International Sociology
Review of Books
26(5) 583–596

DOI: 10.1177/0268580911411513
iss.sagepub.com

Words from writers

The interview with **Michael Burawoy**, current (2010-2014) President of the ISA, took place via phone on February 13, 2011, about half a year after his election at the Gothenburg congress. In the conversation, he shared information about his background, his writing, and about aspects of the profession that evoke his unbridled enthusiasm – among them both activism and teaching. When we asked for a picture to go with the interview, Michael sent concrete evidence of his manifold interests. As a reminder of the provocative ideas that he has authored, the picture shows Michael puzzling out the theoretical structure of 'public sociology', with the help of an archetypical teaching tool, the blackboard.

Devorah Kalekin-Fishman

DKF: My excuse for interviewing you for *ISRB* is my conviction that sociologists are interested not only in what other sociologists write, but in how they do it. This, of course, has to do with background, studies, and research. So could you begin by telling our readers something about your background?

MB: Well, I'm an English boy, born in Manchester, to Russian-Jewish parents. They had migrated to Germany in the first two decades of the twentieth century, but from different places - one from Latvia and the other from the Ukraine. They met in Leipzig (Germany) where they were studying for their PhDs in Chemistry. They saw Hitler's writing on the wall – as Russian Jews they were less likely to be deceived – and they migrated to England in 1933, just in time to avoid the Holocaust. They moved to Manchester where my father became a lecturer in Chemistry at the College of Science and Technology as it was then, and my mother stayed at home. Like good Jewish parents, they obsessed about my education, and I was fortunate in that.

My father died when I was eleven, and I think I must have developed a subterranean interest in things sociological when my mother, having little money, had to take in lodgers – into a small semi-detached house, Those lodgers came from all over the world and my mother proved to be a very hospitable hostess. This was quite an education in itself during my teen years but I don't think I was aware of it. From early on, it was clear I had no talent for the arts, for writing, for languages or history. It seemed obvious that I would pursue science and mathematics. I really wanted to be an astrophysicist. I found that a fascinating area even when I was in high school. But to do astrophysic**s,** I needed to have a mathematics degree. So that's what I did when I went to Cambridge. I was neither good nor interested in mathematics. This was 1965 to 1968 an exciting time to be growing up. For me it was the era of student movements in Europe, but also in Latin America, and in Asia. Like many others I saw students as a transformative force, but whereto I had no idea.

When I was 17, between school and the university, I had gone to New York. And this was a most exciting time for me. Six months there transformed my life. This was at the beginning of the civil rights movement, the anti-Vietnam War movement. At 17, New York completely overwhelmed me. I don't think I've ever recovered since. Then I went to boring Cambridge to specialize in boring mathematics. I'd lost interest before I had begun.

DKF: Let me just see if I'm following. In England, did you go to ordinary schools before you went to Cambridge?

MB: I went to what was called a 'direct grant' school which is a sort of mixture of public and private; essentially it recruits students on the basis of merit from a huge catchment area in northwest England. I was not such a good student. My father spent a lot of time trying to teach me mathematics and I had all sorts of special tutors to improve my terrible English language skills. Once I got to this special grammar school, the only thing I was any good at was mathematics, which was what I did.

While I was at Cambridge doing maths I became interested in economics. There was no sociology since, in those days, Cambridge dons were quite

opposed to this new-fangled discipline, or at least it was new to England in the 1960s. There was 'social administration', the study of the welfare state, especially at the London School of Economics, but sociology was still underdeveloped, and in Cambridge, in particular, it didn't exist. So I continued with my hated maths, but every summer I would travel to different parts of the world. The first summer I hitch-hiked through Africa; the second summer I went to India, and then the third summer I returned to South Africa after graduating and there I became a journalist for six months. Restless as ever, I left South Africa to go to Zambia. This was in 1968, four years after Zambia's independence. It was a very exciting time to be there. Short of money, I began working on the Copper Belt, in the copper industry. On the advice of sociologists at the University of Zambia I tried to understand the impact of independence on the transformation of social relations in the copper industry. After two years in the copper industry, I was admitted to what was then the new University of Zambia to do an MA in social anthropology.

