"These Were Not Boring Meetings":
Miguel García-Sancho Talks with Karin Knorr Cetina

MIGUEL GARCÍA-SANCHO[1]
UNIVERSITY OF EDINBURGH

KARIN KNORR CETINA[2]
UNIVERSITY OF CHICAGO

Abstract

In this interview, Karin Knorr Cetina evokes the first Annual Meeting of the Society for Social Studies of Science at Cornell University in 1976 as a foundational moment for science and technology studies (STS). This conference consolidated a new approach to the study of science based on the anthropological observation of scientists at work in the laboratory. Knorr Cetina argues that, despite geographically cementing in the United States, this approach originated mainly through the work of European scholars. The years that followed the Cornell meeting were marked by intense debates between the defenders of this anthropological approach and other scholars more focused on ideas than on scientific practice. Knorr Cetina describes these debates as "bloodbaths" and recalls having first coined the term "constructivist" as applied to science studies in 1977. For Knorr Cetina, STS is now shifting its attention from the production to the consumption of technoscientific knowledge. Her current interest in the financial markets and other forms of screen technologies is an example of this transition. She argues that STS needs to overcome its current fragmentation and emphasis in isolated case studies. The establishment of basic research centers with the financial resources to develop collective and long-term programs would help scholars to expand their horizons. In his following reflection, Miguel García-Sancho explores the connections between STS and travel in both a sense of intellectual shift and a more mundane meaning of physical movement.

Keywords

Karin Knorr Cetina; 4S; constructivism; laboratory studies; ethnography; social studies of science; sociology of finance

MG   I would like to know about your entrance into the field of STS.

[1] Miguel García-Sancho, Email: miguel.gsancho@ed.ac.uk
[2] Karin Knorr Cetina, Email: knorr@uchicago.edu

KC   I think the basic point is that I was quite interested in science. I was at the Institute for Advanced Study in Vienna and they had a scholarship program, you could do some sort of a postdoc there, and there I met other people—Hans-Georg Zilian for example—who were interested in science and we studied Hans-Georg Gadamer and others who had worked on hermeneutics.[3] At that time in Europe you had the, what in English might be called the fight, the struggle over positivism going on, *positivismusstreit.* I'm not sure whether that was something that happened much in England, but there were people in England involved in it: in Germany it was Jürgen Habermas and in England there was the philosophers Karl Popper and Imre Lakatos, and also some social scientists. The question was, to what degree the positivist model of natural science, was applicable to the social sciences. So the fight, the discussions—when I say fight or struggle it was really more ongoing discussions and workshops and conferences on the topic—was about the status of the social sciences in relation to the natural sciences. I stayed at the Institute for several years and it offered a fellowship to go to the United States, a Ford Fellowship. I got this fellowship—I think it was in 1976/77—and I chose to go to Berkeley on the recommendation of Prof. Aaron Cicourel, who had been one of the guest professors at the Institute. Before I left, I did a small study in Vienna of some natural science units. It was a study of the organization of the natural sciences in research units, the Austrian part of a six-country study. Using a common questionnaire, we went to researchers, research teams in companies and state institutions and asked about what was conducive to their research and what was not conducive. I mention the study because it frustrated me a lot, it led to the sort of cathartic disorientation that Levi-Strauss reported after his fieldwork among native tribes in the *Tristes Tropiques*. I pre-tested the questionnaire myself so I went and talked to people using the questionnaire. I was a novice sociologist with no experience of fieldwork, and these people, nobody answered the questions as they should. We had a Likert scale five, with five answers—agree very much, agree, agree approximately, etc.—and they were supposed to answer along these lines. Nobody did that, they always deflected the question back to me, and said how do you mean that, do you mean it in regard to this or that, when I make that assumption the answer is this, when I make another assumption the answer is that.

MG   What were the main questions you were asking?

KC   The questions were simple questions like, do you have the financing you need, do you have the equipment, is the equipment important and how do you work in a team together; what is the communication structure; things like that. We were to find out more about how these research teams were organized in practice and how they could be better organized. A six-country comparison was intended to give us some results on where it worked, where it didn't work. Depending on who I talked to, I got thrown out of the questionnaire in different ways, so if I talked to researchers and technicians, they would

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[3] Hans-Georg Zilian (1945-2005) became the leader of the Styrian "Thinking workshop" devoted to labour market and unemployment research. Hans-Georg Gadamer (1900-2002) was a German philosopher best known for *Truth and Method* (1975 [1960]) on hermeneutics.

have different problems with the questions, everyone had problems with the questions and wasn't ready to answer as they should have in this quantitatively oriented survey research style way. I believed in this method, I didn't really know anything, so I said OK, this is the method of sociology, I will use it and I will do the Austrian part, and see what I can get. But the experience was traumatic because I couldn't, I started to doubt the scientificity of sociology, I can't apply this the way I should, they won't let me, they give me other answers, what's going on? So I did my best but I was annoyed. So when I came to Berkeley I was very, very frustrated with these experiences and thought about doing something else, and thought about going to the lab. I tried to get into the Lawrence Livermore lab but I couldn't get in because I was a foreign citizen. So I went into the lab in which my husband, a microbiologist, worked because that was the easiest way in, but I didn't believe in what I was doing. I had read a lot of philosophy which was the wrong thing to do so I believed that scientists think differently and in the lab you couldn't [capture anything about the way they thought]. It felt foolish to go there but I went there anyway because I had a year in Berkeley, I wasn't going to do another survey research study, I had finished the Austrian research, at least enough to leave, I wanted to do something else.

MG   What was the lab in Berkeley?

