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# Diversity and Worth. A Retrospective Account

## David Stark in Conversation with Filippo Barbera

by Filippo Barbera *and* David Stark

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### 1. Conversation

**Filippo Barbera:** Let me start from the beginning. You got your Ph.D. at Harvard in 1982. Who was your supervisor? And what was the topic of your PhD Thesis?

**David Stark:** I had two advisors. The first was Theda Skocpol. She was just finishing her PhD when I arrived with my incoming cohort at Harvard, and so we did overlap as graduate students for a year or so. Theda wrote an outstanding dissertation that became a great book: *States and Social Revolutions* [1979]. I was a Teaching Fellow in the undergraduate course she taught on that subject while she was turning the dissertation into the book. That was a wonderful opportunity for me. Theda left Harvard to go to the University of Chicago. She did come back to Harvard, but by that time I had finished my thesis. I already had been working with Sandro Pizzorno, and he became my dissertation advisor.

The actual topic of my thesis was not the one I intended to do. I intended to write about “peasant-workers” in what was then Yugoslavia. I was so naïve when I left Cambridge to go to Zagreb. I didn’t really grasp that Yugoslavia at that time was a police state. On top of that, I arrived just as Marshall Tito, the dictator of Yugoslavia, started to die. It was the worst possible moment to be an American sociologist trying to study a good but sensitive topic and I never did get to do the research despite being in Yugoslavia for nine months trying to do it. So, I came back to Harvard, and

began to learn Polish. But then martial law was declared in Poland and that meant the end of my Polish studies.

The disappointing result for me – because I had so wanted to do ethnography – was that I ended up doing a library dissertation. It was on the relationship between Taylorism and Leninism. The conventional take on that starts with Lenin’s fascination with Taylor. That’s true, but as an explanation it neglects an important difference in levels of analysis. My take was that Taylorism was impossible in the Soviet Union because Leninism is an attempt to move Taylorist principles from the factory level to the level of an entire nation. Attempting “scientific management” of an entire national economy makes scientific management impossible on the Soviet shop floor. That was the argument of my thesis which was itself a combination of essays. For example, my early article, “Class Struggle and the Transformation of the Labor Process: A Relational Approach,” [1980] was a chapter of the dissertation, and another paper “Planning, Politics, and Shop-floor Power: Hidden Forms of Bargaining” [1982], written with Charles Sabel, was a chapter as well.

**FB:** Let me elaborate a bit on these points. Both the topic and the analytical perspective of your thesis seems to me quite far from Pizzorno’s.

**DS:** I got to know Pizzorno well only at the point that I was writing the last part of my thesis. But although he didn’t shape the choice of topic, he nonetheless had a big impact on my thinking and my approach to working. I remember very well the last year or so when I was writing the thesis. Typically, we would talk while taking long walks together. Sandro was so energetic. He never used the elevator (down or up, even though his office in William James Hall was six flights up) and he walked tremendously fast. He has a very quick mind. There I was trying to keep up with his thinking while I could hardly keep up with his rapid pace. Pizzorno was such an important figure for me. He never tried to push me on a particular line, he was very open to new ideas, and he always had smart questions trying to get inside what I was doing. The most important influence of Pizzorno was how to be independent – that it was not necessary to be part of a school. He was himself a very-hard-to-categorize person: he is not part of a school and he did not himself create a school, but he is widely read across disciplines and camps.

**FB:** Once Peter Bearman told me that the true Columbia spirit is not being part of a school, but to solve scientific problems! Do you agree?

**DS:** Yes, completely! One of the most interesting things about the Columbia School is that we do not fit easily into the academic categories. Almost every other sociology department, regardless of the theoretical orientation, is divided into camps along qualitative and quantitative methods. That’s not possible at Columbia. Both Peter and I, for example, do quantitative and qualitative work. Furthermore, we are

all of us, each of us in our own ways, “relational.” And, although we were not directly trained by Robert Merton, we share a strong emphasis on middle-range theory. We have nobody who is “just” a theorist. Yet, at the same time, everybody is doing theory. Our work in Columbia is very theoretical, but it is not Theory, as in theory at the level of society. We are striving for theory appropriate to a particular problem or specific research question that is being asked. And always working out theoretical ideas with empirical material. It is the Columbia style, rather than the Columbia School.