DKF: The road to social science began with your mother's lodgers and led on to the US, to student movements, and ultimately ended in Zambia in sociology, which is obviously your calling. But was it obvious to you throughout?

MB: Well, I actually had an intimation. One of the last lodgers, before I left England for Africa in 1968, was Moshe Shokeid whom you might know, as an anthropologist at Tel-Aviv University. He was studying under the great anthropologist, Max Gluckman. I remember bringing home Parsons' *Social Systems,* and Merton's, *Social Theory and Social Structure.* And he started laughing at me because I was beginning to read all this abstruse sociology from scratch by myself. One of the appeals of sociology, it seemed to me, was the great scope it gave for teaching. I had always wanted to be a teacher in the university and if it wasn't going to be astrophysics then sociology offered the opportunity of engaging directly with the lives of students. But, I suppose the real thing was that sociology was somehow resonant with the times, with the public issues I was interested in.

DKF: Well, when you went to Zambia, you didn't go as a journalist, right?

MB: Right. In my previous travels when I hitch-hiked through Africa, from South Africa to the North, I had stayed with one of the executives of Anglo-American, one of the biggest mining corporations in Africa. So I called on him. I said, 'I'm looking for a job,' and I told him roughly what I was interested in. He told me of two possibilities. Fortunately they were then setting up what was called a 'personnel research unit', a relatively new social research unit. And it was there that I embarked on a sociological study of the mining industry. I was able to observe at first hand, the relations among copper industry management, the trade unions, the political party and the state. And that was quite an eye-opener for me. I was very lucky to have access to these high level negotiations taking place in a post-colonial context. At the same time I was very interested in what was going on underground in the different work places and on the surface in the different processing units. So I was able to create a picture of the industry as a whole from the standpoint of the changing relationships between class and race.

To what extent had the color bar –that had defined the colonial order within the mining industry– shifted? To what extent had it been eliminated? After all, the racial color bar was supposed to disappear after colonialism. But I discovered that it continued; the color bar floats, it floats upward, but it doesn't disappear. And I was interested in the mechanisms within the industry that reproduced the color bar, and the sources beyond the industry that allowed this to happen- the reproduction of the racial order in a post-colonial society.

DKF: Did you feel that you were thinking in a different way, now that you were applying your reading in sociology – by contrast with the way you thought as a mathematician?

MB: That is an interesting question. Actually when I teach social theory, I teach it as a deductive system. Marx and Durkheim have a very deductive way of thinking. Even Weber can be presented in a logical fashion, how Calvinism, for example, gives rise to the Spirit of Capitalism and how this is one element of modern rational capitalism. That's on the one hand, but on the other hand, I have always been an ethnographer, so I participate in the messy life of others. This gives rise to a rich complex set of data that cannot be reduced to logical connections. So there is always a tension in my empirical work between the way I think theoretically and the way I actually undertake my investigations … I like to think it's a productive tension; I try to marry the ethnography with a broader theoretical framework and in that way rebuild theory. I think of empirical investigation as rebuilding, improving, reconstructing pre-existing theory. In my own case I'm interested in reconstructing Marxist theory on the basis of the ethnographic work that I do.

DKF: In your article on the extended case method, you say that you are interested in pushing theory forward and not 'merely' in making it more complex. What does that mean?

MB: Well, I guess there are two sides to social science, to sociology. There are people who say the world is incredibly complex, and, therefore, our theories have to map that complexity. On the other side, there are those who say – and I belong in this group -- the world is incredibly complex, and therefore the function of the sociologist is actually to simplify. Here the idea is not to mirror the complexity but to try to see its underlying pattern, its essence. So one shouldn't 'reconstruct theory' absorbing anomalies or contradictions, by just making theories more complex. That's easy. I think that one has to reconstruct theories in ways that preserve or increase their elegance, their parsimony. And here lies the art of reconstruction. One can always add on saving lemmas, auxiliary hypotheses, specify special conditions for the application of a theory, all to counter refutations, but that simply makes it more complex and cumbersome. I don't think that is good theory.

