skocpol on comparative history (Skocpol & Somers, 1980), I had pondered questions of historical methodology. In the late 1980s I brought together historical methodological concerns with those in the philosophy of science and social science to develop my thinking in historical epistemology. The term is purposefully oxymoronic: It intentionally challenges the ahistorical requisites of standard epistemology and instead proposes that all our knowledges, our logics, our presuppositions, indeed our very reasoning practices, are indelibly marked with the signature of time. They are "history laden"—a phrase meant to evoke, to disturb, and to invert the well-known Kuhnian claim that all data are "theory laden." A history-laden perspective suggests that much of social theory is founded on unquestioned taken for granted historical claims, as I argue in "Where is Sociology after the Historic Turn" (Somers, 1996a; and see Somers & Gibson, 1994).

My most ambitious effort at combining historical methodology with the philosophy and sociology of science is " 'We're No Angels': Realism, Rational Choice, and Relationality in Social Science" (Somers, 1998).[7] "Angels" addresses the rational choice critics of historical sociology who lament its "empiricist subversion" of the theoretical aims of social science. I argue in reply that recent developments in the philosophy of science cast doubts on the critics' un-

7. This became the touchstone of an AJS Symposium on Historical Sociology and Rational Choice Theory with Michael Hechter, Edward Kiser, Craig Calhoun, Raymond Boudon, and Jack Goldstone [*American Journal of Sociology*, *104*(3), 1998].

balanced anti-historical views of theory, and introduce recent post-Kuhnian developments in the philosophy of science that I dub a *pragmatic historical realism*, which supports a relational and problem-driven approach to explanation, and a more *causal*, less law-like, view of theory. I also defend the epistemology of historical sociology against the potential tyranny of rational choice theory's anti-democratic implications by asking: If theoretical entities trump the empirical, how else but by greater power alone will social truths be adjudicated?

DH: Once more, your approach seems to be about middle paths through polarized theoretical terrain, here rejecting both theoreticism and empiricism in favor of something pragmatic. How did you put this approach into practice? Does this set up your work on "the historical sociology of concept formation"?

MS: Yes, I'll explain how I got to this. Standard Anglo-American citizenship theory makes citizenship an ancillary effect of capitalist development. Reflecting on the difficulties of convincingly recounting my alternative story, I came to recognize the power of a metanarrative—a gatekeeping naturalized "knowledge culture" that, like a paradigm, defines the spectrum of allowable propositions and adjudicates what counts as reasonable evidence (Somers, 2008 Ch.7, 1999, 1995b). Clearly, it was insufficient to simply tell my story; I had to destabilize and "unthink/untell" the hegemonic one, much of which is based not on empirical granular English history but on a grafting together of assumptions drawn from Locke's social contract theory with a naturalized neoclassical economic history. The result is a heroic narrative of the market being liberated from the tyranny of the state and setting free individual liberties based on property rights. My work found just the opposite—that without early social rights as foundations, more encompassing citizenship rights would not have been possible. That centers the law, the state, and "the people" at the heart of capitalism (Somers, 1994b).

None of this work would have been possible without my having formulated a *historical sociology of concept formation*, a method which problematizes the complex and skewed relationships between the practical world of political economy and social organization, and the conceptual vocabulary and cognitive maps that oblige us to think in certain constrained ways (Somers, 1995a, 1995b, 1999). By deconstructing concepts through historicization and reflexivity, an historical sociology of concept formation reveals that concepts taken as given have histories of contention and transformation—histories not unlike the social phenomena that we normally study. By subjecting the concepts of civil society, the public sphere, social capital, and even citizenship itself to an historical sociology of concept formation, I was able to "denaturalize" that which has hardened into the frozen thinking of hegemonic knowledge cultures (Somers, 1995a, 1995b, 1999, 2005, 2008 Ch.5, 6, 7).

Underlying all this work has been the central interplay between history, ideas, epistemology, and theory. Although I have tried to make them to stand on their own, many of the theoretical concepts and epistemological arguments I have developed have been driven by empirical historical puzzles, thus making the theoretical project at once an historical sociology. And because I believe that underlying most social theory is a particularistic view of historical events and causal processes in the making of the modern world, I have consistently argued that theoretical renewal in sociology requires a simultaneous historical deconstruction of that metanarrative/knowledge culture of Anglo-American citizenship theory. For a sociology of citizenship and rights, it is not possible to privilege either theory or history; both must proceed at once.

7 Encountering Karl

DH: Let's return to your work on Karl Polanyi. Starting with your 1984 paper on Polanyi's "Holistic social science," to your extensive explorations of the concept of "market fundamentalism," through to writings in the past year on how Polanyi can inform our understanding of the work of Thomas Piketty and the politics of "predistribution," you've explored many themes and their applications. So I'd love to talk a bit more about this.

MS: Let me start with my collaborative work with Fred Block on welfare reform, ideational embeddedness, and the rise of market fundamentalism. When Clinton's 1996 Welfare Reform Bill passed, its resemblance to Polanyi's discussion of England's 1834 New Poor Law in GT was too striking to ignore, as both eliminated versions of social citizenship for market-based poverty policy. In Block & Somers (2003, 2014) and Somers & Block (2005) we compare these two "welfare reform" bills and treat them as indicators in the rise of laissez-faire and neoliberalism, respectively. Most notable was the little noticed influence of early English welfare history on the political discourse of neoliberal American poverty policy. Like their English predecessors, American conservatives mobilized Hirschman's (1991) "perversity rhetoric" to forge their most significant political achievement—reassigning blame for the poor's condition from "*poverty to perversity*," in which structural problems of poverty are discredited as *empiricist illusions* while the *real* problem is redefined as poor people themselves—their sexual promiscuity, personal irresponsibility, and cultural dependency. Coupling economic sociology with a sociology of ideas, we devise the concept of *ideational embeddedness* to characterize the power of such ideas to shape, embed, and change markets. We argue that *ideas count*; but *not all ideas are created equal*. Only a successful few can fuel radical market transformations, in this case drawing on the prestige of Newtonian physics to create a structure of unfalsifiable assumptions to analyze the consequences of welfare that are immune to empirical disconfirmation.[8]

DH: How has your understanding of Polanyi evolved since you first read his writings?

MS: As the Polanyi fan club has grown over the years, so have claims to canonical knowledge of what he "really meant" (Somers & Block, 2020b). Since some of my views are idiosyncratic, I've taken to calling what I do simply "Polanyi-inspired" political economy (Somers forthcoming a, 2021). So, for example, there's an ambiguity in GT between Polanyi's allusions to the 19th-century economy's "disembeddedness" from politics and society, and his argument that the self-regulating market is an impossible "stark utopia," since all markets are constituted by states, laws, and social relations. I'm now convinced that for Polanyi, economic "disembeddedness" is ideational, never actually institutionalized. Instead of confused ambiguity, he's theorizing capitalism's bifurcated political economy: What *appears* to be an autonomous self-regulating market is in fact organized by legal and political engineering. The economy, in short, is *always* an "instituted process" (Polanyi, 1957; Somers & Block, 2021).

Performativity of political economy. Polanyi is clear, however, that while the market's autonomy is not empirically "true," as an ideational regime it is very *real;* the self-regulating market has *causal powers* to force the world to conform to its image. This makes Polanyi as much an *epistemic* political economist as an institutionalist one. By demonstrating how 19th-century social science (political economy) outweighed the effects of technology in the industrial revolu-

8. This theoretical achievement explains why welfare reformers—both then and now—have been surprisingly casual about developing serious empirical support for their arguments—yet with no seeming loss of persuasiveness.

tion, GT (pp. 124-125) distinguishes between scientific "truth" and ideational *power*—or *social facticity* (Somers, 2018). He thus anticipates by decades the "performativity of economics," by which certain economic fictions can "make themselves true" by becoming the *engines* of social change (Bourdieu, 1998; Callon, 1998; MacKenzie, 2006; Block & Somers, 2014, p. 107).

Ideational embeddedness. Reading Polanyi as an epistemologist led to our concept of *ideational embeddedness:* Markets, even "free markets," are not only constituted by coercive rules and institutional arrangements; they are *also ideationally* embedded by epistemic regimes. Neoliberalism did not disembed markets; instead, it institutionalized a regime change in which a new set of ideational stipulations designed to coerce the poor to be more responsive to market signals displaced the previous one. Once we acknowledge that ideas *do* exercise market-making powers it becomes clear that many battles over social and economic policy should be redefined not as conflicts over *whether* but over *which* ideational regime will do the embedding.

Ideational embeddedness is an analytic tool to diagnose the nature of market processes, but the strength of ideational influence relative to other factors is entirely empirical. We are *not* arguing that ideas alone can explain social or political or economic outcomes—these are determined by a complex mix of structural and ideational factors, especially the distribution of institutional and economic power. The leap from identifying causal ideational mechanisms to causal outcomes is a misguided leap we don't make (Somers & Block, 2005; Block & Somers, 2014).

Market naturalism. The most important instance of ideational regime change was the rise of $18^{th}/19^{th}$-century social and market naturalism, the make-believe story by which classical political economy reinvented the socioeconomic universe (although not the political). Society is not "like" the natural world; rather, the natural and the social worlds are one and the same and subject to the same self-regulatory biological laws. By endowing the economy with the same self-equilibrating dynamics as those of nature, market naturalism scientifically justified the economy's capacity to self-manage distinct from the state, as only an entity anchored to the self-propelling laws of nature could usurp the government in its own administration. More importantly, social naturalism remade the ontology of the poor (or people who must work for a living) from moral beings into biologized creatures incentivized exclusively by *biological instincts* of hunger and pain, no different from bears or other animals. This justified social policies designed to trigger (incentivize) biological drives rather than human morality or social obligations, instincts that the political economists made to serve as proxies for economic motivations and activities. It is an argument about "human nature" still mobilized today to justify cruel conservative social policies (Somers, 2021, 2020b, 2008b; Somers & Block, 2014, 2005).

8 Market Justice, Predistribution, and Dedemocratization

DH: Your most recent work in this vein returns to the notion of "moral economy" and debates over morality and markets. Can you tell us a bit about what you are working on now?

MS: Yes, of late I've felt the urgency of coming to grips with capitalism's moral economy, an urgency motivated by how often I've seen it argued that today's egregious levels of social exclusion calls out for a new moral economy, often referring to Polanyi for inspiration. But one of Polanyi's great contributions is to disabuse us of the sentimental delusion that when it comes to the economy, morality has a progressive heart. Too often misread as a story of confrontation between morality versus markets, GT instead makes clear that *all* economic matters traffic in

morality, and that those who fail to reckon with its moral justifications will fail to understand the power of capitalism.

A moral economy is a normative apparatus that justifies certain economic arrangements on the grounds that they produce morally superior—fair and just—outcomes. Capitalism's moral economy is *market justice*—the normative claim that distributional outcomes produced by legally voluntary market transactions operating in an allegedly neutral price system are by definition morally just. Market justice teaches us that today's most grotesque forms of inequality, economic domination, and dedemocratization are not symptoms of the absence of morality; rather, they are signature expressions of the dominant moral economy of market justice (Somers, 2021, 2020a). To explain, I point to its three most significant diktats:

1. Market justice provides the original justification for inequality, as it declares earnings and wealth result are produced by nonpolitical, nonbiased *natural* market forces. As the basis of neoclassical economics' *marginal productivity theory*, market justice subjects humans to a *tribunal of moral worth* based on the invention of "just deserts" (Somers, 2021, 2020a, 2017).
2. Redistribution is theft. Alleviating suffering through redistributive social provisioning disrupts the organic autonomy of market forces, thus threatening efficiency and growth. More importantly, it morally violates the just distribution of rewards produced by natural laws, as per "everyday libertarianism" (Murphy & Nagel, 2002).
3. Democracy is a moral and mortal threat to market justice, as it "politicizes" the neutral economy and preys upon property rights in the effort to redistribute from the "deserving" to the "underserving." The threat mandates *dedemocratization*, targeted above all at those accused of violating market justice—people of color, especially African Americans, and marginalized Others (Somers 2017, 2021, 2022).

DH: A related contemporary conversation you are engaged with concerns the idea of "predistribution," or the importance of centering how regulation and state action shape market income (rather than simply redistributing it after the fact). How do you approach the topic?

MS: Market justice tells us that inequality evolves from impersonal objective prepolitical market forces that we interfere with at the peril of market distortions and threats to economic freedom. Naturalism is the predicate of market justice: It is wholly dependent on the market's alleged neutrality and freedom from the coercions of politics and governance. Destabilizing market justice thus demands *denaturalization*: Actual markets work through the very power, coercion, and violence abhorred by the market naturalist ideal; free markets do not exist in the wild but are engineered to appear as such. The market is itself an allocative institution of power engineered through institutionalized *predistribution*.