KC   It was called the Western Regional Research Lab. It had about 300 researchers some doing very basic research in the microbiology/biology area, some doing more applied research dedicated to agriculture and some health issues too, but mostly agriculture. And so what happened when I went into the lab—I still remember the first morning I went in––was that because it quickly became obvious that what I was watching was really an action system, there was not one researcher in front of a desk, there were several researchers. It was a bench-work lab so there weren't huge numbers, but there were 3-5 researchers, talking to each other and doing manipulations in the lab, it was very much observable: there was no problem observing it. And when they were talking and I asked them questions, what came out wasn't some special sort of thinking, it was normal thinking. I had bizarre ideas that maybe scientists think differently, it was completely foolish, of course, after the fact. Sometimes I didn't understand the technical terms and had to ask more questions about what they meant technically, but there was no special logic or anything involved, it wasn't particularly complicated, you just had to do ethnography or you had to do observation studies, which is something I knew, not because I had a lot of experience in it—I didn't really have experience—but I knew it by heart from my teaching experience. I had taught a methods class at the university so I knew ethnography inside out, and I applied it. Then, a little later in October, was the first 4S meeting at Cornell University.[4]

MG   The very first one?

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[4] A copy of the program of this meeting can be found here http://www.4sonline.org/files/orig_prog.doc

KC   It was the original meeting, yes, the Society for Social Studies of Science had been founded the year before in 1975, and I have still a little needle which says I am a founding member of that society. So I went to that meeting and at that point, I hadn't done much research yet, but at that meeting there was Bruno Latour, there were some of the older generation of science studies people, like Diana Crane[5], I remember her from the University of Pennsylvania, I think Harry Collins was also there, I cannot remember whether David Bloor was actually there or not but he might have been there. I also think Thomas Kuhn was there, because we had read Kuhn and he was an important influence for all us, it was '76 so the second edition of the *Structure of Scientific Revolutions* (Kuhn 1970) had come out. So I met these people, I listened to these people giving talks and I found that meeting very exciting, it was very stimulating and inspiring for me because I found someone who did the same sort of study—Bruno Latour was at San Diego at the time—and we immediately decided we would get together after the meeting at some point. We had, I think, at least one, if not two, meetings with Bruno Latour in Berkeley, and we talked about what we were doing, and we talked about Kuhn, and we talked about Harry Collins. We also talked about David Bloor quite a bit. Bloor had published his book *Knowledge and Social Imagery* at that time (Bloor 1976). So this getting to know people who did similar things, who for the first time went and looked at the natural sciences as a sociologist, a social scientist, that was really the first time this happened; there had been no other lab study I was aware of before, and the only thing we were really all aware of was Kuhn. But it began to emerge at that meeting that there were more than two people doing this, there was Bruno Latour, I was doing it, David Bloor was doing something along these lines, not a lab study but also looking at the natural sciences.

MG   Sociologically?

KC   The sociology of knowledge dates back to the 1930s and 40s—Barry Barnes has written about that for example—but what Karl Mannheim and Max Scheler[6] had not done was apply it to the natural sciences; they were of the opinion that social conditioning of thought is something that happens, maybe, in the social sciences but not in the natural sciences. So this was the first time that a sort of sociology of knowledge approach was extended to the natural sciences [and one of the places in which] that happened was Edinburgh. Gradually, during '76/77 when I was in Berkeley, and then when I went back and during the next two or three years, I became aware of all of the people who were doing similar work in different places. Several conferences were organized and Bloor's book (Bloor 1976) was out, it created a row because the philosophers thought that it was an attack on their take on the natural sciences. And so there were meetings being

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[5] Diana Crane is professor emerita of sociology at the University of Pennsylvania who currently works on the sociology of fashion.
[6] The sociologist Karl Mannheim (1893 -1947) and philosopher Max Scheler (1874- 1928) both contributed to the development of the sociology of knowledge

organized for example, one was at Rensselaer Polytechnic organized by Sal Restivo[7], another one was in Canada, Toronto maybe, and Don Campbell, a very renowned psychologist who was interested in science came there. And there were these bloodbaths, these controversial discussions being set up between the people who came from sociology and philosophy. All of that was of course very exciting. It had already started at the Cornell meeting because at that meeting there were some philosophers, I believe Kuhn was there, for example. And then another philosopher, Larry Laudan[8], attacked demarcation in philosophy, but also attacked Bloor and the sociology of science for not understanding what the natural sciences were really about. These meetings, but also the discovery of people—at the time there were just five or six of us, Barnes, Bloor, Latour, me and Collins, becoming aware of each other, even if we didn't immediately meet personally. All these people becoming aware of each other and becoming aware of the fact that there was something to be done, a new thing to be done and discovered.

MG   At that point, were there differences between the sociologists as well? Were there also debates between Bloor and Latour, you and Latour?

KC   Not really debates that resulted in mutual attacks or anything. Because we were happy to have someone else doing something, we were supported by that, but there were obvious differences in methodology and there were differences in personality and there were latent tensions at times. So I found I had relatively close relationships to Latour at that time because we were both doing a lab study. Bloor, and then the students at Edinburgh––Donald MacKenzie and Steve Shapin—were doing much more historical studies and working with an interest approach. So the differences were between the much more historical approach of the Edinburgh School and their work with notions like interests, which I didn't do. I didn't have an historical approach I had an ethnographic approach, more of a lab studies anthropological approach, and I didn't find the notion of interests very useful because I would have to link what goes on in the lab to how the scientists are embedded in social classes and what their outside relationships are, and this didn't seem to be obvious in the contemporary world. For example, high energy physics, which I studied later, is such an esoteric field, it's very much path-dependent on its own past, so what happened in physics earlier is very important for what happens now, but it's not obvious that what these physicists do in the lab is more or less influenced by what they do in their spare time, partly because they have no spare time, they're thinking and doing physics all the time. So the Edinburgh approach was not so applicable in my area, which was about using ethnography and trying to find out from the ground up what was going on in the lab.

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[7] Sal Restivo is Professor of Sociology and Science Studies in the Department of Science and Technology Studies, and Professor of Information Technology in the Information Technology Program at Rensselaer Polytechnic Institute in Troy, New York.
[8] Larry Laudan is a philosopher of science. His most influential work is *Progress and its Problems* (1977) and he currently works on legal epistemology.

MG   At that time as you were travelling between the two continents did you perceive differences between what was going on in the United States and in Europe?