But the most general point is that we don't start from scratch when we enter the field. Many ethnographers believe you have to rid yourself of the theory you have in your head, and enter the field *tabula rasa* to see the world as it truly is. I don't believe that's possible and I don't even think it's worth striving for. Rather than emptying one's head of preconceptions it is better to clarify what

they are. Therefore, one enters the field armed with theoretical preconceptions that are necessary to be able to see anything in the field. That's the way we are able to recognize surprises which are the basis of reconstructing our theoretical frameworks. Fieldwork is like a series of experiments – making predictions, discovering falsifications, rebuilding theory, leading to new predictions and so forth.

DKF: …. And seeking to preserve the parsimony, the elegance and the clarity.

MB: Yeah – as far as possible, yeah.

DKF: How about changes in you as a sociologist and as a person? This going into the field and constantly checking the interventions you made yesterday; checking what you've seen and done and then going ahead to make new ones. That sounds to me like living.

MB: Yes, it is. It's living made conscious. I write field notes. You have to write field notes when you are an ethnographer. The act of writing forces you to reflect and provides the data one analyzes. I have two parts to my field notes. One part is describing what happened, what I saw and what I heard. The second part is analyzing that in the light of the emerging theory that I'm using to interpret what I observe. The two parts of the field notes are in dialectical interaction! Field notes are a dialogue, you might say, between theory and data, a continually evolving process through the research. So in a sense, the design of the research – who one talks to, what the comparative cases are, etc. – is an on-going process, conducted in close connection to theoretical reconstruction. It's not a matter of 'collecting the date, coding, then producing the theory'. It is a continual interactive relationship between engaging with the world, gathering your data, theorizing, and then going back into the field, gathering more data and retheorizing. As you say, we are simply making more self-conscious what we do in daily life.

DKF: What you have said is not entirely clear to me. Do you go into the field choosing one of the theories that you know and take it from there, or do you observe and then the appropriate theory 'jumps into your mind'?

MB: Yes, I go into the field with preconceptions. I think we all do. I try to make them explicit. So in my case I'm working with a Marxist theory; and one of the problems of Marxist theory in the 1970s was to understand working class consciousness. People were only rarely studying the lived experience of workers directly. So I became a worker myself in order to understand working class consciousness. Specifically, I wanted to understand how it was that workers produce what Marxists call 'surplus labor.' How do they produce more than the value of their wage? Or to put it more bluntly: 'Why do workers work as hard as they do?' Previous literature in industrial sociology had always asked, 'why don't workers work harder?' Or as the literature put it, 'Why do workers restrict output?' I posed the opposite question, not just as a theoretical matter but because I was genuinely astonished at the pace of work that workers voluntarily maintained. My answer evolved as I worked in an industrial plant in South Chicago for over a year (1974-75). I could tell a similar story about work games and incentive systems for Hungary and Russia.

So, yes, I start with a theoretical framework that poses a question. Now, it's quite possible that you drop the question or you drop the theory because, for one reason or another, it doesn't make any sense. Other questions may appear. That can happen. For example, I'm always working with quite a few graduate students who do ethnographic work, and I teach a course on participant observation. I watch how students' preconceptions are often rapidly discredited, which leads them to search for alternative theoretical frameworks to make sense of the site they're studying. In my case, I come in with a broad Marxist framework, but there are few students today who are committed to such a grand schema. They are more likely to work with middle range theories, from which they pick and choose.

DKF: Let me ask you about yourself as a person. You've spoken about how things change as you do ethnography. Do you think that doing ethnography in the way you do it has changed you? Or does that question mean anything to you?

MB: Of course! How can one not question who one is when one is an ethnographer. If nothing else, ethnography is a dialogue between self and other, a dialogue in which one becomes self-conscious about who one is. I have been conducting a conversation with myself and the way I think about the world throughout my sociological career, starting in Zambia in 1968, moving to Chicago in the 1970s, Hungary in the 1980s, Russia in the 1990s. I stopped working in factories in 1991 because the factories in Russia were closing down. So after 1991, I became much more interested in the survival strategies of the workers that I had been working with in the new economic dispensation of post-Soviet Russia.