Predistribution[9] conveys the Polanyian insight that inequality is engineered by government policies and legal institutional powers, by private law and infrastructural social relations. The concept plays on the more familiar one of redistribution: Whereas the latter focuses on government policies *outside* the economy that tax and *re*distribute income and profits after they have been earned, *pre*distribution exposes how government policies and legal powers shape market dynamics *inside the economy* and determine those (usually unequal) pretax incomes and profits

9. The term predistribution is usually attributed to political scientist Jacob Hacker (2013), and for having been put into currency by Ed Miliband in 2012, then leader of the UK's Labour Party. See Somers (2018) and Somers & Block (2020a) for the Polanyian roots of the term.

in the first place. It thus upends the binary that attributes politics, power, and governance to the public sphere, and freedom *from* power to the private. By putting law and government engineering into the heart of the price mechanism, predistribution puts an end to the myth of the stateless market.

As I argue in Somers (2020a, 2021), denaturalizing market justice reveals the exercise of power at its heart: Power, not neutrality or nature, decides the economic and moral status of economic actors. Market outcomes, especially our egregious levels of inequality and social exclusion, reflect not merit, justice, or fairness but the unequal bargaining power engineered into labor relations, just as corporate profits reflect not the price mechanism as neutral regulator but government-protected monopolies. Predistribution exposes how the fairness, worth, and desert attributed to market distributions are reflections of power and coercion smuggled into the "morality-free" economy under naturalism's protective cover.

Predistribution also makes nonsense of the belief in neoliberal "deregulation." To be sure, neoliberalism has for over four decades thrived under the deregulative ideal. Yet no more than laissez-faire, neoliberalism has never been about market freedom from power; rather it is a project that deploys political and legal power to reshape and reconstitute the market by accelerating monopoly power, driving bargaining power away from working people, and insulating against participatory rights of the citizenry—all to the effect of redistributing wealth and income upwards. The characteristic trait of capitalism's "alchemy of misrecognition" (Somers, 2018) is that this has been accomplished all the while convincing us that the ensuing maldistributive outcomes are the result of the free market at work. Deploying predistributive political engineering to reorganize the economy by seizing and repurposing law and state power *under the guise of returning to the free market* is the signature achievement of neoliberalism (Somers, 2018, 2021). Deregulation is simply the term of neoliberal art for upwards redistribution. Predistribution teaches us that market justice fashioned a morality of deceit that occludes the political power that advantages wealth, all the while misdirecting us to see maldistributive market outcomes and the suffering they inflict as the result of natural free market forces and the unimpeachable morality of market justice.

Predistributive analysis has also been at the center of my recent work examining Piketty's influential work through a Polanyian lens. In his first acclaimed volume, in which neither predistribution nor Polanyi's name appears, Piketty (2014) develops the famous r > g model, implying that inequality results from economic relations free of political power. Predistributive analysis disrupts Piketty's misplaced naturalism by centering legal and institutional power (Somers & Block, 2020b; Somers, forthcoming a). *Capital and Ideology* (2020), however, names Polanyi as one of the book's major influences. This provides a kind of natural experiment: What difference does it make to his thesis ex-ante Polanyi versus ex-post? Apparently, a great deal, as Piketty (2020) prioritizes institutions, ideology, and politics over naturalized economics, and attributes greater causality to predistribution than redistribution in driving inequality.

DH: A last strand of your work tried to bring this Polanyian approach to predistribution specifically back to questions of democracy — coming back, in some sense, to your earliest interests in citizenship and rights, armed with a few more decades worth of theoretical and empirical equipment. What's next for democracy? How can this moral economy lens help us through our present predicaments?

MS: *Predistributive dedemocratization*. Once we recognize that freedom of the market from political power is but a powerful performative fiction the critical question is whether those powers and coercions will be democratic or authoritarian. Yet from the outset, as in

market justice's anti-democratic diktat enumerated above, *market society has aspired to be free from democracy*. Although the history of repressing democracy in the public sphere (voter suppression, Jim Crow, etc.) has always been the more conspicuous, what I've dubbed as *predistributive dedemocratization* is more foundational as it hardwires—and naturalizes—*dedemocratization* into the heart of the economy by excluding ordinary citizens from exercising democratic influence over the processes that shape their livelihoods (Somers, 2021). Polanyi demonstrates how the American Constitution prevented democratic "contamination" of market processes by instituting a constitutional firewall between economy and politics, thus policing the boundaries of politics and shielding market "efficiency" from political and moral considerations of equality and distribution. Predistributive dedemocratization also works through the disproportionate property rights allocated by the state to the firm that insulates it like a "private government" (Anderson, 2017) from the democratic influence of its own workforce, as per legal realism.

Predistributive dedemocratization thrives on the threat of pitchforked masses preying upon defenseless property owners. Four decades of neoliberalism have rendered ludicrous this narrative of capital under assault by a rapacious democratic mobocracy. Predistributive dedemocratization has instead institutionalized structural bulwarks deep inside the market economy to bar the democratic citizenry from wealth and property. From the constitutional firewall between politics and property, to the prohibition of workers' voices in "private governments," to the judicial support for monopoly and monopsony, to the dedemocratized central banks and global financial organizations, naming the problem as "too much democracy" is risible.[10]

History, in fact, demonstrates the reverse — predistributive dedemocratization allows property owners to prey upon the demos. Indeed, it has been the refusal of business interests to tolerate socioeconomic reforms that precipitated not merely plutocratic control of governance but moves to eliminate democracy altogether. Writing in the early 1940s at a moment of indeterminate futurity, Polanyi ends *The Great Transformation* by forcing us to confront the fork in the road that presaged global fascism in the 1930s. It was precipitated by an impasse between the forces of social democracy, primarily seated as labor parties in Parliaments, demanding economic reforms, social protections, and democratic control over currency on the one side; on the other, the global capital elite, operating fully according to the diktats of the gold standard, refusing to concede to the slightest whiff of "interference" in currency or national economies. The fascist solution to the impasse, Polanyi writes, "can be described as a reform of market economy achieved at the price of the *extirpation of all democratic institutions"* (GT, p. 245). He continues:"The victory of fascism was made practically unavoidable *by the liberal obstruction of reform* involved planning, regulation, or control" (GT, p. 265, italics added). Faced with the choice between authoritarian protection of capital or social democratic reform, global elites decried the latter as tantamount to the expropriation of property and cried out in anticipation of what Hayek called "the road to serfdom."

Echoes of the calamitous fate of Europe in the 1930s are found in today's extreme social exclusions and surging market authoritarianism. The complicity between the American Republican Party, a colluding juristocracy, and a neofascist populist base underscores that it is not free markets or market justice that is threatened by democracy, but democratic citizenship that is threatened by the contemporary moment of what Polanyi saw as capitalism's innate antidemocratic ethos. What Polanyi can't help us with today is democracy itself being used as

10. The neoliberal campaign against "excess democracy" began with public choice (Buchanan and Tullock, 1962) and the Tri-Lateral Commission (Crozier et al., 1975) and see MacLean (2017).

the means to achieve autocracy. In his time, democracy was the alternative to fascism; today, seizing the machinery of democracy is often the pathway to autocratic success. As authoritarianism has spread, so have sham elections in which the outcomes are predetermined by control of the electoral apparatus. I wish I could be more optimistic.

DH: Thank you so much for your time. To end on a Polanyian note, perhaps we can hope that the clear-eyed analysis you laid out may help us resign ourselves once more to the social realities we confront, to accept "the reality of society," and in so doing gain (as Polanyi predicted) an "indomitable courage and strength to remove all removable injustice and unfreedom." It sounds like we're going to need it!

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Helga Nowotny in Conversation with Elena Esposito

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Abstract

Helga Nowotny, Professor emerita of Science and Technology Studies at ETH Zurich, is a leading scholar in the social studies of science and technology. In her extensive publications she dealt, among other topics, with social and individual structuring of time, technological innovation, uncertainty, social effects of AI, and the interaction between biological life and social life. Always intensely engaged in research policy, Nowotny is one of the founding members of the European Research Council and was its President from 2010 to 2013. In this conversation with Elena Esposito, she talks about her scientific biography, the role of technologies in the experience of time, and the relationship between STS and sociology of science. Drawing on her experience in the organization and funding of science at EU level, she also reflects on the relationship between research and science policy and on the ongoing transformations in the way of doing research and in gender issues.

Keywords: Social time; STS; sociology of science; social impact of algorithms; science policy; gender.

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Elena Esposito: You have had a very diverse scholarly path, from legal studies to sociology, from Vienna to the US and then back to Europe. Has this combination of cultures and academic styles affected your research and approach?

Helga Nowotny: I would call my biography nonlinear. It had unexpected swerves, was uneven and certainly not planned this way. I switched from law to sociology because I went with my husband to New York. I had been an assistant professor for penal law and criminology at the university of Vienna — what to do with this in New York? A position closest to what I had done before was in Philadelphia, but commuting was out of question. From one day to the next, I decided to study sociology at Columbia University with Paul Lazarsfeld and Robert Merton. It was an exciting time with the ongoing war in Vietnam and students protesting. My Ph.D. defense took place in Lazarsfeld's living room as the university building was occupied. When I returned to Vienna I joined the Institute of Advanced Study as head of the Department of Sociology, a postgraduate institution founded by Lazarsfeld and Morgenstern to introduce the quantitative social sciences at their highest level to Austria. Later, I spent a sabbatical year in Cambridge, UK, hosted at King's College. Tony Giddens asked me to be a tutor for a remarkably well designed course: "Science, Knowledge and Belief". It broadened my knowledge of the sociology of science as taught by Merton to include anthropology and contributed to anchor me in what later would become STS. Back in Vienna I became the director of a UN-affiliated institute that was doing Social Research. There was no position for me at the university, as the system was very closed. It was a bit absurd: despite a Ph.D. from Columbia University, a doctorate from the University of Vienna and having been an assistant professor, I needed a *Habilitation* to apply for a professorship in Austria or Germany[1]. And this brought me to Bielefeld which was a young so called, "reform university" in Germany. The academic environment was open and friendly and I was accepted for my *Habilitation* there. Niklas Luhmann was in my committee, along with Everett Mendelsohn from Harvard and Peter Weingart, who was the main person to sponsor me. It was a high level committee, and Luhmann was very supportive. I think what connected us was that he knew I had studied law before. After the *Habilitation* I was expected to teach in Bielefeld as part of my "*venia docendi*", so for a couple of years I took the night train to go there. In 1981, to my surprise, I received an invitation to join the newly founded *Wissenschaftskolleg*[2] in Berlin as a Fellow. Why me? I asked. Later I discovered that they had first invited Norbert Elias who declined and proposed to invite me in his stead. During my time in Bielefeld I had become a close friend to Norbert Elias. We both lived in the ZiF[3] which had a swimming pool which Elias used every morning. As he was my neighbor and I had a car, I offered to buy whatever he needed for the weekend. He lived alone and after a bit of hesitation he gave me his shopping list. This is how it started. Soon he invited me to accompany him on his daily afternoon walks in the *Teutoburger Wald*[4] behind the ZiF.

1. The *Habilitation* is an additional qualification at a higher level than the doctoral degree, required in Germany and Austria to achieve the rank of a Full Professor.

2. The *Wissenschaftskolleg zu Berlin – WiKo* (Institute for Advanced Study in Berlin) is an interdisciplinary institute dedicated to research projects in the natural and social sciences: https://www.wiko-berlin.de/en/institute. Helga Nowotny was a Fellow there in 1981-1982 (inaugural year) and in 2003-2004.

3. The *Zentrum für interdisziplinäre Forschung – ZiF* (Center for Interdisciplinary Research) is an Institute for Advanced Study in Bielefeld, Germany: https://www.uni-bielefeld.de/ZiF/. The ZiF promotes and provides premises for interdisciplinary and international research groups.

4. The *Teutoburger Wald* is a range of low, forested hills in the German states of Lower Saxony and North Rhine-Westphalia.

We had fascinating conversations about his life, his work and the world as he saw it. It was a very special experience for me to get to know this remarkable and very special man. Have you met him in person?

EE: Unfortunately not, I was in Bielefeld later. Was your connection with him only personal or does it also concern your work? In your books you quote him, but you deal with different topics.