KC   Later on, we discovered Michael Lynch and Sharon Traweek. I didn't know of them when I was in Berkeley because they published their work around 1985 and they were not present at these meetings, in my recollection. Their work was interesting because until then it was really Europeans who did these sort of studies—people in England, Latour in France and I came from the German area—and in the United States scholars had not done or were not doing anything like that. These two scholars, Sharon Traweek and Michael Lynch have to be counted as being in this first generation, and also to some degree Sal Restivo because he was part of the conversation from the beginning although he didn't do a study until later.

MG   What were the Americans doing at that point?

KC   The established American sociology of science was there. This was the second generation of the Merton School of sociology of science, and the difference between that Merton School and what we were doing was pronounced, and we also made that difference a topic of discussion. Merton, of course, initiated a sociology of science in the United States but he thought, like Parsons and others, that really what you could do as a sociologist was look at the institutions of science and its organization, and with that came questions about forms of communication, the reward system in science had been studied and was being studied by Warren Hagstrom (1965). Derek De Solla Price (De Solla Price 1967) had been doing citation analysis before he talked about big science and small science, and researchers were looking at communication patterns between scientists and scientists in organizations. So they took this approach, which you could call externalist, it was sociological rather than political; you didn't look at science policy necessarily but at the outside communications and the institutional structures in which science worked. Merton's (1973) famous article about the norms of science had been published but these norms could not be found easily in scientific practice, there were also counter-norms, there were other things going on. So this whole normatively and institutionally oriented approach was rejected.

MG   And I guess in your ethnographies you wouldn't find that. So you would find a contradiction between these abstract norms that Merton was formulating and what you would find in the laboratory while observing the scientists?

KC   The critique of the Mertonian approach to science, and of its functionalism, was coming out of science studies. This was not a critique that was abstractly made before we did the empirical studies, in my recollection; it was only when we had done the empirical studies that we became quite clear that, when you look at the actual practices of science, you find things that contradict the philosophical take on science and the Mertonian sociological take on science. So this whole group of science studies scholars, called itself the new sociology of science, making the point that this was really a different direction, it looked at the epistemic practices, it looked at the close related practices of science, and it didn't look primarily at the institution or at the organization, or at the communication patterns.

MG   What do you think were the main contributions of your early publications?

KC   I think I was the first scholar to use the term *constructive* as applied to knowledge. I remember that I was sitting in the library trying to make sense of my data. I took data, I wrote them down at the time by hand, I recorded some data, and then I sat in the lab and tried to type them out. I was thinking about it and it came to me, it just occurred to me. I published an article, this 1977 article I published is, I can't remember the title but it has constructivist science in it (Knorr 1977). I think I was the first to publish that and it did not come from Berger and Luckmann, because it was a very different sort of constructivism. Berger and Luckmann (1976) really had talked about how things that were socially constructed, were legitimized, how institutions were social constructions, whereas we found that constructivism in the area of practice, of people acting. So it was much more a combination of an interaction-oriented approach, if you wish. The legacy is more from Marx who can be seen as a constructivist since he talked about work, human labor, as constructing things, human labor as creating things. I don't think that Marx used the notion construction, but the theme was in there in Marx. When I tried to make sense of my data and tried to find a concept for what I saw, the term construction occurred to me, science is really constructive and cannot simply be understood in terms of a correspondence theory of truth.

MG   What do you think were the factors that led to the emergence of this constructivist study of science in Europe and made it differ, politically and sociologically, from what was done by Merton and his followers?

KC   I think it was more political only after the fact, because it was taken to be partly a critique of science, which was not intended. What was intended was to do an empirical study of the actual practices, and we didn't know that it could be done, I didn't believe it could be done, but when we saw it could be done, that was the result. I think what may have contributed to it was that, at the time, in various countries in Europe there were these discussions and conversations over positivism and the role of science, initiated by Habermas for example, from a more political point of view. Was science guided by interests or not; were interests relevant to science; were scientists really that neutral, didn't they have the exploitation of nature more in mind than actual science; wasn't the exploitation of nature something [questionable]? The issue of knowledge society was not discussed at that time, but what you had in the 50s and 60s was a discussion of the military industrial complex. And the military industrial complex included science because in order to make military progress you needed to develop technologies. That had been discussed and been criticized. But that also alerted you that we were really moving in the direction of a knowledge society and there was the intuition more than, maybe, the certainty that science was becoming much more important than it had been previously for the development of society. I think that was the context that made it interesting to look at science as another area that could be empirically studied. Science became economically more obviously important, and probably the atomic bomb, all these things played a role in that growing interest. The increasing preoccupation with science in

society made it possible for this work to be financed. When I went back to Germany, the work that was later published in *Epistemic Cultures* (Knorr Cetina 1999*)* was financed by the German National Science Foundation, so the interest was there.

MG   How did this approach spread during the 80s? What were the factors that made it grow?

KC   The first factors certainly were that we identified each other and had these controversial meetings, these cross-disciplinary meetings with historians and philosophers about the status of science and the status of knowledge, and whether truth was to be explained by a correspondence theory or whether it was explained in the Bloor sense—like falsehood—by social factors. That was what philosophers disliked. So in bringing three disciplines together—history, sociology and philosophy —you had these meetings which were highly polarizing. This gave the whole STS thing some attention because these were not boring meetings—they were packed meetings, everyone was interested in how one side destroyed the other side, and whether you succeeded in it. So it raised a lot of—how would you say that today?—a lot of buzz, and people who were not directly involved were listening to it and others carried it further. You had that discipline-transcending approach going on right from the beginning in science studies. STS was not purely sociology: some of us were sociologists—Harry Collins and I were sociologists—but not that many of us were; David Bloor was a psychologist, Bruno Latour was a philosopher by training. But STS had that cross-disciplinary approach: the philosophers were talking about it, the historians were talking about it, and also there was a decision that history, sociology and philosophy, because they addressed similar topics but with very different methodologies and different findings, should continue to talk to each other. So we agreed that the Society for Social Studies of Science would have co-located meetings in the future with the societies for history of science and philosophy of science and we would continue to talk to each other. I think it was the interest in science *per se* in many areas, in science and knowledge, and the feeling that something new was coming out of this field because it was truly for the first time that one had thought of the natural sciences that way and that led us to some interesting results.