Over time, my ethnographic ….. what's the word …. my ethnographic capacities have diminished. It became much more difficult for me to be an ethnographer the older I got. Not only because my age distanced me from younger people and I tended to mix more and more with the old guys on the shop floor. But also because I think that as I got older, my ethnographic taste buds became weaker. My memory is weaker; I see things less clearly; it's more difficult to write notes. The whole business is so tiring. It's tiring just to stand on your feet for eight hours – at least for an intellectual! There are many ways in which growing older has shaped my retreat from ethnography. I retreated into social theory. So I spend more time now thinking and teaching theory. But I treat theory as an ethnographic project. As you know I'm very interested in Bourdieu. So for the last five years, I've been in a very specific field site – the texts of Pierre Bourdieu – and trying to put them together, putting them into dialogue with Marxist theory. I have a similar relationship with Bourdieu's texts as I had with the factory. I entered with a preconception and as I read more and more the preconceptions deepened and changed. I came across all sorts of unexpected findings and experiences as I read this man's amazing corpus. Like fieldwork, it is a conversation that continues through time and has far from ended. Obviously, it's easier for me to dwell in Bourdieu's texts than to work in factories. So I take an ethnographic approach to social theory.

DKF: That is fascinating – do you think that other people experience this as well?

MB: You mean the declining taste for fieldwork? Yes, I think so. But my case is perhaps rather unusual. Well I have never been a great field-worker. I've supervised many dissertations based on ethnography, so I know that many of my students are far superior field workers. I was drawn to ethnography because I thought that it was so important to engage directly with the people I was studying – engage them in their space and their time. My first teacher in Zambia, the social anthropologist Jaapan van Velsen, scolded me when I showed him the survey I was planning to conduct on the Copperbelt, 'You can't understand anything with a survey. It's a waste of time.' He probably didn't say exactly that, but he did shape my approach to sociology. I like to think I'm far more nuanced in my understanding of surveys now; I've studied what the best survey researchers do – people like Howard Schuman who are brilliant ethnographers in the way they approach the design of questions and interview schedules. But Jaap was an anthropologist and as far as he was concerned there was only one way to do research and that is through participant observation. He was such a fierce man that I dared not believe anything different.

DKF: And, from what you say, it has colored the way you look at the world altogether – as you describe your relationship to theory. How about the practicalities - how have you done participant observation research? I suppose you get a job and you get paid.

MB: Well, that's a very tricky thing. Let's take my Hungarian experience as an example. It was difficult to get jobs in socialist Hungary in the 1980s, especially for a foreign sociologist. No Hungarian sociologist would dream of doing something so crazy. The only other person who had ever done anything like this was the Hungarian dissident Miklos Haraszti, who had been forced to work in a factory by the state – and he then wrote a brilliant book, *A Worker in a Workers' State*. I was no dissident but a foreign – 'American' no less – academic. To get a job involved a long chain of reciprocal connections and networks. It was a fascinating sociological project in itself, working through the labyrinths of the party state. But once I got in – for example in the case of my dream job as a furnace man in the Lenin Steel Works – management was terrified that I would get killed. It was ok if a Hungarian worker got killed, but, a US professor killed in the Lenin Steel Works might create an international scandal. So they insisted that I have a 'nurse' in permanent attendance who would make sure I didn't do anything stupid. Of course, this was a dangerous place – someone could pour molten steel on me and I'd be burnt alive. So they wanted somebody to shepherd me around the whole time. Anyway, that didn't come to fruition, but they did insist on my being on day shift all the time. And then I said, 'Look, I can't be on day shift all the time. I've got to be with the same brigade that rotates shifts.' So eventually I got my way.

But the point is this, they started paying me when I was on the day shift, and they paid me more money than anybody else on day shift. This was because they thought: here we have a bloody American professor. He must get more money. It wasn't a lot more money, but it was somewhat more than what was paid to my co-workers who had been working there for many years. I remember the first

time we got our pay checks. We all were sharing our pay slips that indicated how much we earned. They were furious: 'Look how much you earn, Misi, – more than we did, and we've been here thirty years; and you just came three weeks ago.' That was most embarrassing, even humiliating, and could have permanently ruined my relations with my fellow workers. I had to quickly put that right so that I earned the same as anyone else who would begin work in the Lenin Steel Works.