HN: Yes, there was some overlap. We were both in the Editorial Board of the Yearbook in the Sociology of Science which was an early "scientific home" for me and he participated in some of our annual meetings. We also shared an interest in the topic of time, although my interest was still at a nascent stage. You know... sometimes people ask me who is my role model. I don't think I ever had a role model, but the closest person would be Norbert Elias. I admired his life and his work and how he had achieved to merge the two: despite having lived through some of the horrors of the 20th century as a Jewish refugee in exile his intellectual trajectory was marked by a profound humanistic outlook and his persistent quest for understanding how humans could collaborate in the evolving "civilizing process" (Elias, 2000), yet fall back at any moment into committing the most atrocious crimes against each other. The range of his interests was phenomenal. After his retirement from the University of Leicester — his first secure academic post at the age of 57 — he went to teach at the University of Ghana. Let me return to the *Wissenschaftskolleg* as it is connected to the story of my *Habilitation*. In Austria my *Habilitation* in Bielefeld was not recognized and my entry to the university remained just as closed as before. However, when I received the invitation to Berlin, I decided to put exactly the same material that I had submitted for the *Habilitation* in Bielefeld into an envelope and sent it to the University of Vienna. I still remember that I had to paste *Stempelmarken* on the envelope, a kind of tax, for the then very high sum of 990 shillings. The *Stempelmarken* had to be bought in a kiosk operated by tobacconists, but none of them had so many stamps. I therefore had to make the rounds and collect *Stempelmarken*, which was just ridiculous. But I did it and left for Berlin. During my absence the University of Vienna granted me my second *Habilitation*.

EE: So you have two PhD's and two *Habilitation*. A not linear trajectory also has burdens...

HN: Yes, but looking back I think that I had a much more interesting life and career than most of my former male colleagues who all had a very smooth career: they started working with their professor, in those times often a relationship involving dependence also in personal terms, then moved up the next career step and finally became professors for the rest of their life. Smooth, predictable, and often rather narrow. On the other hand — I enjoyed my independence, but there was a price to be paid. I knew that I could not expect to enter academic life in Austria or Germany based on intellectual merit and was not willing to compromise. I had to lead a kind of "double life": as director of the UN-affiliated research center moving in an international policy environment while pursuing my academic work in STS in my "spare" time. I soon became a self-taught expert in time management. I also knew that I had to remain in the international sphere, as the national academic environment would be far too restrictive. I had always been international and felt very much at ease as part of the scientific community that sociology of science represented. It soon became known as STS — science and technology studies. As a new academic field it was still in its founding phase, distributed between Edinburgh, London, Paris, Bielefeld and Harvard. At one point I had to decide between keeping my contacts with the US or to concentrate more on Europe, which I did.

EE: This leads to my next question. You came from a background in sociology of science, in the tradition of Merton and Lazarsfeld, and now you are completely open to STS. How do you see the relationship between the two approaches? Many people say they are very different, others say that STS is exactly the development of the tradition of sociology of science. How do you see it? What are the elements of continuity and discontinuity?

HN: What happened represents a kind of epistemic watershed. Robert K Merton, the father of sociology of science, was predominantly interested in the analysis of social structures that constituted and upheld science and never touched the content of scientific knowledge. The switch came when Bruno Latour and Steve Woolgar published *Laboratory Life* based on their field work at the Salk Institute (Latour & Wooglar, 1986). The subtitle of the book — "The Construction of Scientific Facts" — says it all. By following the scientists, not the science, as Latour had admonished, the "deconstruction" of what before had seemed "off limits" to social scientists, began. Much of it was perceived as provocation by the natural scientists and the unfortunate "science wars" only underlined the many misunderstandings that persisted. But the gate had been opened and STS enabled us to ask new questions by empirically investigating practices and institutions, meanings and narratives as well as the impact of science and technology on the social worlds that people inhabit. Delving into this world where scientific knowledge was co-produced was new and fascinating. On the methodological side STS borrowed the anthropological practice of doing field work. Laboratory studies remained rare, as we soon realized that the social world had become a laboratory. But the question of the relationship between sociology and STS is still open. At ETH Zurich where I had been Professor for STS my collaborator and I published a piece on what STS and sociology could learn from each other (Guggenheim & Nowotny, 2003). Sociology had its ups and downs — right now I see it on the upswing, regaining visibility and relevance — but both can and should benefit from each other. Sociology has yet to come to terms with the role and future-shaping impact that science and technology have in contemporary societies. STS rightly speaks about "co-production" or "co-evolution". On the other hand, STS has grown and diversified. Sometimes it risks to become too micro-oriented, losing itself in a "language game" that is applied indiscriminately to whatever it studies. An example for mutual complementarity would be one of the latest concepts that is gaining ground in STS: infrastructures and infrastructuring. We don't know how Merton would have approached it, but STS looks at the *processes* of infrastructuring. This is a much wider and dynamic perspective as infrastructures are distributed across many domains and sub-subfields. They are planned, or not; need maintenance and repair; depend on a variety of different kinds of resources and their interconnections and so forth. However, one can easily fall into the trap where everything becomes infrastructuring and thus devoid of exploratory or explanatory value.

EE: It would be so interesting to go on talking about this, but we should also move to another topic, because I think that one cannot have an interview with you without mentioning time. You dealt with a lot of different topics, but time seems to be a sort of common thread. Some decades ago, you proposed the very influential concept of *Eigenzeit* (Nowotny, 1989), linked to the notions of extended present and of changing boundaries between past, present, and future. What role do technologies play in the experience of time? How did you move from *Eigenzeit* to predictive algorithms, and how are they connected?

HN: Let me take one step back. I started to work on time when I had a sabbatical in Cambridge, UK. My personal life was a bit turbulent at that time, and arriving in Cambridge meant that all of a sudden I had time to read, think and write. Practically, I had no obligation and

found myself confronted with an existential question: "how come that all of a sudden I have so much time, and what does it mean?" The Provost at Kings College was a famous anthropologist, Edmund Leach. We met and spoke about my interest in time. At one point he said: "look, if you want you can use my personal library, just tell my secretary when you want to come". So, I started to read voraciously whatever I could find about time, mainly anthropological literature which was just fascinating. By chance I discovered the existence of an *International Society for the Study of Time*[5] and that it planned a conference in Japan during the summer. I wrote to the organizers, explaining that I would like to attend and was invited to give a presentation. However, it was left to me how to get there. Not having much money, my friend and I took the Siberian railway to Vladivostok — via Moscow and Irkutsk. In Vladivostok we took an East German cargo ship that went to Osaka. I remember doing the last corrections of my lecture on the borrowed old typewriter of the captain — these were the days long before laptop or tablet. We were the only passengers. Altogether the travel with the train plus the cargo took about ten days, including a stop in Irkutsk, the capital of the "wild East" of the former Soviet Union. I did not like Moscow, while Irkutsk, at least in the summer, was beautiful, bathed in the light of the white nights. At the conference below Mount Fuji my talk was very well received. I spoke about the social dimensions of time measurement and how they shaped and were shaping social structures (Nowotny, 1975). The ISST turned out to be the most interdisciplinary group that one can imagine: there were sinologists studying time in China under the Ming dynasty; a number of serious theoretical physicists; musicologists and practicing musicians — time and music are intertwined —; historians of different epochs and continents; psychologists and literary scholars. The only ones conspicuously absent were the economists, for whom such interdisciplinary exchanges were obviously a waste of time.

EE: Was the discourse about time in such a group productive?

HN: Definitely, yes. I enjoyed talking to people from many disciplines and to see that the topic of time can be approached from so many different perspectives. So I kept in touch and later became one of the Presidents of the International Society for the Study of Time, involved in organizing the conferences held every couple of years. The founder of ISST, J.T. Fraser, was a charismatic personality. After his death, the nature of ISST changed, as it often happens. But the topic of time, once you begin to engage with it, never leaves you. It becomes part of you. The book I wrote on *Eigenzeit* is about social time, which includes chronopolitics, gendered time and how technologies impact the experience of time in post-modern societies (Nowotny, 1989). But there is a personal, hidden subtext: what time means to me, how I define and use my *Eigenzeit*, its creative potential and relevance. Even the setting of writing was very time-structured: I had only five weeks as a scholar in residence in Bellagio[6] to finish it. It was gratifying but hard, like running a marathon.

EE: Did you have a deadline, or did you set yourself a deadline?

HN: I was still director of the institute, so I knew: either now or never. If I could not finish in five weeks, it would be delayed for a long time to come. The book was a big success in the German-speaking world and has been translated into several languages. I still meet people who have read it and tell me what it meant for them. Last year I was invited at the *Wiener Fest-*

5. Established by J.T. Fraser in 1966, the *International Society for the Study of Time (ISST)* (https://studyoftime.org/) is an interdisciplinary organization of professional scientists, humanists, and artists, exploring the multiple dimensions and perspectives of time across the disciplines.

6. At the Rockefeller Foundation Bellagio Center: https://www.rockefellerfoundation.org/bellagio-center/

wochen[7] by the vice director of the *Burgtheater* for a session where people choose a discussion partner. She had read *Eigenzeit* and said it was still relevant for her. It made me curious: I went back to what I wrote more than three decades ago and must say — it stood up well.

EE: Of course, definitely. In hindsight you see that some topics became more and more relevant with the course of time. For example, the notion of the extended present.

HN: Yes, this is an important part of *Eigenzeit*. Now, with digitalization came also digital time. We live in a kind of digital time machine where the future has moved into the present. You can call it extended present or the future moving into the present, but it's exactly the same.

EE: That's how I understand the connection with the idea that the disappearance of the future is completely compatible with the new relevance of the future connected with new technologies.

HN: Absolutely. For me it is a kind of evolution, something that I anticipated without knowing how it would turn out. Nobody could foresee which role technologies would play in the future, and now with digital time, as you know from my book (Nowotny, 2021), a new strand of time has been added to physical, biological and social time. Technologies were always relevant to measure time, but especially during the period of industrialization clock time became such a dominant way of structuring the life of everyone. Many people don't think of it, because we are so Eurocentric, but countries around the globe had to adapt to the Western notion of clock time. They did not have clocks or, if they used them, clocks did not have the same function as they did in the West — structuring work, structuring life, structuring education and pension systems, thus penetrating every cranny of life in the industrial peril of modernity.

EE: This is impressive, but now of course the new technologies are different, the algorithms are different. You have this idea of the paradox of the future, connected with control and agency.

HN: Predictive algorithm are an integral part of extending the present, letting us glimpse a part of the — imagined? — future, something you are also interested in. No doubt, digital technologies and algorithms are powerful instruments to let us see further into the future (Nowotny, 2017). But the predictions they make are based on data from the past as the future has no data as yet. What I call the prediction paradox is that we leverage AI to increase our control over the future and uncertainty, while at the same time, the performativity of AI, the power it has to make us act in the ways it predicts, reduces our agency over the future. We transfer part of our agency to this technology. I am also concerned about the risks that come with this. Partly, they are the self-fulfilling prophecies, an old sociological concept, that now can be scaled up and hold many people and parts of society in their grip. But we also risk to fall back into a deterministic worldview which prevailed for the largest part of human history. For thousands of years, humans believed that gods or God or the higher powers, have decided already their fate, setting their destiny. In my view, it was one of the greatest social inventions of humanity to see the horizon of the future as open and, at least to some extent, humans are capable to shape it. This goes back to the work of Reinhart Koselleck (1979) but also to Norbert Elias (1939): given his own life experience, he knew that there is no inevitability of humanity progressing towards a higher — and better — state. Falling back into what he called barbary is possible any time and I fear that right now we see signs of a dangerous regression in many

7. *Wiener Festwochen*: https://www.festwochen.at/en/home

domains.

EE: Sure. The open future does not necessarily mean progress, we know this very well. I find your reference to determinism really fascinating, because as you know I've been working on the idea that these algorithms have procedures that are really close to the ones of divination, and divination has been the technology for dealing with future for thousands of years (Esposito, 2022). Of course we cannot give up our open future, but there's a strange clash between these attitudes. Your paradox is a great way to express that we are moving in two direction that are not compatible, and we need both of them. The complexity of the open future is connected with this.

HN: This is what I very much like about your work. But we also tend to forget probabilities. Evolution has not equipped us mentally and cognitively to deal well with probabilities. You know the work of Gigerenzer et al. (1989), Daston (1988) and Porter (1986). Doctors often don't know how to tell their patients what probability actually means, and the patient leaves the doctor's office shattered and depressed. We know that — but the world is full with probabilities. This is why we have to learn to live with them. The future is inherently uncertain and instead of being afraid of uncertainty, we should embrace it (Nowotny, 2016).

EE: It is probably also connected with the open future: probability means that we don't know. It measures what we don't know, not what we know.

HN: Probabilities never can tell you anything about one individual.

EE: While the algorithms seem to do that.