MG   The professional migrations that took place between Europe and the United States, do you think that could have also contributed to this transatlantic growth of STS? There's your case, I'm also thinking in Steve Shapin later on.

KC   I think we brought this stuff to the United States and we established it in the United States. Latour went back to Europe, but he got the offer from San Diego to be a visiting scholar every year for a while. After being in Berkeley, I had an incentive to come to the US as I wanted to get out of Austria anyway—it was too middle Europe, too stuffy a country. Then there were others in the US—post-Sharon Traweek and Michael Lynch—who took this up, for example, Tom Gieryn who was at the University of Indiana, a student of Merton, and he took to this approach. So American scholars took it up and carried it further.

MG   Let's jump a bit in time. If we take your next major work, *Epistemic Cultures* that was published much later in 1999, how would you say the field was transformed, and what were the main transformations?

KC   On the level of content, the field changed because different approaches took root and developed. STS studies were not lab studies any more, they were doing other things: the feminist approach, discourse analysis approach. You had these different approaches taking root including the move into technology: the field was changed by Trevor Pinch, a student of Harry Collins, one of the first writing about technology as constructed. Trevor Pinch carried the constructivist approach that had been developed in the lab studies, over into the study of technology. And he also created a little bit of a program for technology studies. Studies of technology had existed before, but they were like history of ideas studies, they looked at how one technology developed out of another technology. There was a technological determinism in sociology saying that the technology really mattered a lot and explained how institutions changed, but opening up the process of technology creation to be studied as a practice, that was not the case. Wiebe Bijker in Amsterdam, and Donald MacKenzie and Judie Wajcman in Edinburgh also did that. These people all talked to each other at some point, so it's not so easy to say any more that that person invented that or was the first to do that. Trevor Pinch was certainly one of the first to publish a whole book on the issue of technology using a constructivist approach. On the institutional level, there were a number of science studies units created, for example several in the Netherlands. In the United States the science studies unit at Rensselaer Polytechnic became a department, probably one of the first science studies departments, and the programs at Cornell and at San Diego took off. So institutionally the field developed by the creation of programs and centers and even departments. So every new generation, every five years or so, you had people coming into the field picking up some of the concepts and working with these concepts and then also creating new types of research, for example multi-sited ethnographies, anthropology took to science. I used an anthropological approach and my degree is partly in anthropology, but anthropologists themselves at the time hadn't really looked at science, they started doing that with Sharon Traweek who was an anthropologist, and now science studies is a very important part of the anthropology department here [in Chicago]. Other sorts of things [began to be explored], for example Tom Gieryn began looking at architecture, how lab architecture influenced what was going on, so that the field diversified in terms of the approaches and content in that period when it became institutionalized as a field in its own right. That was a discussion at the beginning: should science studies be a field in its own right or should it be part of sociology, part of history, part of this and that? But it just became a field in its own right, through the MIT science studies program, the Rensselaer science studies program, the San Diego science studies program, and many others that are not so prominent. What happened then is that once you institutionalize the field, meaning you create units and programs around it, you train new students and as you train new students new things emerge and they become established.

MG   How did this consolidated and diversified STS of the 1990s and early 2000s influence other academic fields?

KC   I don't know exactly when it originated but I am sometimes surprised to get invitations from fields I don't read. For example, organizational sociologists have to work with knowledge and scientists in organizations, and they somehow took to my work. The area of education, I have had nothing to do with education up to now, but just two weeks ago I got another article that uses the notion of epistemic culture in education, they use terms that we invented in science studies, cite us and invite us. I've seen it in economic sociology but that was a little later, that was after 2000.

MG   And how can STS shape other areas outside academia?

KC   Well it depends on the studies you do, I don't think you can give a wholesale answer to that. If you want to do an interventionist study that is not really my orientation, I like to do basic research, as Bloor does in my view, as MacKenzie does in much of his work, although he has a site where he engages with the public more, maybe because it's more required in the UK. I think that depends a lot on how you do the study because if you do a study like the epistemic cultures study, the general public might not be interested in these questions because of the sorts of results we generate. In fields like education, they take the epistemic culture notion, and say, OK we have to see all knowledge in terms of cultures and we have to then think about how you educate, how you give credit to the fact that you cannot simply transmit knowledge as if it were all of one kind, but you have to take into account the fact that in education too you have different epistemic cultures and you need to pay attention to them. So there are fields which translate into practice I would say. Organization studies is also one of these fields and the education area, these are the ones I know best. The interventionist demand is not so strong at the University of Chicago, so the question is where does the demand come from? Anthropologists tend to be more interventionist now. One of the reasons for that is they also have to do with populations which are constantly exploited and misused. For example we have Kaushik Sunder Rajan here in the anthropology department, he looks at how the pharmaceutical industry tests its products in India and what form of exploitation is going on there. And these people, also at MIT there are people now who are doing work on knowledge that's very practical knowledge, for example there's one scholar, Heather Paxon, who worked on cheese-making as a form of expertise … I have a young scholar here who comes originally from Germany, Alexander Dobeson, he's a pre-PhD, but he looks at the fishing industry in Scandinavia, he uses an STS approach for that, including questions of technology because the fishing industry apparently uses screens like you have them on the trading floor, to monitor now what goes on in the ocean and all these fishing boats, where the fish are. So technology and knowledge have infiltrated a lot of practical fields and there if you do these studies, you look at the fishing industry at what it does you can tell them some things about how they could improve the situation. But if you look at basic natural sciences like I have done, then nobody cares really in practice about this sort of knowledge.

MG   In this regard, how has the experience been of applying STS to the study of financial markets, as you have recently done?

KC   In the financial market areas, I had a lot of trouble using concepts in science studies and it took me a while to discover why I had these troubles. In most of these areas you don't really produce that much knowledge, you extract knowledge from the market data, from the price for example, and you consume knowledge, its information knowledge so you have to have a consumptionist approach. It took me quite a while to understand that. I tried to apply our own concepts, and I thought what is going on here, it's not working, it's not making sense of what I see. You have to then pay more attention to the fact that these information elements are streamed on screen: the users, the traders, think of the price as information, they think of what's going on there as knowledge but it's not a knowledge production enterprise like you have within physics.