DKF: Did you ever have to apply for funding like the sociological proletariat?

MB: When I was doing my Hungarian and Russian research, I got funding. I would work in the summers,, and take semesters off. I received funding from the (US) National Science Foundation and various foundations that supported work in Eastern Europe. I didn't really need much money as it was quite cheap then to live as a worker in socialist Hungary and the Soviet Union, and I tried to live just like my fellow workers. Still, as long as I presented my research in terms relevant to the advance of sociology I generally received funding. There has been a lot of talk about US foundations not being friendly to qualitative sociology, but I never experienced that. Of course, studying the 'market transition' was, for a time, at the cutting edge of research and I had a track record of factory ethnography.

DKF: You remember that some people at the National Associations' Conference in Taiwan complained about their (academic / scientific) dependency on foundations – because of the need for funding. You didn't find that?

MB: Of course, I'm talking about a specific time in a specific place. In the 1990s there was more money in the social sciences than today and I work in a privileged university in the US. Things are changing dramatically both here in the US and elsewhere. Universities are in crisis in most parts of the world, which was something we discussed in Taiwan. In many places in Africa and the Middle East the university as a research entity is disappearing, replaced by all manner of think-tanks, NGOs, consultancy firms. In other places public funds are being withdrawn as is happening to my own University of California where student fees have jumped by leaps and bounds, where the administration goes in search of donors or corporate sponsors. Universities are moving from being a public good to a private good. In other places economic pressures are supplemented by regulatory pressures, forcing universities to compete in the world arena, trying to establish their place in some world ranking. States might funnel funding to one or two high prestige universities, leaving the rest to fend for themselves. The polarization within national university systems but also between them becomes deeper and deeper. There is an intensified flow of students from periphery to center, especially to English-speaking countries. As sociologists we have to be very attentive to the different ways this is working itself out in different parts of the world. I created a blog at the ISA website which deals with this very issue. It's called Universities in Crisis [http://www.isa-sociology.org/universities-in-crisis/]. The situation today is very different from what it was when I was doing my research in the 1980s. We may have talked about some

sort of economic crisis then, but it was nothing like what we are facing today. In the academic world things are changing very rapidly.

DKF: and never for the better…

MB: I don't see much of a silver lining here. We have to rethink the meaning of the university rather than trying to restore the university of yesteryear – that university has gone forever. Thus, I have become an ethnographer of my own workplace.

DKF: You said something before about writing field notes while doing ethnography – describing what you are doing and what you are discovering about the emerging theory. Is that all you want to say about working as an ethnographer?

MB: Of course, the most important thing for an ethnographer to do is to write field notes. There's no point in doing ethnography if you don't write field notes. And if you think you can write the field notes three days later, you're mistaken. You've got to write them immediately after the field experience, everyday. It requires great discipline. It was especially difficult for me because I was working, at least eight hours a day. I was pretty tired, and then of course, there was travel to and from work, so it could be as long as ten hours. The pressure did vary from site to site. I always sought to integrate myself into the working class community. In Chicago it was pretty difficult. Everybody just scattered at the end of the shift. But in Hungary, we all went drinking. Drinking takes time, but it also had consequences for my mental capacities, and powers of concentration. So the more successful my fieldwork was, the more time it took, and the less time I had for field notes but also the less mentally and physically equipped I was to write them. It was a strange situation to be in. But yes, to return to your question, everyday you have to find time to write those bloody field notes. Since I read so many people's field notes, I'm only too aware of the extraordinary variation in the quality of people's field notes. Some people are just brilliant at writing field notes, but they may not be so good at theorizing them. Other people are brilliant at theorizing but terrible at writing field notes. There are multiple talents involved in doing ethnography. Social skills are crucial. Some people are just born ethnographers. They walk into a situation and an hour later, they've figured it all out. Other people can walk into a situation and a year later they haven't figured out what's going on. It is a special talent, and I've known very shy students who genuinely come alive in the field, just as I've known extraverted students unable to cope with fieldwork.

DKF: You believe the 'participant' part of participant observation complicates things, don't you?