HN: Yes, they *seem* to do that. We tend to forget that large parts of social behaviour *are* predictable. We are creatures of habit and without repetition and routines, without social norms that regulate social regularities, living together would be very cumbersome. But this can numb us into believing that "normality", the belief that life will continue just as before, is to be taken for granted. The pandemic and its aftermath, and now the war in the Ukraine with its prolonged and unpredictable negative fallout, have shattered such assumptions.

EE: They actually do it, but what they say is not necessarily right. Algorithms work with individual data, but what they predict is not necessarily the right prediction. It is a really interesting debate, but there is at least one other completely different topic that I would like to address. I was always fascinated by the fact that you as a social scientist became so active and influential inside policy, as a founder and president of ERC, and in many other roles. What is remarkable is not only that they are such important positions, but also that a sociologist of science is holding them. As a sociologist, how do you see the connection between research and science policy? How do changes in the organization of science affect how people do science?

HN: Again, I did not plan it. I was always interested in speaking with scientists and learning more about what they do. Coming from a background in STS I found it easy. When I was at the ETH Zurich[8], I often got my colleagues from the natural sciences to speak about their problems, and usually it went very well. They trusted me and thought that I'm competent enough to understand what they're talking about, so I learned a lot about the problems of working scientists — by listening, analyzing and elucidating. After all, science policy is about the organization and funding of science, but in order to do so you have to know what researchers are

8. *Eidgenössische Technische Hochschule* (Swiss Federal Institute of Technology).

actually doing, and what their problems are. They have a passion for science and are guided by epistemic norms, even if they do not know it. Most are truly fascinated by a research problem, but you have to know whether the goals they set themselves are feasible and how it fits into the specific scientific problem space in which they work in order to be able to make a real contribution to the production of new knowledge. When I was in Berlin at the *Wissenschaftskolleg*, a foundation was housed in the basement (the name escapes me now) that organized small meetings of selected scientists from different disciplines. In attending some of these meetings I was struck by differences in the dress code: for instance, biologists dressed differently from geologists. More importantly, I was impressed by a recurrent phrase that cropped up in the discussion: "I would like to know" which was followed by a concise definition of what the speaker identified as the problem to be better known. In other parts of the discussion participants asserted: "we will know this in five years". So I asked them: "how do you know that you will know this in five years?" The explanation was: "this is where we are now, we have these new instruments and know they can deliver. Therefore, we can foresee where we will be five years from now, approximately". I mention this because it taught me a lot about the value of instruments in the natural sciences while in the social sciences we mainly believe that scientific advancement is driven by new concepts. I continued to learn much by talking to practitioners, but I also realized how important it is to better understand how the system of science functions and the role played by funding and how to obtain it. In science, there exists a peculiar balance - or an inherent tension - between competition and cooperation. Scientists know that they need each other and they cooperate at different levels and in different forms. At the same time, they compete with each other, foremost in reputation, which is partly linked to success in funding. So, I became interested in the role of funding agencies. It all came together when I became active at the EU level. My first experience was as member of an EU Social-Economic Science Committee that evaluated and decided on grants. This was at an early stage in the Framework Programmes and I quickly noticed how successful our British colleagues were in obtaining funding. Why? Of course, you might think, their proposals were simply better, writing grants in their native language helped and so on. But I soon discovered that something else was involved. The British members of our Committee went home after each meeting and shared with their colleagues our mode of working. In other words, one of the advantages British applicants had over those from other countries was that they knew which criteria, emphasis on methods etc. mattered in the evaluation. In other countries, however, if you happened to be a member of such a committee, the tendency was to keep this kind of knowledge to yourself. As social scientists, we were far from "Open Science" and from sharing valuable information, working more like the alchemists in the Middle Ages. A bit later I was approached by a search committee of the European Science Foundation who asked me to become the Chair of the ESF Social Science Committee. Over a lovely meal in Paris I agreed. The only problem was that I had never met the Committee before and they only learned that I was their new Chair shortly before we met in Uppsala. As you can imagine the meeting started somewhat awkwardly. Looking back, I think this is when I learned on the spot how to chair a meeting, a kind of acid test, which I passed. Ever since, I have chaired a large number of meetings of different sizes and kinds. I have learned how to listen to what people say, interpret what they actually mean but don't say, combine the diverse inputs and in the end follow my own instinct where I want to lead the group.

EE: And they should recognize it as their own will.

HN: Yes, that's part of the secret. At the next meeting of the Committee I asked my assis-

tant to remove all the ashtrays on the table — back then, people still smoked heavily during meetings. Understandably, the smokers were surprised and upset. I briefly explained that we all know that smoking is not good for others in the same room and that there will be a break to smoke outside. Since then we had no-smoking meetings and no more complaints. Sometimes you just have to lead by doing. Through the European Science Foundation I got to know the European scientific establishment and the EU bureaucracy in Bruxelles dealing with research, technology and innovation. I took part in numerous "high level expert groups" and the conference circles in which RTI policy was discussed. At one point, a new advisory body to the Commission was set up, consisting of 45 persons, half of them from academia, the other half coming from industry. I was invited to become the chair, but had two conditions. First, I wanted to be able to select my own Committee Secretary. From experience I knew that if the Commission would appoint such a person for me, I would have had to rewrite the minutes of each meeting myself, as it would have been written in "Commission speak". The other condition was addressed to the members of European Research Advisory Board. They were accustomed to meet as separate factions before, one for academia, one for industry. I told them that we needed to sort out differences not before, but during our joint meeting. If we wanted to become credible, we needed to speak with one voice and with every member behind it. I succeeded in getting a wonderful woman from Norway as Committee Secretary and EURAB members agreed that it was better to meet together if we wanted to make a difference. One of the most important and consequential recommendations was to strongly support the establishment of the European Research Council, ERC. It needed fresh money, at least 1 billion € to start. We were not the only ones. The Nordic countries has been strongly engaged in pushing for an ERC and so did many scientific lobby groups, organizing workshops and conferences. It was fascinating to be part of this collective effort that eventually led to the establishment of the ERC. Until the very end, two countries — Germany and the UK — opposed it with the argument that the Commission would not be able to set up such a funding agency guaranteeing the kind of scientific quality that prevailed in their respective national funding systems. A German law professor from a Max Planck Institute argued that on legal grounds all official languages had to be admitted, which clearly would have made the practical operation of the ERC impossible. There were also more political hurdles to be taken and at one point I sent my Scottish Vice-President to speak to Lord Sainsbury (at the time the UK Minister of science) before a crucial meeting. At the conference, Lord Sainsbury declared that he had come with a differently prepared speech but had been convinced that the ERC should be set up. This was a real occasion to celebrate!

EE: The issue of combining different national cultures is still here, but out of my experience I could observe that the way of doing research has dramatically changed in the last decades — and I think that the ERC and its fundings were part of it. It means doing research and academic activities in a deeply different way. How do you see it? You had a privileged standpoint to observe how this changed in the last decades.

HN: The ERC started with a Scientific Council of 22 members who had been chosen by a selection committee of five, chaired by Lord Patten (now Chancellor of the University of Oxford) out of a list of 450 names. The Selection Committee did a great job as we, the 22 founding members, were highly committed to establish a new European funding agency for frontier research (a code for fundamental research) on the basis of scientific excellence only. The ERC was to be "run by scientists, for scientists". We quickly agreed on the principles in the very first meeting, including that two-thirds of the funding should go to younger researchers. There was

also immediate consensus that the social sciences and humanities would be part of the ERC and that the funding available should be allocated to the three domains according to demand. As Vice President I argued for 18% of the budget for the SSH which, as comparative figures from other international funding agencies were too few, was reduced to 15%. Now, the share is back to my original proposal. The European Commission deserves praise for the courage to take the radical step to leave the strategy for running the ERC programme entirely to the ERC Scientific Council. Most Commission officials expected the 22 members of the Scientific Council — all highly respected and busy people in their respective research fields — to become tired and gradually pull out. Indeed, the workload was huge. We had to set up 25 panels covering all fields of science and scholarship and appoint the best scientists we could think of as panel members. We had the privilege of building a quite unique funding agency at world-class level and were determined to live up to it. So, contrary to what some bureaucrats expected or hoped for, we did not become tired. We were a pioneering and cohesive cohort, determined to make the ERC the success story that it became. Problems arose from the fact that the implementation of the scientific strategy decided by the ERC Scientific Council was left in the hands of the Commission, first a small group, later the ERC Executive Agency. The implementation of our vision and our strategic decisions therefore had to meet many Commission rules not designed for such a purpose. Many obstacles had to be overcome, but in the end the quest for scientific excellence prevailed.

EE: In hindsight we can say now that what came out corresponded to the initial planning, with some adjustments. It was a big success.

HN: Yes, and it has been recognized.

EE: Did you expect you could do what you planned? And did you also expect it would be so successful?

HN: We worked in thus incredible pioneering spirit and we were a group of wonderfully dedicated people. Some of my colleagues eventually lost patience with the bureaucratic structures of the Commission. For the EU bureaucracy we, acting as independent scientists in the name of science, were a strange animal, almost an accident or perceived as a threat. My advantage coming from STS was to see the structures behind the hindrances we faced, while some of my colleagues from the natural sciences took much of it personally. Fotis Kafatos, the first ERC President, asked me to chair the Scientific Council meetings. Following Fotis' resignation in March 2010 my colleagues elected me as President in which function I served until the end of the Framework Programme in December 2014.

EE: That's certainly a lot of personal talent, but one also hears the sociologist speaking.

HN: Well, it helped me — because you don't take it personally.

EE: Yes, you have a sense of the institution.

HN: That's right, and it helps to see the people as they are. Of course, every person is different, but you also see where they come from. From this point of view, it helps to know that there is a background you can interpret and appreciate. Working in a European context also means it is good to know how different funding agencies function. Science is international, but scientific careers are still largely shaped by national science systems and the national contexts in which researchers work. Moreover, there are different scientific and disciplinary "sub-cultures" which may also somewhat differ from one country to another.

EE: A last question. You wrote very early about gender (Hausen & Nowotny, 1986), and now the issue is so relevant. How do you see the topic today? Is science still male, and in the same way? What role has gender played in your research and in your career?

HN: A lot has changed, and much for the better, I must say. But we are not there as yet. As you know, also in science unconscious bias is pervasive. At the ERC we set up a working group to look into how this could be mitigated. Did discrimination occur? Looking carefully at the figures available we wanted to know especially what happened in the last phase of the evaluation and whether women applicants were treated the same as men. It turned out that in physics, as expected, there were very few women. The women who made it to the end were usually very good, and there was no problem. In the social sciences almost as many women as men reached the final stage and, by and large, it also worked. The only domain where women seemed to fall off were the life sciences. The figures were roughly the same over years and the same pattern persists to this day. Why? My hunch — which I could not corroborate as the data were not sufficient — was that in the life sciences hidden hierarchies exist. For the ERC it is very important that the candidate comes with an original project proposal and above all, has an independent scientific mind. Apparently, in the life sciences and especially in medicine, work is organized in a more hierarchical way, with the director of the clinic or the big lab assigning who works on which problem. This means that young researchers have fewer opportunities to come up with their own ideas and that women are less able to resist. This is noticed in the interview by the panel probing the scientific independence and my hypothesis why more women failed. We also looked for unconscious bias in the deliberation processes of the panels. We asked the staff of the ERCEA who were present in the meetings to write down if a remark, however casual, strikes them as possibly pointing to unconscious bias. They showed us the results and we asked them to report back to the panel without revealing the who had said what. Most panel members were taken aback. They realized that also they were prone to unconscious bias. Being unconscious, it is difficult to say how long the effect of such a feed-back lasted, but it was a start. The book *How Male Is Science* (Hausen & Nowotny, 1986) was based on the talk I gave as a Fellow at the *Wissenschaftskolleg* in 1982. It was a reaction against the experience I had at WIKO during my stay. Last year, on the occasion of the 40th anniversary of WIKO, Barbara Stollberg-Rilinger, the current Rector, asked me to give the annual Welcome Address in which I also spoke about how it felt then to be a woman among the many *Germanisten*[9] who were the Fellows during the first year. We were only two women and my colleague, a literary scholar from Israel, announced that she was pregnant. I was deeply shocked to hear these scholars discuss whether a pregnant woman can be a fellow and decided to change the topic of my Fellow lecture. It was a memorable event. The lecture was public and most men in the audience kept silence, either because they were clever or indifferent. Those who spoke ran into fierce opposition from the feminists who were present. My WIKO co-fellows, however, thought that the topic was neither scientific nor of any concern to them. Later, Karin Hausen approached me to organize a conference on the topic which was turned into a book. A last little anecdote about this conference. We had invited women from different scientific disciplines, but also four or five men whom we asked to comment the papers given by women. They all were sympathetic to feminism. However, during the week two of them approached me separately and told me that only now they realized what it meant to be in a minority as a man in a women's conference, and how women must constantly feel when they are in a conference dominated by men. Intellectually they could analyze it, but emotionally they felt unable to cope and told

9. German Studies scholars.

me that they would leave. Maybe this is a reminder how much of "society", cultural norms, socialization and feelings are involved when doing science which is a wonderful, but and above all, a *social* endeavour.