**FB:** Looking at your scientific work, two main periods can be identified: i) from the 1980s until the 2000s, and ii) from the first half of the 2000s onwards. In the first period you were strongly focused on the East European countries and on the transition from socialism to capitalism. In the second period, new topics emerge such as finance, organizational innovation, and creativity. One of your first papers was “Class Struggle and the Transformation of the Labor Process” [1980]. One of your most recent papers, “Game Changer” [*AJS*, 2015], is about creativity in the video games industry. Is there a continuity or divide between the two periods? Between, so to say, the post-socialist path in Hungary and the analysis of video-games?

**DS:** You are right about the periods. There is a period until roughly 2000 when I moved to Columbia and started to actually do research in New York City: the new media start-up, finance, innovation. So there is a divide in terms of research topics, sites, and empirical problems. But across those two periods there is a strong continuity, I think, in terms of the analytical questions: I have always been interested in diversity. I explicitly became interested in diversity after 1989, in light of the problem of the transformation from state socialism in Eastern Europe. But if I look back to the dissertation topic – the one that I wanted to write but I did not write – there I was interested in the peasant-worker because I was interested in people living in more than one form of social organization at the same time. Peasant-workers in Yugoslavia would cultivate the land for the market or quasi-market *and* have jobs in socialist industry. They moved from one mode of production to another on a daily basis!

Then I began to think about that as a more typical situation: the social world is a place where we can move from one set of principles to another and there is a multiplicity of worlds that we inhabit. And this is the regular condition of living in modern societies, diversity of principles, diversity of organizational principles, diversity of evaluation. But remember, this is the dissertation I did not do. From there I did fieldwork in Hungary in the early 1980s, moving from Paris where I was working with Pierre Bourdieu and Luc Boltanski. I took the Orient Express from Paris to Budapest, where a friend who met me at the train station told me that something new was happening in Hungary. Workers (about 10% of the labour force) were al-

lowed, in the same workplace, to work part of the day in a rigidly hierarchical way while working in another part of the day in an organizational form in which they themselves organized the means of production (in off hours and in the weekends). As they used to say:

*From 6 to 2 we work for them, but from 2 to 6 we work for ourselves.*

In the daytime you work in a bureaucratic place, while afterwards you work in self-managed one. So by the time I left the train station, I thought “I have to study this” and by the next week I started doing interviews with János Lukács. So my very first fieldwork in Hungary was about multiples ways of doing things, multiple principles of worth. After 1989, I got concerned about the topic of what was going on in Eastern Europe, where a transition was actually happening: from the monoculture of planning, to the monoculture of market. Then diversity is also the main analytical perspective of my *Sense of Dissonance* book [2009]

**FB:** This idea of diversity, of belonging to multiple circles, seems to me closely connected both to Georg Simmel, on the one hand, and to Harrison White, on the other.

**DS:** I actually didn’t read Simmel early on in my career and, in fact, I read Harrison White quite late as well. At Harvard I was a Skocpol-Pizzorno student and not a White student. That was a divide: it was actually Skocpol vs White, with Pizzorno not belonging to any camp. I was not really exposed to White’s ideas until I was at Cornell in the 1990s and then, first hand, when I moved to Columbia in 1997. But you’re right: I can trace a link with Simmel, but it is an indirect one. If I think about what was most influential, it is pragmatism, Dewey in particular. Marx and Weber of course were important, less so Durkheim. But my early exposure to the socialist police state shook off any uncritical reading of Marx.

Coming back to Harrison White, what I found interesting with his thought is the reversal of classical sociology problems. Classical sociology looks for the sources of order, White flips this over and asks “how do we get disorder and live with it?” Furthermore, whereas social sciences usually start with individuals as rock bottom points, instead Harrison asks: “How do we get an individual”? And he would say that we get an identity (meaning an individual as identity) only through the management of the differences we belong to. Identity is the ability to live with ambiguities in the social space of differences. That’s why Harrison White is the most outstanding postmodern American sociologist, even he would never accept that characterization. But he is!

**FB:** Would you define yourself as a “postmodern” sociologist?

**DS:** Not really. I am more shaped by the research I do, rather than being shaped by a specific theoretical perspective like being “postmodern” or anything else. It’s a kind of fidelity to the world: you have to be faithful to the observations that you are making in the world. That’s a constraint – a productive constraint.

