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Crisis and Critique of Social Sciences

Wolfgang Streeck in Conversation with Riccardo Emilio Chesta

by Riccardo Emilio Chesta *and* Wolfgang Streeck

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1. Introduction

A recent debate on Europe as monetary union and political project brought the German public philosopher Jürgen Habermas to establish a dialogue with an old companion: the sociologist Wolfgang Streeck.

Taking off from some of the theses contained in Streeck's latest book, *Buying Time: The Delayed Crisis of Democratic Capitalism* [Streeck 2014a], the two scholars renew the public discussion on the crisis of the capitalist economic system in terms of democratic crisis and particularly as a political crisis of the European Union [Habermas 2013; Streeck 2013]. Apart from being German and, of course, European, the interlocutors are bound by some closer ties – having been contemporaries at the University of Frankfurt in the heady days of "critical theory" and of the student movement.

Accordingly, the debate picks up on the thesis regarding the economic, social and political forms of "advanced capitalism" (*Spätkapitalismus*), which to a non-expert reader could seem somewhat stuck in a past German intellectual moment – that of the 1960s renewal of Marxist theories on capitalism. In *Buying Time,* Streeck – Emeritus Professor in Economic Sociology and Political Economy at the Max Planck Institute for Social Research in Köln – uses indeed those contributions in light of his long scientific path as one of the scholar of greatest renown in the field of industrial relations and political economy.

Compared to other representatives of the so-called "second generation" of the Frankfurt School, Streeck developed a distinctive theoretical profile and research program. He refused the discursive turn taken by Habermas vis-à-vis the forms of communicative agency, with its main orientation on the analysis of the forms of democratic legitimation. However, his way of doing sociology diverged also from the intellectual routes followed by Oskar Negt – leading into the analysis of media and identity politics – and by Claus Offe, with his macro-sociological theory of capitalism and democracy.

Rather, Streeck's oeuvre marks a return to a resolutely materialist analysis of production systems, drawing on the latest methodological and empirical developments within the comparative political economy literature. In this package, "critique" finds its place in a renewal of social theory on capitalism and democracy built on a firmly empirical research program.

We took the opportunity of Wolfgang Streeck's visit to COSMOS, the Center on Social Movement Studies at the Scuola Normale Superiore in Florence, to pose him some questions on his practice of sociology, and on the meaning of "critique" in the contemporary social sciences.[1] In this interview, Streeck retraces his intellectual and scientific trajectory, giving an account of the tensions surrounding both the scientific institutionalization of his work, and its political use. If Streeck's trajectory has taken its course in world-renowned scientific institutions – the Goethe University in Frankfurt, the University of Madison-Wisconsin, the Max Planck Institute and so forth – it has also consistently been linked to a form of public engagement that follows the evolution of German sociology and society within broader processes of both specialization and globalization of the discipline. Seen from a privileged observatory – like the Faculty of Sociology in Frankfurt in the 1960s – it is also possible to diachronically understand the particular process of institutionalization experienced by German sociology within the general changes affecting the German philosophy of higher education. For example, the relation with students' associational and political cultures where Streeck also socialized – the IG Metal trade unions and the KPD as opposed to the "middle-class radicals" of the SDS (*Sozialistischer Deutscher Studentenbund*) – are considered expression of personal and collective identities – or even, forms of life – essentially intertwined with the constitution of research interests and intellectual biography.

Thus it is no surprise that the emergence of industrial relations and political economy as an autonomous research field is closely interconnected with the waves of growing expectation toward a progressive democratization of western capitalist

[1] The interview was held in Florence, at Palazzo Strozzi, on May 30, 2016.

societies at the end of the 1970s and beginning of the 1980s [Schmitter and Streeck 1981; Crouch and Streeck 1997]. The "golden age" of corporatism in industrial relations were indeed fostering confidence in the possibility of a domesticated capitalism combining efficiency and growth, as well as redistribution and social justice. Therefore, if German capitalism was – and still is – one of the central cases against which to examine the crisis of this reformist politics, the internationalization of political economy research allowed the accompanying disillusion to be observed and assessed from a comparative perspective, in a global context of neoliberal restructuring [Streeck 2014b].

The following dialogue with Wolfgang Streeck also aims to shed light on the practice of sociology as a Weberian *Beruf* – a professional and intellectual craft – and to elucidate its possibilities and limitations in the working and living conditions of contemporary academia.