EE: Thanks so much for your time. I have a lot of really interesting materials.

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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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This interview has been translated from French by Martha Poon.

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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.

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Fixing the Climate: Charles Sabel in Conversation with Filippo Barbera

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Abstract

In this interview with Filippo Barbera, Charles F. Sabel discusses his latest book, *Fixing the Climate* (Princeton University Press, 2022, with D.G. Victor), that dramatically reorients our thinking about the climate crisis. It provides a road map to institutional design oriented around concrete problem-solving that can finally lead to self-sustaining reductions in emissions that years of global diplomacy have failed to deliver. The discussion touches upon a number of key issues of general interest for social scientists: global governance; decisions under uncertainty and risk; pragmatic solutions to wicked problems; technological solutions and innovation.

Keywords: climate change; experimentalist governance; collective learning; policy failure; Montreal Protocol.

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Filippo Barbera: Thanks for this interview. Let me start with a general point. Climate change is the most pressing and crucial challenge the humankind is currently dealing with. Dystopian narratives, "escapism," technological "solutionism," command-and-control measures and business-as-usual market -based scenarios are the mainstream proposals. You are arguing that none of them is working and that actually the best solution is different and hidden in plain sight. Is this correct? Would you please tell us more about the general message of the book?

Charles F. Sabel: The general claim of the book is that despite 30 years of trying, global solutions based on uniform rules, like a carbon tax or globally agreed targets for emissions reductions, have not worked. What is working on the other hand are efforts towards achieving a green transition, in particular sectors like electric vehicles, photovoltaics or the elimination of ozone-destroying substances, and relatedly efforts at greening a particular sector in a particular place, such as cleaning up agricultural runoff in Irish dairy. In other words, attempts to create an (apparently) simple global system of incentives to induce a green transition have failed, whereas concrete efforts to address key obstacles to the transition are succeeding; more specifically, approaching climate change as the result of a negative externality, to be eliminated by making polluters pay a Pigouvian tax reflecting the social harm they cause, led to a dead end; approaching climate change as a challenge to switch from dirty technologies to clean sector by sector, re-conceiving the products of our civilization and how we make and use them is proving feasible, even in domains like steel production, where dirty products were once so deeply embedded in everyday life that no alternative seemed possible.

We say that the promising solutions are hidden in plain sight because, while the 1987 Montreal Protocol on Substances that Deplete the Ozone Layer—arguably the most widely admired and successful international environmental agreement of all time—is there for all to see, and does on inspection embody the principles of sectoral problem solving we advocate, its success was mistakenly attributed to the maturity of the science of the stratosphere and the apparent precision and rigidity of the regime's legally binding pollution reduction targets, rather than the mechanisms by which it searched out and evaluated possible solutions. One task of the book therefore is to provide a compelling explanation of how in fact Montreal did succeed; how the principles of decentralized but coordinated, or experimentalist, problem solving adopted there also emerged in other iconic cases of environmental success, such as the California regulations supporting the development of much cleaner (eventually electric) vehicles; and, more speculatively, how this kind of problem solving can become the basis of a new kind of globalization that does not impose the uniformity demanded by the current one.

Though this framing remains valid, it is important to note a dramatic, if still inconclusive shift in mainstream thinking, with the passage of the (misnamed) Inflation Reduction Act and related legislation in the US and matching measures by the EU. This legislation commits vast funds to the support of accelerated green transitions in sectors such as e-vehicles and the associated supply chains for batteries and critical minerals, as well as wind and solar power generation, carbon capture and storage technologies, and green steel and hydrogen. Publicly the shift is often explained as a concession to the quirks of politics: the public prefers carrots—rewards as incentives—to penalties or sticks, especially taxes on or prohibitions of products or practices. A complementary explanation is that tax experts and economists now acknowledge that constructing efficient and fair markets in carbon tax credits or emissions permissions is much more difficult in practice than theory made it seem, with the result that defense of the mainstream position became more reluctant as criticism of it became bolder. But whatever the reasons behind

the redirection of policy the massive tax incentives contained in the new legislation are catalyzing a wave of substantial investment in the target sectors, demonstrating (perhaps as much to the surprise of the programs' boosters as to bystanders) the existence of a potent green lobby, willing to bet it will do well in the transition, and likely to be an important force in the coming rounds of climate politics. Of course business support for the new legislation is no proof it's all a good thing. Firms can be using the subsidies to commercialize inferior or dated technologies, or for other forms of rent seeking. But compared to the world that you are accurately describing in your question, there's been a big change and it's a big change in the spirit of the book. In fact, it is a very hopeful book—because our argument is that this "theory of change" is sitting in plain sight, is being used in many settings, and it will change the technology and politics in ways that make deep cuts in emissions possible.

FB: That's very interesting. Let me add a minor point to your answer. You mentioned the role of the private sector. Do you think that in this "Green Transition renewal" a new place for the role of the State is at stake? I mean the so-called "mission-oriented state"? Just to understand what I have in mind. Do you think that this "mission-oriented state" also plays the role along with the private sector?

CFS: There's no doubt that the role of the State is more explicitly up for redefinition than it has been in many decades. At a minimum the State will be deploying familiar or new tools of industrial policy to shape market outcomes to a degree that only recently would have been unimaginable. But beyond such generalities the picture is murky. When you look in detail at how the money is being spent—what kinds of projects are eligible; how progress will be reviewed—there is a great deal of indecision about what exactly the role of the State should be. Should the emphasis be on speeding commercialization of market-ready (green) products by offering rebates to consumers and subsidies to investors? (That's what most of the money in the IRA is doing—channeled through the IRS.) What about focusing on the place-based coordination of investments in new green products and jobs with the development of the work force this will require, and the creation of specialized research and standard setting facilities that will help keep the emerging cluster or hub abreast of new developments? What about programs encouraging research at the frontiers of technology, for instance in the development of low carbon concrete or aluminum, or in place-based innovations addressing the problems of agricultural communities or communities victimized by environmental injustice? Some of all of these will be needed of course. But in the United States, and in the EU (to judge by early reports on the implementation of the National Recovery and Resilience Plans) the choice of program seems to be determined as much or more by the vagaries of the legislative process and by worries, widely shared among lawmakers and administrators of quite different political persuasions, that the State as currently configured lacks the capacity to mount and manage programs that go much beyond certifying eligibility for tax credits or similar carrots and conformity to the agreed conditions—that is, writing checks to beneficiaries that effect a transfer of resources without requiring the State actually to deliver services.

So, no question that taboos have fallen, a bit. The role of the State is in play, and some government entities are acting, or preparing to act with the purposefulness and continuous engagement characteristic of mission-oriented projects. But we are still very far from a world in which States can be presumed to be mission-oriented, and have the capacities to deliver on their commitments—especially where those commitments involve disruption of the industrial status quo. We have been freed of some of the contrasts of orthodoxy, but still strain to find a way forward. We are condemned to make the best of the open-ended situation we face, learning

rapidly, we hope, from the successes and failures of the green transition how a re-empowered state can make effective and accountable use of the expanded authority it is being accorded.

FB: Thank you, great, thanks a lot. I think that we can move to the second question. When comparing Montreal and Kyoto, do you maintain that, despite the differences, the two cases are, by and large, comparable? Would you please explain why?

CFS: Let's start with the reasons often put forward to distinguish them. The first is that to do with the supposed difference in the maturity of the relevant climate science at the time of the political controversies. The argument is that understanding of the vulnerabilities of the ozone layer was well consolidated in the run-up to Montreal, leaving no doubt that human activity was endangering the ozone layer and no reason to delay remedial action. The science of climate change was in contrast still immature, allowing skeptics to turn professional disagreements into doubts about the validity of the whole research endeavor, thus reducing the pressure for action. A second argument concerns the availability of alternatives to current products and processes. Safe substitutes for ozone-depleting substances were said to be well known or within easy range of major producers, held to have been reluctant to pursue these opportunities for fear of disrupting current business. Once the incumbents saw the ozone layer needed protection, they provided it. With respect to climate change, the argument continues, the situation was again the reverse: workable alternatives to dirty technology were located on a horizon so distant that they were much more readily imagined than observed.

This account is right in its characterization of the background of the climate change debate, but wrong about the background of Montreal. The science of the ozone layer was far from mature in the late 1980s, when the Protocol was agreed and went into effect. Some of the key, ozone-destroying reactions in the Antarctic stratosphere—especially those involving direct exposure to the summer sun—were well understood because they had first been identified in the 1970s; others, occurring at the edges of frigid, stratospheric clouds in dead winter and in the early Antarctic spring, were not. Because of these gaps in understanding estimates of the contribution of human activity to thinning of the ozone layer, and of the threat this degradation posed to terrestrial life, varied. Nor does the supposed technological optimism of participants in the Montreal discussions bear much scrutiny. Expert estimates at the time were that with best efforts working substitutes could be found for 50% of the products then endangering the ozone layer—roughly the same kind of inconclusive, mumbling out loud that would later reappear in early discussions of the green transition. (That same mumbling still happens today, with much of the debate about climate policy focused on sectors often called "hard to abate"—that is, places where the technological solutions are unknown today.) As we show in the book, in the case of Montreal, it was not the confident expectation of quick and painless solutions that led to a successful problem-solving regime, but rather the formation of a sector-based, experimentalist regime involving actors with hands-on knowledge of problems, that made the problems manageable. None of this is to deny that decarbonizing the economy is a vastly more complex undertaking than protecting the ozone layer. But that difference notwithstanding the thinning of the ozone layer and climate change pose similar challenges of collaborative exploration under uncertainty, and it seems time to learn the true lessons of the Montreal success.

FB: It sounds like the "logic of inquiry," a pragmatic perspective on search as in John Dewey's perspective.

CFS: Yes, this is a very clean example of John Dewey's understanding that problem solving originates in the discovery by a heterogenous group of people of a common obstacle that

frustrates their understanding and thwarts achievement of their goals. In Dewey's exceedingly spare account, recognition of the shared problem leads to the formation of a public committed to solving it via experimentalist methods. (Note that, as Dewey would have expected, the formation of a public and the open-ended problem solving which it encourages also created scope in Montreal, and many other cases discussed in the book, for collaboration between laypersons, with expert knowledge of the effects of current arrangements and possible alternatives on their lives, and technical experts in products and production processes, thus creating some initial possibilities for deliberative, democratic engagement in a transition that may at times seems entirely dominated by technical reason, or at least the pretense of it.)

FB: You single out three dimensions of experimentalist governance: organizational structure, form of deliberation and set of incentives. As for the third, you point to the key role played by penalty defaults. Would you please explain what they are and why they are so important?

CFS: Let me go back one step and say that an important undertaking of the book is to think of how to design incentives to induce self-interested actors to pursue public-regarding ends under uncertainty. This reconsideration is important because the normal incentive structures, formed under stable conditions and tested by time, assume a world in which actors compare the costs of complying with the law and the benefits of violating it. If the cost of a fine for violation, discounted by the probability of detection, exceeds the gains from violation, the actor complies. But such calculations are no guide to action in sectoral green transitions. In these cases the purpose of regulation—and other forms of industrial policy—is not the maintenance but rather the transformation of an existing system by setting targets that yield sustainability gains in the short and medium term and accelerate transition to a low- or zero-carbon set up thereafter. The regulator's problem is that she does not know what targets are feasible (or will soon be) and demanding enough to stimulate continuing inquiry and progress, nor will even the most capable firms in the industry have confident answers to such demanding questions; and in any case many of the capable firms will prefer inaction to the risks of pioneering innovation.

A general, working solution to this problem, documented in the book in the California Air Resources Board's program to reduce vehicular emissions, is to credibly threaten imposition of demanding (but arguably feasible) regulatory requirements, phased in over a period of years, and to accompany the threat by an offer to regularly consider with key stakeholders whether to tighten or relax the standards in view of the accumulating evidence of what's actually feasible. Absent the prospect of discussion convened by the regulator the prudent strategy for the capable actor is inaction—stonewalling requests for information. So long as the capable firm's peers don't break ranks, the regulator is kept in the dark and no firm has to place risky and revealing bets on its favored technologies. But once discussion with the regulator is in view the capable actors face a prisoner's dilemma, and each hastens to confer with the regulator, and eventually its peers, in the hopes that its preferences will influence the standards under construction before opposing ideas catch on, the conspiracy of silence gives way to a competition in collaboration.