**FB:** Let me elaborate a bit on this point. Today’s analysis of capitalism strongly relies on “transitology” (see Paul Mason post-capitalism), with a poor understanding of how social change actually take place. What can we learn from the transition to capitalism of the formerly socialist countries to make our analysis of contemporary capitalism sharper?

**DS:** In 1989 and immediately after, the dominant idea across the political spectrum was that the transition could be analyzed as a switch from socialism to capitalism, from plan to market, from public ownership to private property, from dictatorship to democracy. I was there and I saw people coming from the UK and USA with their recipes on how to move from socialism to capitalism in “six steps.” I had a different idea. My idea was to be sceptical of what I called “the science of the *not yet*.” That view is like looking into a crystal ball and seeing Eastern Europe becoming like Western Europe. More or less, it takes time but this is sure to be the future. The problem was that people were also looking at the present through the distorted lenses of the crystal ball. I hold a different position: put the crystal ball down and see what is happening right now. And what was happening was a combination of different principles and structures, both in politics and business. I then developed the concept of “recombinant property.” The idea is that social change is not replacement but recombination. I was criticized by people thinking that it was just a matter of time because these societies will join the EU and became no different from western European countries. But just look at how things are today: the most innovative political leader of former socialist countries is Viktor Orbán, the premier of Hungary, with his idea of “illiberal democracy.” A mix of populist right, nationalism and authoritarianism. Far from Eastern Europe being the follower, what we might say is that it is the model. We do not know what is going to happen with Donald Trump, but we know that modern capitalism does not need to move in only one direction: we do not know where capitalism is heading.

**FB:** Let me now focus on the second phase of your work. First of all, it is worth mentioning the deep connection with the French “conventions school,” especially with the work of Boltanski and Thevenot (*On Justification*, [2006]). May you explain how and why you become interested in their work?

**DS:** I was at my last year at Harvard and I met Luc Boltanski who was there at the Center for European Studies. I met Luc and I was immediately taken by his

passion and intelligence. We got on very well and he invited me to Paris, where I went the next summer. I was there at Bourdieu's institute in 1983. I was back again in 1986 when Luc formed the *Group sur la sociologie politique et morale*, and that was when he split with Bourdieu. I was in Paris in the fall of 1986, dividing my time between my fieldwork in Hungary and Boltanski and Thèvenot while they were writing *De la justification* [1991]. In those days I would regularly see Bourdieu and Boltanski, separately. They were inquiring about the health and well-being of the other, with great affection.

I then met many of the other scholars from the French conventionalist school, and through Luc I met Bruno Latour. I was there, in their group bringing back my field notes from Hungary and I was already using the idea of economies of worth, but also departing from the original idea in some ways. I was more interested in the tension among worlds and how this tension can be a source of creativity.

**FB:** It seems to me that French sociology, among the European national traditions, is the one with the strongest influence on American sociology. Is it true?

**DS:** It is interesting to speculate why that is so. It would not be surprising – from my Columbia sociology perspective – that a scholar interested in understanding the social world without being caught in a specific method or theory would be fascinated by the work of Bourdieu or Boltanski. They both never use the same method twice. What counts is the exposure to the social world and the way it works. This is a very American idea.

**FB:** Did you ever read Raymond Boudon's work, who in many ways stands opposite to Bourdieu but is also a combination of theory and method?

**DS:** That's a nice example. Even if they are very far from the theoretical standpoint, they are very close as a style. And I could add also Michel Crozier and Alain Touraine to the list.

**FB:** This makes clear that you have a strong connection with the EU sociology. May you underline the differences between American and European sociology?

**DS:** There is definitely a difference. Take a typical American and a typical European sociologist. They both look at the world through some kind of intellectual device, never presuming to see the world as such. The European sociologist will look at the world through theory, while the American will ask: "Is the theory fallible?" The European will answer: "Of course, every theory is fallible." And so the American will then push on: "But is the world fallible?" This is the point. Why don't start with the world? Because, as we said, we need a device to get the world out there. Instead of starting with a theory, why not start with a method? The world needs to be mediated. The world is a constraint but you need the right device and, as an American sociologist, I will start with method as the device. Both European and American so-

ciologist are of course equipped both with theory and method, but the emphasis and the writing style is different. The typical European work is less linear, the argument circles around. The typical American piece is usually clearer, not necessarily better, but surely more linear and clear. There is lot of stuff published in American journal which is corrupted by far too many citations, but at the top of the profession clarity is really important.