MB: Yes, you have to address all sorts of ethical issues. From the very beginning you have to decide whether you are going to declare yourself as an observer, scientist or whether you try to enter incognito. I know there are many people who say that on principle you must always be 'overt', you must always declare yourself. And the 'rules for the protection of human subjects' require that you obtain the consent of the people you study. That is a reasonable rule, but what are the implications? It would mean that my research in Zambia, studying the managers of Anglo-American Corporation and how they were handling the post-colonial

transition would now be impossible. The powerful can protect themselves. If they don't want you there, they can easily remove you. If I had declared my interest in the processes of Zambianization, in localization, in the transformation of the color bar; my employers would have got rid of me. As it turned out, they were totally shocked when I told them what I was doing. It would have been an impossible project if I'd been open about my research; whereas, of course, the poor, the dominated, the homeless are very vulnerable. They can't resist us. In fact, they sometimes embrace us as companions. They have much less to fear. So we have access to them. The result is that sociology becomes very biased in that the rich and the powerful are rarely studied or studied in an innocuous superficial way. In general, participant observation raises many such important ethical questions – questions that are true for all methodologies, for all sciences of society but which most methodologies avoid. The fact is that we are part of the world we study, and participant observation reminds us of this. It reminds us that even if we do our research from the protected haven of the university or research institute, we are still part of society, and indeed as boundaries break down we become ever more aware that we are part of society. Participant observation expresses in acute form the ethical dilemmas of all social science.

DKF: When does the actual writing of articles and books begin?

MB: In the case of Zambia, the report evolved over the four years I worked and studied there. Generally, I will try to write a paper in the middle of my fieldwork, so that I can assess where I am at, and thus think through where I want to go in the remainder of the fieldwork. I'm not of the belief that you have to wait until the end of the fieldwork and then analyze the data. No, it's an on-going process. Thus, my fieldwork in Russia went on for about ten years, repeatedly interrupted. I was writing papers in a situation that was itself evolving, an exhilarating but exacting project. So I would write a paper and perhaps even publish it, but when I returned to the field the world was completely transformed – the enterprise, for example, had disappeared. In that way it was an interesting project. You can go into the field and quickly see things clearly, and you immediately want to write a paper. If you don't write a paper then and there you might never write one because things become so complex. I never produced a book on Russia, although I did ten years of fieldwork. Things were evolving so fast that I could not effectively keep up, so all I could do was write a series of papers but never a book.

DKF: Maybe it's something that you could do in another year or two.

MB: No, it's lost, it's lost. It was the 1990s. Another era. I just didn't really grasp what was happening. I was not in sync with the field. Somehow it was very different from my experiences in Zambia, Chicago or even Hungary where my interpretations have stuck with me for a long time. Indeed, one way or another, I've been rewriting my research in Zambia and in Chicago my whole life. I've thought a lot about the relation between time of history and the time of research. I think ethnography is all about 'revisits', just as life is all about revisits, as you said earlier. Every day in the field is a revisit to the experiences and reflections of the previous day. My revisit to my field site in Syktyvkar in Northern Russia

took place on a yearly basis. And I revisit the interpretations of *Manufacturing Consent* (1979) in the light of the subsequent development of capitalism. I returned to the site of my Chicago research thirty years later (2004) to discover that the plant was no more. In those three decades the whole of South Chicago had suffered deindustrialization, the urban area had become a vast sprawling ghetto, occupied largely by African-Americans expelled from the housing projects near the center of Chicago. It forced me to think why in 1974-75 I had never anticipated such a transformation. What was wrong with the theory I had developed to understand the factory in its wider context? I realized how I had missed the global transformation of capitalism and, indeed, the changing relation of the US state to labor. Equally, I'm still thinking about the research I did in Russia in the 1990s for its significance for what came after socialism. What does it mean for the (re)understanding of the Soviet Union. I don't feel I leave any of my projects behind. I continue thinking and rethinking – their meaning at the time, their meaning now, and how things have changed subsequently, and how all this interacts with the way my own thinking has evolved.