MG   So you needed to change the approach?

KC   I needed to change my concepts and my approach, also the concepts, so the productivist bias we have in the sense of looking at the production of knowledge in this first generation of work could not be maintained. I switched to a notion of consumption and I have a paper on that, information consumption (Knorr Cetina, 2010), because that did occur to me as the only possible way to make sense of what's going on [on trading floors].

MG   Would you say that maybe now STS needs to pay attention to that, to how all this knowledge that has been created is now being consumed and used in the financial markets or maybe in other areas?

KC   Yes, because there are huge areas and knowledge is now almost everywhere, so there's hardly any area that… If I go to the airport tomorrow this is a huge expert system, you could do a fantastic study that nobody has done about the knowledge aspects of an airport, technology and knowledge play a huge role. In areas like financial markets, when you see the price – it's all about prices of financial instruments – and then you see the price as information you have made that connection. So you have this fusion in areas where you both handle knowledge, take knowledge, make knowledge, work with knowledge and at the same time make money, work with money and create profit. It is one and the same thing. It's tough, I find it much more difficult to study because of these dual levels. It is no longer isolated, it was so nice when you had the isolated natural sciences behind walls and you could study them there, and you knew that the rest of the world was shut out, and it influenced maybe a little bit of what was going on, it was paying for what was going on, but it didn't interfere and it stayed outside, and they produced their results for their own communities. That's a much easier situation for us as ethnographers than the one now.

MG   Your more recent studies of the internet and social media, do you also think these distinctions you were mentioning apply as well?

KC   Once you have screen technologies in the picture, let me not be too precise, let me just say screen technologies, the ones you see on a trading floor, computers with screens, with

the appropriate software. You can process the material, analytically you can do a lot more things with this material, whatever is on there; whether it's pictures or numbers or even text. If it's text you can do large-scale quantitative analysis on the text. The knowledge possibilities are immediately immensely big. The knowledge production possibilities are immense once you have things on screen, and the screens are everywhere. For example, in my present study, of what I call scoping media, one study is about extreme sports. You would think these people who do extreme skiing, go down the hills outside the *piste* and make big jumps over the road and turn themselves around three times, you would think they have enough to do, but they also often carry a camera on their head, or lined up alongside where they make their jumps are people with cameras, video cameras, and they film everything. So they watch their own activities immediately afterwards and while it is going on, and they form communities around that, they put the stuff on the internet, others can watch it. Not all of that is analyzed in terms of knowledge, but quite a bit of it is, and it can be used for knowledge purposes. These are information data that are collected, even when I go with my cellphone into a department store they may suck data from my cellphone and store it somewhere, and this data can then be used. So maybe we should call these sorts of information knowledge.

MG   Big data?

KC   Yes. This is knowledge, this is interpreted, it is not raw data in any sense as it has tags, who, where, what. You have all kinds of meanings associated with it, and that's so big now and so widespread that of course you have this whole area that's not within sciences strictly, it's always, I don't want to say corrupted, but it's always mixed with other things that are not scientific.

MG   Another thing we've covered partially before, there's been I think an increasing specialization in STS and now we've got communities of ethnographers, historians of science, innovation analysts, it also has to do with how STS has been institutionalized. Do you think maybe after that we need a bit of cross-fertilization?

KC   In principle, yes. When I go to 4S meetings, I am often almost annoyed, I would, say by the fragmentation of the field, it's very fragmented and the fragmentation has an epistemic component so you don't see a lot of, or even a little bit of progress. You don't want big progress if you don't believe in linear progress, but if you have a fragmented case study here, and a fragment here and another case study there, and they are not ever brought together, nobody makes an attempt to compare these studies and do something with the comparison. Physicists do that very well. They are not necessarily always a better science but they have a number of mechanisms to pull the field together. They do that in their graphs, they put data from different experiments into one graph, and show how the data complement each other. They take 15 different measurements on a particular parameter and then they create an average, they say the 15 measurements diverge on that level but we continue to work with the average or we continue to work with some sort of parameter that brings these data all together, it sums over these data.

So there are technical tricks for doing that and we don't do that in the field of social studies of science. Partly this has to do with there's a lot of qualitative work, and qualitative work cannot be so easily compared. But everyone making different assumptions and doing their own little thing somewhere is not really helping the field in my view, it's not good.

MG   How can we attempt to do that integration of case studies? How can we overcome that fragmentation?

KC   One thing is, something I actually gave a talk on once at a 4S meeting in Washington, I said I need 10 years, I need a center, I need 10 million dollars. I need to create more cooperation and teamwork around a particular topic. That doesn't mean that these persons working together cannot have their own part, it's important for them, but they should also talk to each other and they should work as a team because these are complex phenomena we are dealing with and you have nothing studying them. You need larger teams and you need longer time horizons sometimes, and you need the money for that. So a two-year study, a two-year timeframe is not good for some research. If I had only two years in high-energy physics I would have produced nothing, I was financed for six years by the German National Science Foundation and then they found additional money.

MG   And don't you have the feeling that funding trends now are going in the opposite direction?

KC   Yes, and then funding agencies also want you to work more with quantitative data and once you do that you cannot find out certain kinds of things, you just have to drop them, you ignore them. I couldn't have done a quantitative study of CERN bringing out what I brought out, it would have been impossible because I would have had to know in advance what question to ask, and they would have had to be willing to answer the question on that level, and they don't know often what they're doing, it's implicit knowledge, you can't ask them, you can't use survey research. You have to do more holistic observations, so you have to go within teams and if you compare them it has to be implicitly, all of that is methodologically not possible once you have quantitative data, it just doesn't work.

MG   So what are your main concerns and worries about the situation of the field now? Are there things that are a bit unsettled?