**FB:** Despite differences, both American and European sociology rely very much on a standard format of publication. And in both systems, scientific careers are more and more linked to the publication of paper in top journals. What do you think about this? Do you think journals need to innovate their format?

**DS:** If I was asked to propose the field, of all the possible ones, that has been the least innovative field in the last fifty years, my candidate would be the social science research paper: introduction, literature review, data, methods, findings, discussion, conclusions. Do it in thirty pages. This is the standard. There could be room for a journal trying to do something different. And that's why I was first attracted by *Sociologica*. The journal promotes debates and dialogues. Another possibility would be to include short pieces, or with materials other than words: photographs, pictures, drawings, animations. We have opportunities to present animations of network dynamics, for example, and this is very telling.

I think *Sociologica* from his founding has been doing something different. In my own work I also try to write differently. One of my early pieces "Privatization in Hungary: from Plan to Market or from Plan to Clan" [1990] takes the form of a debate: position and counter position about privatization. Both position and counter-position are convincing from a reader's viewpoint. There is a wonderful paper by Trevor Pinch [1988], actually "by Trevor Pinch and Trevor Pinch" about reflexivity in science and technology studies where he has a debate with himself! I have a paper in the economic journal, *Environment and Planning A*, which is called "Frequently Asked questions" [2009] made just of questions, without answers. Even the abstract is a question! In my teaching I have something which is called "silent lectures." It came about when once I was with my one of my undergraduate research assistant and during a break he asked me: "*Professor Stark, what's the most important thing you have learned about teaching?*" I thought a minute and answered, "*Knowing when to shut-up.*" The student replied: "*Then you should do silent lectures.*" I thought it was a great idea. You can find them on my website. They are self-running power points, each about seven minutes long. One is called "Performance," another is "Models" and the third "Demonstrations." Just images, without words. Then students start talking and debating about what they saw. Think about performances: musicians, dancers, and actors perform; and audiences applaud. Coaches and



sports statisticians measure athletes' performance. Companies monitor the performance of their employees, stock markets register the performance of firms, and at the semester's end students are asked to evaluate their professors. Top Ten lists are everywhere; online ratings of restaurants, movies, and books are all around; and we are frequently asked to rate the reviewers. Again: what is an effective demonstration? Protestors demonstrate. Engineers demonstrate their inventions. Rock bands, technologists, and website builders make demos – working models at various levels of completion that point to capacities for further development just as a protest demonstrates capacities for further escalation. An engagement ring signals commitment and demonstrates future earnings potential. The debates about the silent lectures are always interesting, because there's no right answer. Another one is about valuation. It's a list of photos of a hundred valuation devices, in fact, the "Top 100 Valuation Devices." When I do present it, it's as an example of how a list is not an argument. In my lecture I try to transform the list into an argument and I use this as a provocation to the audience. You know, these are examples of unconventional ways to use animations and different devices to teach sociologically important topics.

**FB:** You have collaborated with art photographer Nancy Warner to publish *This Place, These People: Life and Shadow on the Great Plains* [Columbia University Press, 2013]. What has been your role as a sociologist in this work?

**DS:** The book is a beautiful book, with sixty-five dramatic black and white photo taken mainly in Nebraska. The topic of the book is the disappearance of farm places. They are disappearing even if farming is thriving on the Great Plains. I got interested in Nancy's photos and we started to work together. I wrote the voices: I talked to people and I took field notes, so the photos were framed by the voices. I also have an essay and the end of the book where I describe the socio-economic processes that support the disappearance of the farm places. I was writing as a sociologist but not for sociology. I enjoyed it a lot.

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## **Diversity and Worth. A Retrospective Account**

David Stark in Conversation with Filippo Barbera.

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Abstract: This interview takes its point of departure from the early stages of David Stark's career and it goes from the analysis of capitalism in East European countries to the logics of worth in contemporary market economies. It shows how ideas and concepts of Luc Boltanski and Laurent Thévenot's book, *On Justification: Economies of Worth* in the mid-1980s have been originally elaborated and empirically applied to economic and organizational sociology. Topics such as the similarities and differences between European and American sociology, the standard format of publication and the scientific careers of social scientist are discussed as well.

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*Keywords: Worth; Quality Conventions; Diversity; Capitalism; Economic Sociology.*

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