One lesson I draw from these reflections is the importance of making predictions. Sociologists should stick their necks out. It's fine to be wrong if it forces you to think why you were wrong. Why was I wrong in thinking that the industrial relations that I described in 1974-1975 in Chicago would endure? Why was I wrong in thinking that after 1989, Hungary could make moves toward a democratic socialism rather than toward a market capitalism? Why was I wrong in thinking that Russia would go into an irreversible decline which I called 'involution'? In fact, in 1998, Russia made a turnaround. So wrong predictions force you to recognize the shortcomings of the theory you are working with, and thus reconstruct that theory. It's good to be wrong – as long as you are also right a lot of the time.

DKF: It's quite a luxury to be able to think that it's good to be wrong, isn't it?

MB: You're absolutely right. But that is the nature of being in the academy and being a scientist! You know, Obama cannot be wrong about Egypt.

DKF: Let's talk a bit about writing. You collaborate a great deal with students. Do you collaborate only with students, or do you collaborate with colleagues too?

MB: Well, I have completed two books with students. They were an enormous amount of work, but at the same time most exciting. In these endeavors each student has his or her own project as well as participating in the collective project. In *Ethnography Unbound* there were 11 of us – me plus 10 PhD students early on in their careers who had been in my participant observation seminar. Each did their own project but at the same time each was engaged in the work of all the others – they developed their own ethnographic talents through the relations they developed with all the others. Together we developed the ideas of the 'extended case method.' I did something similar with a group of more senior graduate students whose dissertations I was supervising. At the time I was chair of my department, and therefore couldn't do any fieldwork of my own. The students were planning ethnographies in different parts of the world, and so I proposed we write a book together called *Global Ethnography*.

I had no idea what global ethnography was. We would figure this out together. These collective projects are both exhilarating and exhausting – I was the whip that forced people to deliver draft after draft of their chapters. I have never written papers with students, but rather guided them in the writing of their own papers. At Berkeley we tend to encourage students to develop their own independent projects rather than see them as extensions of our own work.

As to colleagues, I've written one or two papers with my good friend Erik Wright. We might get into an argument about something and then decide to write a paper together. Well, not exactly together. Since he's so brilliant, his mind so clear and his writing so quick, I let him do all the work, and I just lean over his shoulder to make sure he's properly representing my point of view. I've also collaborated with my research partners in Hungary and Russia – Janos Lukacs and Pavel Krotov. We did the research together – I would be working on the shop floor while they interviewed managers. We would come together to pool our knowledge. Writing together was not always easy as we came to the material with very different perspectives. The academic world does not encourage such intense collaborations, and they can be very fraught, but they are often indispensable. I know that without them I wouldn't have been able to make a lot of sense out of what I was observing.

DKF: It sounds as if you enjoyed that more than writing by yourself. Is that the message?

MB: Well, if truth be known, I much prefer to write by myself. Writing is an internal conversation with oneself and others, when two people are involved it gets very complicated as the silent interlocutors are very different. Still, collaboration has been essential, if only as a corrective to how I see the world. It's like fieldwork you have to do it, no matter how difficult, how uncomfortable, how humiliating. Anxiety is part of the package – the more anxious the better the results.

DKF: These sound like seasoned conclusions. What kind of advice would you give to novice sociologists?

MB: I think we first have to say that novice sociologists face very different challenges in different parts of the world. If I am to generalize, what we do know is that people do not go into sociology for economic rewards or political power! Although it is true there are sociologists, such as Fernando Henrique Cardoso, who have made it into the upper reaches of the power elite, this is not common. We enter sociology out of a vocation, as Weber would say, with passion and discipline, with the courage of our convictions. But what sort of vocation is this? It's a double-vocation as scientist and critic of the world. Sociology investigates the world in a disciplined manner but also wrestles with the question of how the world could be different. It deploys research to understand the limits and possibilities in the present. In this day and age of market fundamentalism it requires deep conviction to sustain sociology's critical traditions.