KC   If the field is STS, the worries are different from sociology. In STS I think that one of the worries I have is that STS scholars sort of believe the world started with Bruno Latour, and that's incorrect. Not only was Bruno Latour influenced by a number of earlier writings, but also you can learn a lot by knowing fields beyond STS, for example by knowing sociology. They are using sociological concepts that we have had and continue to have and continue to produce that are not part of STS, so STS shouldn't shut itself up to these fields that were foundational for it because these fields still have also something to offer: certainly, sociology and anthropology gave me methodologies, concepts, analytics, they gave me a lot of things that I brought to bear on what I discovered in

science. So shutting yourself away from the other fields is not good because STS is too young, it's not sophisticated sometimes on the level of exploring its own assumptions and its own beliefs. It can profit from some analytic philosophy occasionally because that explores the logic of argument and STS doesn't teach the logic of arguments, it doesn't teach a lot of things that it should be teaching because it's a young specialized field. What it can do, should do, is to remain open to these other areas and take things from them, and learn them, and not shut them out and say this is all crap. So it should not do exclusively qualitative studies, case study methods because it fragments the field too much, as I said before. It should try and do more comparative studies for which it might require more teamwork because you can't do all of this alone, more comparative and more longer term studies, and also some experimental studies, you can combine qualitative studies with an experiment, it's quite interesting to do that. I've done them last summer, this work is not yet published but I'm trying to, because I get bored by these qualitative studies myself.

MG   How do you regard the way Edinburgh has evolved over these years?

KC   From my more distant view, Bloor, I think, withdrew and did work on issues that are more philosophical than sociological. So I don't see that Bloor had a huge impact in recent years, his impact was early on. But MacKenzie and Shapin as students of Bloor have had quite some impact and took the Edinburgh school in two different directions. Shapin by doing things like the history of truth work, working on scientific life but having really a sociological approach, doing history but with a sociological eye and with sociological concepts, by reviving the notion of trust that would previously have been rejected because it would have been associated more with Merton and his norms of science. So he revived these notions and he went into all kinds of areas with his sociologically influenced history, and that's a nice direction, it's a very interesting direction of work. MacKenzie by moving into the area of economics and finance. That's the evolution. He continues to be best in my assessment when he does historical reconstruction, even if it's contemporary history, even if the history is not old, maybe 50 years old, but he does his work as a sort of historical reconstruction. And he does it in great detail, very interesting and very, I would almost say objective in the sense that he gives credit, he doesn't automatically discredit, he doesn't use the notion of greed as the major explanatory variable. He's doing great work on that level of reconstructing these things and conception, that's the development.

MG   To finish with a positive note, what are your hopes, the things you think STS has potential and is improving and can make a difference?

KC   I think it has potential in its own right as the study of knowledge and science and information and technology, mainly because it opens these things up, it doesn't remain outside, it really looks at the inside of that black box. And that is very important in a knowledge society, where knowledge is everywhere and you have to understand it, you have to open it up, you also have to fight it at times, you have to prevent it from happening, so it's good to know what's going on there, and STS can do that. But the

other thing I think is that it helps disciplines that don't really know knowledge or they don't know technology, like sociology. Sociology doesn't, I'm surprised at times how far removed my colleagues here are from taking technology into account. So, from an STS point of view, people that have had that background can teach fields like sociology that there's not just human relations, there's also object relations involved and what's the meaning of these object relations, how are they feeding social relations or replacing them or substituting for them, and how are they different, what new concepts do we need within the social sciences to deal with this. If you didn't have an STS in place, if you didn't have a field that has that anchor in the actual technology, in knowledge, then you wouldn't be able to do that because it's not a question of saying what has existed for a long time, of using technological determinism, but it's really inscribing things like object relations or material world, the technological aspects into the social theory. That is different from using technological variables as causal variables in some cases.

MG   So you are optimistic about the future?

KC   Yes, I am optimistic, although there are also data which show that these fields, these young fields can go bankrupt, they can die again. But I'm optimistic that the issues are not going to go away, even if the field of STS doesn't make it institutionally. I think the concepts have become so much part of many other fields—anthropology, sociology, economics even, and education and organization, all these areas—that I don't think it will go away.

MG   So maybe after a stage of institutionalization, will STS spread to other fields?

KC   I think what would be nice for STS, in combination with other fields, is to have more centers, more not just programs somewhere where you teach students, but research centers like you have in molecular biology, in genetics, in many natural sciences. Centers allows you to do research in groups, it can be smaller groups but with enough people to address an issue to make a difference, and to do that systematically over time, it could be a Max Planck Institute in Germany, there are Max Planck Institutes but it's for the history of science, but history of science is not enough I think. These centers should not be focused on discipline, they should be focused on topics more than disciplines. And these centers, if the people go there and have enough of a career there so that they stay there would generate more concentrated focused research that would help the field move forward enormously I think.

Reaction, Movement, and Convergence

BY MIGUEL GARCÍA-SANCHO

The day I met Karin Knorr Cetina we were both in the midst of travelling. I had just arrived from Edinburgh to participate in a workshop at Chicago's Field Museum and she was about to depart, the day after, to Princeton. We managed to make our pathways cross at a social club of the Sociology Department of the University. It was a late evening and our conversation involved a lot of discussion about journeys. As I listened to Knorr Cetina, I recalled how the science and technology studies (STS) literature has long addressed the issue of knowledge circulation, interdisciplinary transitions, and the importance of place and locality in scientific experimentation (Secord 2004; Lyall et al. 2011; Collins 1985; Ophir and Shapin 1991). The nomadic career trajectory of my interviewee suggested that STS, as scientific knowledge, may also be seen as a space of travel in both a sense of intellectual shift and a more mundane meaning of physical movement.

Physical movement, as the laws of mechanics show, is a reaction or response of an object to an external force. In the case of Knorr Cetina, the reaction was against positivism and it was a Europe-wide force: social scientists and many natural scientists were disaffected by the rigidity and increasingly mathematical nature of knowledge production. The force originated in the early-to-mid 1970s and had many different manifestations, among them the foundational work of the Edinburgh Science Studies Unit. It is interesting that, despite it being a reaction against positivist epistemology, this rebellious work had an obsession with evidence collection that may be seen as directly inherited from the experimental sciences. Most of the early Edinburgh School members were natural scientists themselves and advocated for a non-speculative and strongly empirical methodology. Knorr Cetina, in turn, devoted most of her formative career to quantitative surveys and it was her dissatisfaction with this approach that prompted her migration to the United Sates.