But, to return to you question: The possibility of this virtuous circle depends on the existence of a credible threat to impose an outcome on the parties manifestly less acceptable than one they could have arrived at in collaboration with the regulator and each other—what we call penalty defaults. A private law analogy would be a family-court judge's offer to a divorcing couple of a choice between a court-concocted separation of assets, based in law but so far from the reality of the two households as to be deeply disruptive to each (the penalty default)

or instead, to avoid this outcome, the sharing by the parties of the information needed for a settlement workable for all. The aim, as in the assessment of new regulatory standards, is to make it risky for the parties to cling to the status quo, and to obligate them to consider cooperative investigation of novel possibilities. The book details the surprisingly many ways de facto penalty defaults are established by law, civil society campaigns, or private actors responding both to public pressure and the prospect or regulation or legislation.

FB: Very interesting. This presupposes a neutral State, without particularistic interests towards some sectors or some producers, doesn't it?

CFS: I wouldn't say a neutral State but rather a State which is committed to advance in a certain direction, typically at the margin or outside the umbrella of orthodoxy that legitimates routine decision making. In the case of *Fixing the Climate* (Sabel & Victor, 2022) the goal is the green transition. The State is (or should be) neutral with respect to the precise technologies that will vector the transition—will green hydrogen be a battery or a fuel? Will it usually be used near where it is produced or will it be shipped afar?—and even surprisingly neutral, at least so far, about the kinds of industrial policies that will be used to support the transition. But, as the massive response to the IRA subsidies shows, the very general direction of development is no longer up for grabs, even if doubts very rightly persist that the transition is proceeding rapidly enough.

FB: This reminds me a little bit of the old idea that Peter Evans illustrated with the concept of "embedded autonomy." The State knows where the frontier of the innovation is and so it's very close to societal challenges and collective needs, but at the same time it's not captured by vested interests in that sense.

CFS: Yes, embedded autonomy in that sense is the goal. But I would say that Peter Evans found embedded autonomy in his Brazilian cases. He didn't explain how it could be generated where it didn't already exist. The discussion of the incentive structure of experimentalism in the face of uncertainty aims to show how embedded autonomy can be the outcome of a deliberative process, with a penalty default in the background, rather than a precondition—an endowment, available or not—of problem solving.

FB: Let me just go back to penalty defaults very quickly. Wouldn't they be working only if the so-called exit costs were quite high? I mean, if there were no alternatives to keep the old technology in other markets, in other countries or whatever. Isn't this very demanding? Isn't this a very demanding condition?

CFS: Well, I guess the answer is both yes and no. In some cases there are alternative markets and the costs of exiting or ignoring transition agreements are low. For example, if you cannot sell palm oil in the markets of the EU and North America, you can sell to the basically unregulated market of rural China or rural India. But it is not at all clear this is a stable solution. These markets are not fast-growing; there are clear limits to their expansion, and they may be shrinking. But above all they are poor markets. They have low margins, they work with "recycled" technology, often with equipment banned from use in advanced markets; and for this and many other regulatory reasons their products can be sold only to similar countries. There is, in short, an incipient global informal economy, where low-quality, environmentally dirty goods are produced by cast-off machines using dirty processes, and then sold to low-wage workers also in the informal economy. Will the existence of this low-equilibrium, global secondary market (which is beginning to take root in economically vulnerable areas in the advanced countries

too) be the bane of a green transition (as well as a social, political and economic tragedy in the making in itself)?

I see two important reasons to think not. First, any country that wants to grow rapidly and acquire new capabilities will not want to be condemned to serving stagnant informal or secondary sector markets. On the contrary, such ambitious countries will want to use technologies and meet the standards that allow its exports entry into the advanced markets. China is a leading example. At the time of the Montreal Protocol, the Chinese were divided about whether to adopt the new standards or not. Recent investments in factories to make the substances which were to be banned counted against joining. The decisive argument in favor was that China could not export refrigerators to advanced countries unless it adhered to the protocol.

The second consideration counts against the idea that the advanced countries can afford to abandon the global informal economy to its fate. For the first time in a long while, maybe the first time ever, the advanced countries truly need the developing countries to participate in meeting a common challenge and will continue to need them as far as the eye can see. If the United States, the EU and Japan clean up all their emissions and nobody else does, the planet will still warm—a lot. So we are bound together by a unique objective constraint, and the evidence is accumulating that we are, however slowly, realizing the gravity of the situation and demonstrating to ourselves that we have the capacity to address it. At any event what we are seeing today is not the usual relation between the center and the periphery, where the former needs low-cost inputs and the latter accepts that because it has nothing else to offer or withhold.

FB: In your book, there is little space for finance and financial capital. We know that, to protect capital investment and return on capital, global players have been cheating and hiding information on climate change for years. The case of "Exxon: The road not taken" is the most compelling one. Why should they be supporting the experimentalist governance solution? Let me put it straight: I see little place for evil and domination strategies in your proposal. How does experimentalist governance deal with evil and domination?

CFS: Let me pass on the question of the relation between experimentalism and evil, except to remark that in discussions of social and political organization, the treatment of evil has become the department of political theory, with other commentators assuming actors stripped of virtues like courage or altruism and vices like the lust for power or delight in deviltry, and therefore prisoners of their self-interest or ideological heritage.

But of course, domination can, and often does emerge from the operation of such faceless motives. The discussion of penalty defaults after all, presumes a world in which the champions of the dirty economy still command the economic and political resources to defend their positions against green challengers. But theirs is a wobbly hegemony. Their answer to the menace of climate change is to temporize, even as the magnitude of the threat becomes more and more palpable; the tide of technological development is turning against them, as it becomes clear that there are feasible alternatives to dirty technologies. Experimentalism exploits the chinks in their hegemony—the disorientation of orthodoxy, and the growing confidence that we can indeed build a low-carbon future—by indicating, as we just saw, how regulatory strategies can make it in the self-interest of capable firms to join the search for green solutions, and how these solutions can in turn help crystallize alliances that push for further change in the same direction. In this context, experimentalism is a strategy for the incremental, but cumulatively comprehensive transition from a dirty to a clean economy, and the changes in governance this will entail.

Even Exxon can't hide from these incentives. In the last two years we have seen three mem-

bers of their Board replaced—for lack of seriousness about climate change. Exxon is now a backer of green transition technologies—at least those that align with their skills, such as carbon capture and storage and also hydrogen. Penalty defaults can influence even the mightiest and create incentives, even for them, to break ranks with the status quo.

FB: Let me close with a question trying to bridge *Fixing the Climate* (Sabel & Victor, 2022) with the proposal of Bruno Latour. In *Down to Earth: Politics in the New Climatic Regime*, Latour (2018) says that it is urgent to shift sideways and to define politics as what leads toward the Earth and not toward the global or the national. Belonging to a territory is the phenomenon most in need of rethinking and careful redescription; learning new ways to inhabit the Earth is our biggest challenge. Bringing us down to earth is the task of politics today. Is there any connection between experimentalist governance and a new way to thinking about our belonging to the "terrestrial"? *Fixing the Climate* would otherwise appear to be detached from the daily life of ordinary people, it deals with regulation, firms, élites, experts.

CFS: Yes, I think there's a very direct connection between what Latour calls "the terrestrial" and the link that experimentalism establishes to place. Latour conceives of the local as an aspect of nostalgia, a retreat to familiar attachments, because they can be presented as unquestionable, and their unquestionability can be marshaled to support conservative rejection of the idea of any change. The global, as Latour has it, is synonymous with modernity's abstract idea of progress, in its worst, most deracinated form. The terrestrial then becomes a residual category, a place or possibility for deepening our engagement with immediate surroundings without being paralyzed by nostalgia or bewitched by lifeless, astral abstractions. You said our book is concerned with things far-away from the daily life of ordinary people. That's largely true of the chapters that deal with innovation at the technical frontier—the development of e-vehicles, or new types of energy storage devices for power networks. In these cases innovations will work largely as expected without regard to the particularities of place. But another central theme of the book is "innovation in context." In these cases, in contrast, general innovation has to be contextualized to the idiosyncrasies of place if it is to work effectively; this contextualization can lead to reinvention of the innovations, and it typically draws local people, as experts in their home range, into the process of reconceptualization. The leading example in the book of this kind of process is a case study of changes in the control of agricultural pollution runoff in the Irish dairy industry, beginning with the discovery that standard limit values for concentrations of nitrates and phosphorus in fields are of little value (because the effect of the pollutants depends more on the accidental slope of the field, composition of the subsoil or the features of the underlying geology than on the limit values) and ending in the elaboration of a system of governance in which farmers, assisted by extension agents and soil and other kinds of experts, develop field by field plans for controlling runoff, and agree measures for treating larger sources of rural pollution. The crucial point here is that the local groups are neither reviving traditional practices nor applying or propagating incontestable, universal knowledge. Rather the local farmers, drawing on their traditions, their experience, and their understanding of agronomy, collaborate with a diverse group of experts to figure out what works on their farms, and then use this deepened understanding to revise general rules. In attaching themselves more knowingly, more deeply, to their local world the actors are escaping both the telluric undertow of brute tradition and the bewitchment of astral abstraction. A shortcoming of Latour's otherwise interesting book is that it doesn't actually say much about what people are doing when they "terrestrialize" themselves. I don't presume to know his thinking, but perhaps he might agree that experimentalism offers a way of "institutionalizing the terrestrial."

FB: Thanks again, it was a great interview. Would you tell us what is your next big project?

CFS: I'm up in the air, not least because the situation is indeed very open, and it's difficult to know which of the many promising initiatives will bear fruit, or at least reward study. One possibility would be to return to the study of the EU, which is often written off or reviled as an outgrowth of turn-of-the-century neoliberalism. In fact, the EU has done quite well in responding to a series of crises (cumulatively, the polycrisis), expanding the scope of union action, but not concentrating power in the European Commission, or the European Council. Perhaps the EU is not simply avoiding disaster by inspired improvisation, but has rather hit on a means of experimentalist learning from and amidst crisis, using parallel searches by actors with hands-on knowledge of problems to find solutions beyond the reach of centralized authorities, and incumbent firms, provoking reconsideration of political alliances and governance arrangements along the way?

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Charles F. Sabel is Professor of Law and Social Science at Columbia Law School (USA). Previously, he was Ford International Professor of Social Science at Massachusetts Institute of Technology (USA). Sabel is a leading figure in political economy and social theory. He studied the crisis of mass production and its implications (*The Second Industrial Divide: Possibilities for Prosperity*, 1984, with M.J. Piore). His more recent work focuses on democratic experimentalism (e.g., *Learning from Difference: The New Architecture of Experimentalist Governance in the EU,* 2008, with J. Zeitlin). His current projects focus on global problems such as trade and climate change.

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Choices and Historical Processes: Elisa Reis in Conversation with Luciana de Souza Leão

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Abstract

In this interview with Luciana de Souza Leão, her former student, Elisa Reis discusses her intellectual trajectory and how it shaped her research projects and teaching in sociology. Specifically, they talk about Reis' work in political sociology, sociological theory, elite's perceptions of poverty and inequality, comparative methodologies, and the current politics of knowledge production in the Global South.

Keywords: Political Sociology; Elites; Inequality; Brazil; States.

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1 Encountering Political Sociology

Luciana de Souza Leao: I wanted to start today by asking you if we could talk a little bit about your process of becoming a political sociologist. How did you get interested in this field?

Elisa Reis: Well, during high school, I was planning to be an engineer. But then I got involved in student politics and changed my mind. I learned about this undergraduate program where they taught sociology and political science at the same time and decided that was what I wanted. So, I moved to the Federal University of Minas Gerais, in Belo Horizonte, Brazil, which offered an undergraduate program on politics and sociology in the School of Economics. Unfortunately, in my very first day of class, the military coup took place. During the first days of the military dictatorship, the army was there occupying the building, and the experience of living under authoritarian rule deeply marked my cohort. The course itself was very much oriented towards social theory, with a strong French social science influence. We were reading French sociology of the time — like the two volumes of the *Traité de Sociologie*, edited by Georges Gurvitch (1957) — that you've never heard about in your generation. At the same time, we had a lot of influence of German sociologists, for example, one semester we were required to read a hundred pages of *Economy and Society* by Max Weber (1922) every week.