To get back to the novices, in many parts of the world, sociology is controlled by a male gerontocracy that stifles youthful imagination. It is important, therefore, to create communities of young sociologists and to connect them across the world. That is one of my projects for the ISA: connecting teams of young

sociologists from different countries. Wherever I go, I try to spend time with young sociologists, encourage them to participate in our new newsletter, *Global Dialogue* as writers and translators. With their energy and enthusiasm *Global Dialogue* now appears in eight languages in electronic format. [http://www.isa-sociology.org/global-dialogue]

I've also been developing an experimental course with undergraduate students at Berkeley, called *Global Sociology, Live!* We read the papers of distinguished sociologists from around the planet and then we enter into a conversation with them through teleconferencing or Skype, broadcasting the result to anyone who wants to watch. You can see the results at http://www.isa-sociology.org/global-sociology-live/. I'm hoping that young sociologists will be able to work together to extend this way beyond Berkeley. Of course, the ISA already has a wonderful program for graduate students writing their dissertations – the PhD Laboratory – and there is also a global network of junior sociologists.

DKF: Younger sociologists are consumed by all sorts of insecurities, so perhaps your idea of creating teams and networks is the way to tackle some of the issues they face early on in their careers. They are and will be responsible for the future of sociology. What do you think about whether or not sociology will change?

MB: I suppose the answer to your question depends on how one views sociology and the challenges it faces. I understand sociology as a scientific discipline that is rooted in civil society – institutions, organizations, movements outside state and market – just as economics is rooted in the market and political science in the state. I believe we are living in a world in which the collusion of state and market is either destroying civil society *altogether* or destroying its autonomy, and thus the basis of sociology. However, defending civil society is not only defending sociology, it's also defending *humanity* against political tyranny and market despotism. The interests of sociologists coincide with that of the human race! In recent days we have been glued to our televisions, watching the unfolding struggles of civil society against the Egyptian state. If you look at the history of the last twenty years in Egypt it is a story not only of the state, but also of the economy. Egypt was the recipient of foreign aid so long as it went along with structural adjustment – policies that led to the dramatic polarization of wealth and poverty. It was the collusion of state and market that suppressed civil society, which now has so unexpectedly sprung back to life.

But sociologists have a place in these struggles -- on the side of civil society against the unregulated and destructive tendencies of state and market. Moreover, as markets and states become ever more deeply enmeshed in global projects, so they can be contested only from the standpoint of a global civil society. That's why I'm committed to the project of the ISA. We have to think about enjoining sociologists young and old across national boundaries, studying how capitalism is transforming the world in different ways in different places, and too often at the expense of an autonomous civil society. One can be inspired by the way civil society can spring into action in unexpected ways, in unexpected places. Latin America, for example, provides a fascinating story of an effervescent civil society inspiring an effervescent sociology. My hope,

therefore, is to bring Latin America much more into the ISA. We can learn so much from our Latin American colleagues, about the diverse forms of local participatory democracy that have spread through the continent. They have their own language, Spanish, so they really don't need an association in which English prevails, but somehow we have to transcend such barriers. That's why I'm delighted that the World Forum of Sociology will be in Buenos Aires. It will be a great opportunity for all of us to learn about Latin American sociology.

We have a lot to fight for beyond the defense of our discipline and our universities. If sociology is to survive it cannot retreat, it has to advance into society. We have to take heart from the struggles of Tunisian, Egyptian, and Libyan people that have so caught the imagination of oppressed people across the world; we have to understand this through the lens of a global sociology. As I speak to you "Cairos" are spreading across our planet.

DKF: Do you think it's a turning point in the nature of the discipline itself?

MB: Yes, I think it definitely is. First, with the erosion of university autonomy, sociology can no longer pretend it is outside the society it studies. It will have to grapple with its place in the world, recognize its distinctive standpoint, and become ever more reflexive – shaped by, at the same time as shaping, the worlds it examines. Second, that reflexivity will be turned increasingly toward the global, but not as some would say by abandoning national containers, nation-states, national sociologies, but to the contrary, by recognizing just how our perspectives, wherever we are, are irredeemably forged in a national context. To leap straight into the global, and worse yet, into the universal, is to be guilty of the worst form of provincialism. We will reach for the global only via a dialogue that is premised on the local, the national and regional. But it will also have to be a dialogue that recognizes and struggles with the very real inequalities we find in our midst, inequalities that are themselves a reflection of the deepening inequalities in the world we study. That is why the next ISA Congress (2014) in Yokohama will have as its theme 'Facing an Unequal World: Challenges for a Global Sociology' – challenging an unequal world with an unequal sociology.