Parts of the academic world on the other side of the Atlantic were also becoming skeptical of mathematical methods. In the US, this skepticism prompted a rise in and expansion of ethnographic approaches. A new generation of scholars fighting for their own epistemic spaces saw qualitative ethnographies as offering fresh perspectives. These methodologies inspired Knorr Cetina during her visit to Berkeley (1976-77) and in the same academic year, Bruno Latour and Steve Woolgar conducted their celebrated fieldwork at the Salk Institute, published as *Laboratory Life* (Latour and Woolgar 1979; see interview with Latour in this series). Latour and Knorr Cetina were at that time bodies in motion. Due to their geographical proximity, they started to attract each other and organize regular discussion meetings. Their transitory intersection in California was crucial for the birth of the laboratory studies tradition and its later American manifestations in the work of Sharon Traweek (1992) and Michael Lynch (1985).

The motion of bodies stops when they reach a new equilibrium. This happens when the force that triggered their movement is successfully counteracted. In the case of the US migrant scholars, their growing intellectual convergence crystallized in a field with its own theoretical and methodological repertoire. It is important to note that this intellectual convergence was prompted by a parallel process of physical convergence that defined the new epistemic space. The process of putting the academic bodies in motion together started with the foundational

meeting of the Society for Social Studies of Science (4S) at Cornell University in 1976. This conference allowed practitioners of laboratory ethnography to interact with those developing other historical and sociological approaches that effectively challenged positivist perspectives on science.

Cornell's initial gathering was followed by other meetings that, according to Knorr Cetina, were exciting and "packed" because everyone enjoyed watching "how one side destroyed the other." These entertaining "bloodbaths" created the momentum in which science studies consolidated and became institutionalized, with centers across Europe and the US. The first breed of science studies institutions trained a new generation of scholars that expanded the field into its current form: science and technology studies. The emergence of technology studies as a subfield of STS can be seen as a consequence of this process over time: Donald MacKenzie, one of the editors of *The Social Shaping of Technology*, was a student of David Bloor, a founding member of the Edinburgh Science Studies Unit (MacKenzie and Wajcman 1999 [1985]; see interview with MacKenzie in this series).

The Conditions for a New Journey

When scholars in transit become stabilized, new forces are needed to break the equilibrium and cause movement and reaction once again; otherwise, the field they have formed may get stuck. Some interviewees in this Talking STS project warn against STS becoming too immobile. Knorr Cetina addressed the risk of immobility by deciding to travel again after the publication of her masterpiece, *Epistemic Cultures* (1999). This was, at first, an intellectual journey and entailed shifting her attention from the production to the consumption of technoscientific knowledge; from laboratory studies to the observation of the social construction of financial markets. Other scholars, such as MacKenzie and Michel Callon, transitioned to this topic in parallel and have contributed to an increasing focus within STS on objects outside the traditional spaces of science and technology. Forty years after the foundational 4S meeting at Cornell, the 2016 conference that 4S now holds jointly with the European Association for the Study of Science and Technology (EASST) addressed *Science and Technology by Other Means.*

These transitions may invite confidence in new journeys. The shift to financial markets portrays STS as a field that moves hand-in-hand with the rapid transformation of scientific and technical knowledge. The image of Knorr Cetina and colleagues touring the world and vividly arguing with each other demonstrates their attempt to capture something as volatile as knowledge and its impact on society. If this is the object of STS, scholars—like Knorr Cetina in Chicago and me—are bound to be always on the move. While this movement does not necessarily have to be physical, there are a number of advantages in associating intellectual shifts with physical movement and a number of conditions for these actual journeys to happen.

Knorr Cetina outlined some of these conditions in the interview. The first one is sustained and long-term funding—a financial commitment that enabled her to conduct the fieldwork that led to *Epistemic Cultures* and to combine academic positions in Europe and the US. The decreasing availability of this type of grant today may be a cause of the fragmentation of STS

research in self-contained case studies. This tendency, that Knorr Cetina considers especially problematic, may have been favored by the generalization of three-year postdoctoral contracts at the end of which scholars lack the resources to build broader narratives. In other words, the journeys abruptly stop after the three-year projects and the authors of the case studies cannot stabilize their movement by converging with other scholars. The impossibility of matching intellectual with physical travelling leads to a fragmented STS, as opposed to the cohesive field that the migrant scholars put together in 1976 in Cornell. STS now, in contrast to its foundational meeting, is formed by a much broader array of perspectives. While this diversity is undoubtedly an enriching element, it also complicates the development of big-picture accounts.

However, some recently concluded projects are hopeful testimonies that things may be different (Müller-Wille and Rheinberger 2012; Jasanoff and Kim 2015; see Jasanoff's interview in this volume). The researchers in charge of these projects had the institutional and financial support to integrate different perspectives and broaden the scope of their findings. The results are comprehensive narratives showing that a given phenomenon—such as heredity or modernity—can be approached as transcending disciplinary, geographical, and temporal confines. These big-picture frameworks will inspire future scholars to travel beyond their case studies and, ideally, find other like-minded colleagues in motion.

For this inspiration to occur, another crucial condition needs to be met. Scholars are increasingly pressed by small, repetitive tasks that leave little room for intellectual journeys, not to mention being away from their workplaces. As a solution, Knorr Cetina proposes a research institute in which members are given the space and time to think, travel, and collaborate. This freedom could, in my view, also be achieved in the university, as long as faculty are given a flexible amount of teaching and few administrative commitments. Research-intensive institutes and universities already exist in the field of history of science.[9] This suggests that the investigative model of the humanities may help a new generation of STS work, progressing at a slower pace and seeking longer-term goals that go beyond the accumulation of empirical data.