Along my course, I experienced the broader transition in Latin American social sciences from a French influence to North American influence. It was as if suddenly some of the younger professors came back from studying abroad, and they started teaching modernization theory and the mainstream political development literature at the time. That was what I was really reading most of my time. At that time, there was no master's program or PhD program in Brazil. It was just the undergraduate program, and we were supposed to be professionals once we finished high school or our undergraduate studies. And I was really nervous because I did not know what to do with my bachelor's degree. My father mentioned, "Oh, I could talk to a friend of mine who is in the Development Bank of Minas Gerais." But I knew I did not want that. And right at that moment I was very lucky to receive a fellowship in Chile. I accepted it immediately. I don't think I ever stopped to consider what on earth this fellowship was about. I mean, I didn't pay attention to the fact that I would be required to speak Spanish, which I had never spoken before. But I went and it was a wonderful experience.

It was a course on sociology of development, also very much oriented toward leadership skills. There were two levels. I was in the advanced level, which was a graduate course in development and there was a more basic course oriented toward forming leadership for the whole of Latin America. There were people from all over Latin America involved. It was exciting, and Chile was very attractive because I think everybody that had to go into exile from the other dictatorships in Latin America moved to Chile. Those on exile benefited from the fact that the country then enjoyed democratic stability, and also from the fact that several international organizations had offices there, so it was possible to find jobs. I spent the whole year of 1968 there, and when the fellowship finished, I had to go back to Brazil. As I told you, there was no master's and PhD program, so I was qualified to teach sociology at the university. And I got my first teaching job at the Pontificia Universidade Católica of Rio de Janeiro (PUC-Rio) to teach sociological theory.

I was there until I moved to Cambridge for the PhD later on. But I also decided to enroll myself in a master's program that was starting at a private institute, called IUPERJ. So I did my master's course while I was teaching at the PUC-Rio and it was a master's program in political science because there was no sociology at the time. I finished very quickly because I had this

whole year in Chile, and they accepted my credits from there. I wrote a thesis on the relationship between the state and the coffee growers, seeking to show that as the powerful agricultural export sector managed to secure state support to protect coffee prices, the state managed to gradually transfer export generated resources to the incipient industrial sector. Furthermore, I argued that this process also contributed to the state building process itself, to the extent that the political authority played a mediating role between agrarian and industrial elites. As soon as I finished my master's, I got accepted to MIT for the PhD.

LSL: In 1972, you started your PhD in political science at MIT. If you could tell me a little bit — what was graduate school like in the early seventies being a woman? I imagine it was also a very male dominated department. What were some of the most formative experiences you had with your mentors, or some people that might have met in the Cambridge area that might have influenced how you think about sociology?

ER: Yeah, the thing you mentioned about being male-dominant, MIT was like that. I remember that the provost was talking to the whole '72 class and he was saying, "Oh, MIT has 7% women." And political science was strongly male dominated. But at that time that did not affect me as much as the fact that I was from South America. This is something curious. I never thought of myself as South American. I was Brazilian, but in the department, I became the token South American because there were no others. And there were very few foreign students — a couple of Canadians, one or two British. I think that was most of it. There were also the first Black students. There were maybe four or five including the previous classes, '70 and '71.

But I became a research assistant, which was very good in terms of integrating me. If I were not so shy, I would have benefited even more, but I was really shy. Still, being an assistant gave me some sense of security to interact. I was in committees recruiting professors. Besides, I was very much excited with the possibility of cross-registration with Harvard. And I think I did split my courses between MIT and Harvard. At MIT, talking about influences, Hayward Alker was someone that strongly influenced me, mostly because he was someone doing mathematics for political analysis, which was a real challenge for me because I didn't have very good training in math, but I decided I should have. And he was also teaching philosophy of science. And more than that, he was a friendly person. I mean, most of the students liked to interact with him. He was a very welcoming person. I recall that in the summer of 1973 he took a group of students to Chile for a two-months course in methodology. The Social Science Research Council funded this summer course that selected 20 people from Latin America studying in different US universities and brought us to Chile. This was right before the military coup in Chile. Very exciting times. A very nice life experience to be there at that time. Unfortunately, it ended very badly, but it was a good time to be in Chile to see how things could work.

Well, there were also other people that I found very inspiring later in my time at MIT. Susan Berger became my advisor because my previous advisor moved to another university, and Suzanne was really very helpful, mainly because she was not a Latin Americanist, so she would question me about everything. Nothing was taken for granted and that was very useful. There were also younger professors coming to MIT close to her like Charles Sabel and Joshua Cohen with whom I had some interaction. And at Harvard I was taking courses with Samuel Huntington and others. Some of the courses I took just as listener, but it was very helpful. Although I disagreed with Huntington about almost everything, it was very good to be in his class. He was an excellent professor.

I think these were the people who most impressed me. But there were also lots of people in the Cambridge area and from the Boston area who were very exciting. Like, I was so much

into Noam Chomsky. He was in linguistics at MIT but very active in giving public lectures and showing movies about the Vietnam War, all that stuff. There was also another experience, very interesting, which was the joint Harvard-MIT seminar, putting together scholars interested in political development. For two or three years, I was selected as a student representative from MIT. There was one graduate student from each of the universities. The system was that once a month they would invite someone from outside to give a lecture, usually big names, and we would have dinner together. It was exciting to meet senior folks, and also young scholars or people who were about to finish the PhD. I remember that Theda Skocpol presented her dissertation to us.

LSL: It's so interesting that the workshop was called "Political Development" because this idea of political development is not central anymore.

ER: Yeah, that's gone because there was some teleology. People were expecting that at some point all nations will end up converging to the same point. It was sort of a fatalistic idea to think that history has a self-direction, so it's good it's gone. But at the time, it was really the paradigm, in fact a paradigm that we students were already contesting. I like historical sociology because it denies that assumption that development follows a natural trajectory.

2 The Brazilian State as a Case

LSL: That leads us to the next topic. You are very well-known for your scholarship conceptualizing the Brazilian state as a case. This started with your PhD dissertation about the agrarian roots of conservative modernization in Brazil. And if you could tell us a little bit about how you chose this topic and where did this interest in the state as an analytical category come from? Is it related to what you were saying about the dominant thinking at the time — that modernization is going to happen in the same way everywhere? And the counterpoint brought by historical sociologists, suggesting that maybe that's not how history works. So if you could tell us a little bit about that. And I'm particularly interested hearing how you think about Brazil as a unique experience connected to these general trends?

ER: The state was already in my mind when I did my master's course. I remember reading Nicos Poulantzas and finding it very interesting that he thought that the state could be conceived as a political actor in itself, not just as the committee of the bourgeoisie. That already interested me and that's why I went to study the Brazilian coffee growers and industrialists being mediated by the state. But the choice of my dissertation topic at MIT (Reis, 1980) was strongly affected by the fact that Brazil had so much problem establishing a liberal democratic system. I think I was not the only one. Several of us doing PhDs at that time from different angles were questioning why democracy could not take roots in Brazil?

To me the possibility of looking at a trajectory was something really interesting because I didn't like the idea of history being a series of pre-set events. I like to think of processes because then you allow space for the actors' choices, for responsibility. I have learned that people choose things, that fatalism has no place in history; at every moment, confronting real situations, people choose, and they are responsible for the choices they make. So, I was trying to dig into history — what sort of choices did Brazil make that led to this authoritarian present that we were living? It was not only an intellectual inquiry, but also an existential issue. I had colleagues who had lost their lives fighting against the dictatorship. Besides, I knew what it was to be in class fearing you cannot say what you think because you may be taken to jail.

And so, it was a mixture of theoretical concerns and existential issues that led me to think of the vicissitudes of democracy in Brazil, and I decided to look at two important historical processes. One of them was the abolition of slavery. Slavery was so central in the colonization process, and it was maintained after Brazilian independence. So, of course, slavery had to have an impact on our choices for the future. And the other process was the trajectory of state building in Brazil. I was interested in exploring how these two processes intersected. This was the enigma I put to myself. I mean, I told you I was reading historical sociology — I was fascinated by Reinhard Bendix (1964). Perhaps Bendix was the scholar that most influenced me. Although I never met him personally, I read him with great curiosity and enthusiasm. I wanted to further explore ideas that greatly impressed me reading his work, and the focus on state building was something very important in his analysis, as well as the process of citizenship expansion. So, I thought if I look at slavery and state building, I may have a clue. I may have an interesting answer for the authoritarian reality we were facing in Brazil. And just at that moment, I started going to the Barrington Moore's class. I read his book on the agrarian roots of dictatorship and democracy (1966), and I thought that his focus was a good theoretical instrument I could use. I mean, at first, I thought of using his authoritarian model of modernization because Brazil was authoritarian. But in my perception, the theoretical component of the book is much better structured in terms of the democratic model, while authoritarian modernization and peasant revolutions were subproducts of his theoretical reasoning. So, what he really offered in theoretical terms was a model for democracy and it was fascinating to see how he put together countries with such different trajectories as France, England, and the US, through different process, all end up consolidating liberal democracy.

I mean the idea of staying at such a level of abstraction that these three historical cases could be seen as following a common path was something that I liked. I still like very much the idea of exploring how to merge theory and singularity. So, I decided to compare the question of slavery. I took the US as my comparison point. I said, "Could the very different process of abolishing slavery in Brazil and in the US help to explain the diverging political orders they developed?".

Moore had shown how abolition in the US was critical because it constituted a real rupture with the past. And I my hypothesis was that Brazil never had such a rupture because the state promoted a kind of an alliance between former slave owners and the new agrarian owners interested in an expanded labor force in coffee field. These new landlords could no longer count on new slave imports because the international slave trade had been forbidden at the time by foreign pressure. So how to solve the labor supply problem if new slaves were no longer available and coffee planters were eager to push the land frontier further and further in the Central South of Brazil? At the time, the old landowners of the Northeast had already sold their slaves to the new coffee growers of the Central South, both because their sugar cane plantations were decadent, and also because being an area of older colonization, the Northeastern landowners counted with a significant semi-servile population under their control. Actually, in the eve of slave abolition the old plantation owners had already sold their slaves to Southern farmers and their representatives in Congress were voting in favor of abolition. What they did not give up though was the control over their semi-servile labor force.

So, looking at the diverse interests at play, the state provided a solution that contemplated both the old and new large landowning sectors at the time. Paying for the costs to bring foreign migrants from Europe the state granted an abundant labor supply demanded by the Southern coffee planters, while assuring Northeastern landowners the maintenance there of the existing labor force, which they kept in some sort of extra-economic coercion. In a way, I think that this process reproduced the sort of reactionary coalition that Barrington Moore refers to when

discussing the authoritarian modernization path that took place in Germany. But going a bit further, I think that the abolition of slavery in Brazil was a process that created room for a spatial segmentation of the labor market with political and economic consequences in the long-run. Instead of integrating the whole labor force in the country, there was sort of an artificial boundary whereby in the South a predominantly labor economic coercion system took roots, while in the Northeast, the labor force remained for many more decades in semi-serfdom conditions. So that explains to some extent how liberalism was contained through that sort of conservative alliance sponsored by the state.

The German experience was actually nearly contemporary to the Brazilian one as political unification there took place more or less at the same time that Brazil consolidated its independence from Portugal. In both cases, there was room for this sort of precautionary strengthening of the state that Moore talks about. The state managed to become a strategic partner to both the new and the old elites, a sort of sponsorship that created room for a state less responsible to demands from below.

3 Inequality: The View from the Top

LSL: Another important part of your work, which perhaps you are most famous for in the international sphere, is your work on elites' perception of poverty and inequality. Starting with your 2005 book with Mick Moore that studied this topic in five countries of the so-called "Global South" (Reis & Moore, 2005) to your more recent work in Brazil and South Africa, you have done in-depth comparative research about how elites understand the social world. Why study inequality through the perspective of elites?

ER: Inequality too is an issue that mobilizes me both in theoretical terms and in existential terms. I mean, it bothers me to see so much inequality around me. Brazil, as you know, is extremely unequal. That is something that really bothered and bothers me in daily life. I wanted to try to understand it from the perspective of the elites. Most of the literature in sociology at the time I started this project was looking at inequality from below. There were many studies on how the poor manages to survive, how the poor sees his place in society, what are the distinctive elements of the culture of the poor, etc. I was not happy with this sole focus because I perceived it as some sort of a patronizing way of looking at the problem, a paternalistic approach that was often mobilized to appeal to philanthropy and I was not satisfied with that. I kept insisting that interests, not just ideals, have a place in people's attitudes towards poverty and inequality. So, if those who could help reduce poverty don't do it, it's because they are not negatively affected by it; or don't see opportunities to benefit for an expanded consumption market, so they don't have an interest in changing it.