The transformations that Knorr Cetina suggests would propel a second wave of STS journeys. Scholars could circulate beyond Europe and the US, given that the institutional proliferation of STS has led to exciting places to visit and research cultures to know. Time, money, and institutional support are essential to this. Email or Skype interactions do not replace face-to-face experience and, despite Internet forums having become prolific sites of discussion, they do not have—at least for now—the transformative potential of in-site exchange.

Another factor to take into account is that collaborative results are often slower than single-authored contributions. This may clash with the British Research Excellence Framework and other research assessment exercises that demand short-term results. One possible way of responding to this pressure for productivity is to become an immobile, single-author researcher. The system should encourage exactly the opposite: travel in all its possible dimensions instead of the serial manufacturing of case studies.

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[9] See, for instance, the Max Planck Institute for the History of Science, https://www.mpiwg-berlin.mpg.de/ (last accessed June 2018).

Author Biography

Karin Knorr Cetina is a Professor of Sociology at the University of Chicago and at the University of Konstanz, and a member of the Bielefeld Institute for Global Society Studies. She obtained her PhD from the University of Vienna in 1971. Her publications include the award winning *Epistemic Cultures: How the Sciences Make Knowledge* (Harvard University Press, 1999), and *The Sociology of Financial Markets* (co-edited with Alex Preda, Oxford University Press, 2005). Her work has been based on ethnographies conducted in complex expert settings such as the European Organization for Nuclear Research (CERN) and the trading floors of large investment banks in Zurich, New York, London and Sidney. Her current projects include a book on global foreign exchange markets and on post-social knowledge societies. She continues to do research on the information architecture of financial markets, on their "global microstructures" and on trader markets in contrast to producer markets. She also studies globalization from a microsociological perspective continues to be interested in ethnographic studies of science and technology laboratories, particularly in the life sciences and in particle physics. Karin Knorr Cetina is a former member of the Institute for Advanced Study, Princeton. She was president of the Society for Social Studies of Science, and she is currently chair of the Theory Section of the American Sociological Association.

Author Biography

After his PhD at Imperial College London, Miguel García-Sancho worked at Manchester University, Centre for the History of Science, and the Spanish National Research Council (CSIC), Department of Science, Technology and Society. His research interests are in the history of contemporary biomedicine, with special emphasis on the transition between molecular biology and new forms of knowledge production at the fall of the 20th century: biotechnology, bioinformatics and genomics. He is now a Senior Lecturer in the Department of Science, Technology and Innovation Studies of the University of Edinburgh and is leading a project entitled *Medical Translation in the History of Modern Genomics,* with funding from the European Research Council. He has also investigated the emergence of agricultural biotechnology and the cloning of Dolly the sheep.[10] His book *Biology, Computing and the History of Molecular Sequencing: From Proteins to DNA* was published by Palgrave-Macmillan (2012, paperback edition in 2015). He previously worked as a journalist and is interested in science communication and public engagement.

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[10] See www.stis.ed.ac.uk/transgene and
http://www.stis.ed.ac.uk/research/projects/completed_projects/historicising_dolly (last accessed June 2018).

References

Barnes, Barry. 1977. *Interests and the Growth of Knowledge*. London: Routledge & Kegan Paul.

Berger, P. and T. Luckmann. 1967. *The Social Construction of Reality*. Garden City, NY: Anchor/London: Allen Lane.

Bloor, David. 1976. *Knowledge and Social Imagery.* London: Routledge & Kegan Paul.

Hagstrom, Warren O. 1965. *The Scientific Community*. New York: Basic Books.

Collins H.M. 1985. *Changing Order: Replication and Induction in Scientific Practice*. Beverley Hills & London: Sage.

Gadamer, Hans-Georg. 1975 [1960]. *Truth and Method*. London: Sheed & Ward.

Jasanoff S. and S. H. Kim. 2015. *Dreamscapes of Modernity: Socio-Technical Imaginaries and the Fabrication of Power.* Chicago.

Knorr, Karin. 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. Oxford: Pergamon Press.

Knorr, Karin. 1977. "Producing and Reproducing Knowledge: Descriptive or Constructive? Towards a model of research production." *Social Science Information* 16(6):669-696.

Knorr Cetina, Karin. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, Massachusetts: Harvard University Press.

Knorr Cetina, Karin. 2010. "The Epistemics of Information: A Logic of Knowledge Consumption." *Journal of Consumer Culture* 10(2):1-31.

Kuhn, Thomas S. 1970. *The Structure of Scientific Revolutions (2ⁿᵈ edition).* Chicago: University of Chicago Press.

Latour, Bruno and Steve Woolgar. 1979. *Laboratory Life: The Construction of Scientific Facts*. Beverley Hills, CA: Sage.

Laudan, Larry. 1977. *Progress and Its Problems: Towards a Theory of Scientific Growth*. London: Routledge and Kegan Paul.

Lyall, C., A. Bruce, J. Tait, and L. Meagher. 2011. *Interdisciplinary Research Journeys* London: Bloomsbury.

Lynch, Michael. 1985. *Art and Artifact in Laboratory Science: A study of Shop Work and Shop Talk in a Research Laboratory*. London: Routledge & Kegan Paul.

MacKenzie, Donald A. 1981 *Statistics in Britain: 1865-1930*. Edinburgh: Edinburgh University Press.

MacKenzie, Donald. A., J. Wajcman J. 1999 [1985] eds. *The Social Shaping of Technology.* Milton Keynes: Open University Press.

Merton, Robert K. 1973. *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press.

Müller-Wille, S. and H. J. Rheinberger. 2012. *A Cultural History of Heredity.* Chicago: University of Chicago Press.

Ophir, A. and S. Shapin. 1991. "The Place of Knowledge a Methodological Survey" *Science in Context* 4:3-22.

Price, Derek J. De Solla. 1963. *Little Science, Big Science*. New York: Columbia University Press.

Price, Derek J. De Solla. 1967. "Networks of scientific papers." *Science* 149(3683):510-515.

Secord, J. A. 2004. "Knowledge in Transit" *Isis* 95:654-672.

Traweek, Sharon. 1992. *Beamtimes and Lifetimes: The World of High Energy Physicists.* Cambridge MA: Harvard University Press