I wanted to understand how elites rationalized the fact that they had so much, while so many others had nothing. I decided to investigate how elites explained to themselves how so much inequality exists, and what could lead them to accept redistributive policies. My focus on elites derives from the fact that they have some monopoly control over symbolic and material resources. And there was something else. I think the primary motivation I had was the fact that I wanted to know what holds society together if people experience such unequal conditions, such disparate life horizons. There is a basic theoretical question underlying this query: why do societies stay together? How can such an unequal distribution hold people together? Of course, I have no answer to that, but this is some I keep questioning in my present-day research.

LSL: And why did you study elites comparatively?

ER: First, I should also mention that I always went for an institutional definition of elites. I was not interested in the elite person I interviewed as an individual. As you know, there is great interest in the personal history of elites in the literature, but that was never what mobilized me. I wanted to talk about people who occupy key positions and why they make decisions in their day-to-day in their institutional context. I started with a Brazilian survey of elites, and then I organized a seminar here in Brazil and some foreigners in that seminar were very much interested in my work, and they asked me, "Would you like to explore the subject in comparative perspective?" Of course, I was interested. At that time, I was reading the book by Abram de Swaan (1988), *In Care of the State*, and they agreed that we could invite de Swaan to join the project.

The idea progressed and I brought de Swaan to Brazil for a couple of months and we started working together with the other country researchers. The selection of the five countries involved in the project was more or less casual. I mean, we looked for people interested on the subject that could make sense to incorporate. There were some criteria, of course. The countries selected were all very unequal and counted with quite large populations. The only exception was Haiti. Haiti was actually a case that I decided to include because I had some funds available and there was so little published about Haiti. So, I said, well, let me do something that might be useful. There was not even much data on Haiti, almost no data at all. And I knew someone who wanted to do a dissertation there. I contributed to his stay there, and he did the study for the larger project. But the other ones were The Philippines, Bangladesh, South Africa, and Brazil. Originally there was also India, but unfortunately the person who was in charge of India ended up deciding to focus on small farmers instead of elites.

It was interesting to compare because the differences help you illuminate a single case, as well as the contrasts. For example, looking at the four big countries, I realized how Brazil was similar to South Africa, comparatively to the Philippines and Bangladesh. Probably if I didn't have as many countries as we had, the four of them, I would have never thought of it. But the contrast tells you a lot. And that was even more illuminating when, 20 years later, I went again comparing Brazil and South Africa and realized that they were no longer so similar in some respects. They were still similar in many aspects but not in their interests. Let me explain. During the first study, Brazil and South Africa were both very much into the economistic vision that you must grow faster to industrialize. Then later, you think of redistribution. When I repeated this study 20 years later, South Africa still thought that way, but Brazil did not. I mean not everybody in Brazil, but a significant part of the elites changed their position. Even part of the business elites was in favor of redistribution. So, it became very relevant to investigate what emerging conditions contributed to the change of perceptions among the Brazilian elites.

You asked me if the elites are similar everywhere. They are similar in the sense that they are all motivated by interests, and provided a window of opportunity to gain from redistribution emerges, they accept redistribution. Unfortunately those opportunities are not very frequent. But when they are available, it's possible to think of realistic redistribution strategies.

There's something curious about that comparative study we did in the five countries. At the time, my research partners were much more interested in studying poverty than inequality. I had to insist with them to put "inequality" in the book title. They said, "Well let's call it perceptions of poverty. This is what really matters". In the end, I convinced them, and the book title included "poverty" and "inequality". Well, now everybody is worried about inequality. I think the evolution of neoliberalism made all of us concerned about inequality because inequality reaches such a degree that we all must confront it. How is society possible if people have such

different life perspectives, life expectations, et cetera? I mean, I believe that inequality today is as relevant an issue as other existential problems such as pandemics, climate change, chemical weapons. I mean, all of that are things whose collective nature demand collective solutions.

4 Theorizing from the South

LSL: Another issue I would like to discuss is what does it mean to theorize from "the South"? You told us a little bit in your PhD trajectory about your relationship with theory, but could you tell us a bit more, at this moment of your career now, about your relationship with sociological theory. Why did you choose to teach it when you were such a senior professor? And what do you think are some of the challenges and opportunities of even using this word "theory" when you are not in the epistemic centers like Europe or the US?

ER: Look, I always thought of theory as something that must fulfill a role. You know, many people teach theory as a series of biographies of big names. I always refused that because I think the practice of theory is the practice of abstraction. And I like this idea. I told you that I like Bendix. One of the aspects that fascinated me about Bendix is that he treats concepts as sort of a theoretical exercise. And I agree with that. I think when you decide to conceptualize something, you do some abstraction. And if you are rigorous about it, it helps a lot in terms of creating intersubjectivity. When people read your work, they must know what you mean by this or that idea, by class, by group, by elite. So, I think I see "concepts" as the first degree of theorizing and I think if you stop thinking of theory as something ready-made, you can do theory wherever you are, North or South. They are articulation between concepts, between statements that help you to synthesize, somehow to abstract from the extreme complexity of reality.

And I like to teach theory because I think of it as an opportunity to show students that they don't have to be deferential in the face of theory. They can do theory if they want. Unfortunately, it's a path that not many people follow and the idea of being original is something that sometimes is taken in a very distorted way. People think that if they describe something original, they are being theoretically original. And that's not enough. I mean, to be original you must establish a relationship between the things that you look at. And I think that by teaching sociological theory, I could tell students how exciting it can be to theorize. It's something that not everybody likes. They think it's the wrong way. I don't think so. More and more, I'm convinced that we should teach students that they have to practice this exercise of abstracting to be able to understand similarities and that's what theories are good for.

LSL: Yeah, thank you for saying that. It's good to remind myself of the importance of abstraction, learning how to theorize, also connecting to concept building. Because many times, for example, with this contemporary push for post-colonial theories, there is a concern with the inequalities in the theory-making process: Who has a voice, which audiences hear you, etc. And because I am from Brazil, I am always pushed to answer, "What do you think about post-colonial theories?" And I always feel uncomfortable with this question because all the post-colonial push in the US seems to me completely disconnected from what is happening to sociology in Brazil. How do you react when people bring this type of debate to the table about all these inequalities that exist in theory-making?

ER: Look, this thing about post-colonial, I don't like the expression itself because — contradictory as it may appear — it implies some deference to the colonial past. Well, I understand

that it has a role in academic communities. At my age, I can see how similar the post-colonial discourse is to the previous dependency theory. Better saying, they are not similar, but they fulfill a similar role for a significant part of the academic community. In the sixties and seventies, everybody was talking about dependency theory. The so-called "underdeveloped countries" at the time, or Global South in today's terms, and later on also some scholars in the US and Europe, were thinking in terms of dependency theories. In retrospect, dependency theory was not a theory. It was a departure point, more like an angle to look at things. And that angle created some sort of a community, a common language among the Latin Americans for example. Actually, first Latin American, then the rest of the underdeveloped world at that time.

It's interesting to have a common language. It's also — in psychological terms — interesting that people create some strength when they think that they are mobilized against something. So I am from the South; I'm doing post-colonial work, but actually most of the post-colonial discourse is mainly a criticism of the theoretical status quo. And criticism is not enough. There's very little theoretical formulation that actually contests what is seen as the old colonial theory. And there is one additional problem, with respect to the criticism of the so-called hegemonic theory. Actually, what is hegemonic is something that has its origin in the birth of social science as science.

There is a genealogical issue to be solved because social science as such was built in a given historical context. The first social scientists, like it or not, were in the colonial center. Now, in my view, not only us in the Global South, but everybody must take into account that we all belong to the same universe today. Even if there are a lot of asymmetries, we all belong to the same universe. We all have the same communication channels. So, we all must face this problem of how to overcome the birthmark of colonialism. And it's not only the South that is affected by that. Think, for example, of the foreign migration issue today. It's clearly the last chapter of colonialism. I hope it's the last, that we find the solution, because what we are experiencing now are former colonies revamping the flow and coming to the old centers. Look, the people who migrate to Europe or who migrate to the US today, are people who are doing that because they are in a sense the surviving victims of colonialism.

It's clearly the last reminiscence of colonialism. The reverse of the flow. And people feel that they go there because there is something that tied them together in the past, even though in very unfair terms. But they come to the old center because they were on the periphery before. So, the whole thing of colonialism, it's a new problem for us all. It's not just for those in the South. Unfortunately, it's everywhere. I think the post-colonial theory is not very much focused in that.

As I see it in the US, for example, it's mostly an offspring of the identitarian issue. And that is another problem we face today. The normal trajectory of individualism brought us to this stage in life where identity became the big issue. So, in a way, identity and post-colonial theory are not completely separate issues. They are both a problem derived from the process through which so-called modernization took place. And it's interesting that modernity itself is no longer so central as it was a few years ago in the discussion of theorists.

5 What's Next?

LSL: Changing a bit of gears, more recently, you've been involved in international forums, such as the International Panel on Social Progress and the International Science Council. And I'm curious as to what drove you to be involved in this type of initiative. What are some of

the things that you learned by being engaged in these forums? And if you can connect that — because I know a lot of the work on social progress connects to your current research agenda — what are some of the things that you are excited about?

ER: I think one of the things that mobilizes me is that I want to react against the ongoing pessimism I see around. Maybe it's naive, but I keep thinking that if I didn't believe that some change is possible, I would move on to do something else. I would stop doing social science because it would be too boring to be studying something that has no perspective to change. Of course, there are no ready-made recipes. But if you look at historical processes, you see where things went wrong, what could have been done better, why we are failing. And even if we are not getting better, it's important to know that it would have been possible if people had chosen different paths, acted differently. So that is something that always mobilized me — could be some personality trait, I don't know, but I refuse to accept fatalism.

Being involved in international organizations to me is a way to talk to other people, to look at how could we have done it better? And most of all, I like the possibility of dialogue with other disciplines. All scientific disciplinary activity is very lonely. We do very tiny things because reality is too complex. But if you listen to others, at least you can glance at the world of possibilities that exist. And how could we think of acting together? Like, for example, during the pandemic, I was involved in the international network of pandemic research. We opened to people all over the world the possibility to join together to talk about their own project. And so many interesting dialogues happened, putting together people working on health, science, environment, sociology. That is something that I think is rewarding.

Besides, I tend to believe that, at this point in societal life, science is being redefined. There is so much progress that science is creating — new possibilities, suggesting new paths, creating new disciplines — even if, to some extent, we are all very much caged in our disciplines. On the other hand, new possibilities in our dialogue are creating new disciplines. For example, biophysics or biochemistry, so many new combinations of disciplines that were born out of this. So, I like to explore this possibility. And both the International Panel of Social Progress and International Science Council are examples of that. There is also the World Academy of Sciences for the developing world (TWAS) where I am, which also has this objective to put people together to explore joint possibilities and think collectively.

LSL: But when you say you want to react against the pessimism, can you share some of the optimistic things that you are seeing? I teach global poverty at the University of Michigan. And if you look historically, there has been social progress if you treat the world as an average; but then you will see that there is geographical inequality because some places are actually worse than they were 50 years ago. But on average the world is better. So, if you take the long-term into consideration, we are better today. But if we consider genocides, pandemics, our climate crisis, there are also new existential threats and challenges. Given this current moment we are living — where we don't see globalization as necessarily the solution, but it's also not necessarily the problem — how do we think about progress, and how do we think about existential problems that we have to find a collective solution to?

ER: Look, it's not easy not to be pessimist when the existential threats are so obvious to all of us. I mean, some people will suffer more, but we are all going to suffer from climate change, for example. Yet, if we could take collective actions, we could mitigate it. How to do it, I don't know. But I agree with you, there is extreme inequality spatially or even in a place like the city I live in. But, as you say, on the average, there has been progress. Not only in material terms, but I think of issues like abolition of death penalties — it's been a progressive, positive change.

Less countries have death penalties nowadays. You also see a trend towards more rationality on things like gender differences, which still has such a long way to go, and has even suffered some setbacks, but thinking in global terms there have been improvements. Some these problems have been overcome in many places and issue-areas through participation of civil society.

I know it's not extremely operative, but if you think that today the United Nations is a place where hundreds of non-governmental organizations get together, it is something impressive. Maybe the progress they get is a very tiny progress, but it's still progress. They voice the concerns of those who could never voice their own concerns. So, you know that I was part of the team who published the book, *A Manifesto for Social Progress: Ideas for a Better Society* (Fleurbaey et al., 2018). I think we have examples in the book of possibilities for improving life conditions. And by the way, the International Panel on Social Progress is launching its second phase later this year. This time we are going to talk directly to people who act. I mean, we realize that we cannot remain secluded as thinkers. We have to be thinkers who have an open dialogue with "doers." And the idea is to offer them, not recipes, but some solid evidence that can be used for social policy, for example.

LSL: That's nice. Well, with this message of hope, I wanted to thank you Elisa, and I hope to continue these conversations in the future.

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