2. Conversation

Riccardo Emilio Chesta: The first question concerns the main outcome of the debate on "Late Capitalism" that started during your first training in Frankfurt at the end of the 1960s. What are the main paradigms and the lesson you take into account for your ways of practicing social research nowadays and specifically contemporary research on capitalism?

Wolfgang Streeck: Your questions refer to a time that is difficult to understand for someone studying social science today. It was a different world, for better and for worse. Briefly, you're asking what the important formative experiences were for someone like me, not coming from an academic background. I had had nine years of Latin, six years of Greek, and excellent training in literature, philosophy and the like, and one might have thought that I was really well-prepared. But when I arrived at the university I found myself in a context that appeared completely alien to me, as it did to quite a few of my fellow students. Today the way you study sociology is hyper-structured: there are methods courses, courses on how to use a library, how to write a scholarly paper or give a conference presentation. At that time, there was no structure at all. Professors still lived in the old German university where students arrived ready for independent research, the *Abitur* being like the *Master* today. Students just came to seminars and took part in the discussion, and if they had something to say, professors listened to them. If they didn't, they didn't belong there. It took me two years or more to feel even remotely at home in this world.

I should add that I could live with this as I arrived at the university in 1966 as something of a political radical, way before student radicalism spread in 1968 – a condition that was not preordained by family background. I had decided to go to university rather than work as a journalist because I was interested in leftist politics and wanted to learn something useful for it. To my surprise, this was not something on offer in the world of critical theory as it existed then. What did impress me, nevertheless, was how seriously our academic teachers – Adorno, Horkheimer, Habermas – were taking intellectual life. You can say: *existentially seriously.* As a student you could learn, mostly by observation from a huge social distance, that the life of a scholar in the Frankfurt sort of social science was not about passing examinations and getting a job but about wrestling with the truth, for the truth – not just in a positivistic sense, but regarding nothing less than the right way to live, what society should be like etc. There was a pathos, a passion behind the kind of scholarship we witnessed and tried to embrace, without anyone "teaching" us in today's sense, that clearly had to do the fact that in the generation of our teachers, the memory of the barbarism of the first half of the Twentieth century was always present, whatever the subject was that was being dealt with on the surface. What was much less present was the Marxist tradition. Everybody believes today that Frankfurt in the 1960s was a center of Marxist theory. In fact, Adorno and Horkheimer at that time seemed somehow embarrassed about their work from the 1930s, before their emigration to the United States. *Dialektik der Aufklärung* [Adorno and Horkheimer 1947] was suppressed by its authors and had to be made available by the students as an illegal reprint. I think for both Adorno and Horkheimer, who were of course of Jewish descent, the paramount experience of the century was the Holocaust, which they found impossible to explain in the framework of a theory of capitalism. After the war and after their return to Germany and Frankfurt (I remember having always been puzzled as to why someone should want to set foot again in a country that had planned to kill them) the moral reeducation of German society for them took priority over everything else. It even seems that the reason why they substituted the term "critical theory" for Marxism or historical materialism was to make them more acceptable as academic figures in anti-communist Germany. They had both been saved by emigrating to the United States, and I think they had even assumed American citizenship in addition to German citizenship. In any case, the students never convinced them to sign a public statement condemning the Vietnam War.

REC: But how was the relation between you and the others in this intellectual environment in Frankfurt, not only the main theorists such as Adorno and Horkheimer, but also the other students and particularly Habermas?

WS: Habermas was obviously the rising star. Horkheimer was no longer really active, and Adorno was writing his philosophy of esthetics [Adorno 1970], which was practically impenetrable for the outsiders, certainly for the students and even more for me. On a personal level there was practically no relationship at all. I had a job as a research assistant at the *Institut für Sozialforschung* and sometimes one encountered Adorno on the staircase, which was very narrow. He was an immensely kind man so he didn't want to pass by you without saying something, but he didn't really know what to say, which resulted regularly in unbearably long moments of embarrassed mutual silence. In any case, the old German university had been epitomized as a place of *Einsamkeit und Freiheit* (solitude and freedom in English, but perhaps a better translation of *Einsamkeit* would be loneliness). You were free to do whatever you wanted, but you were essentially on your own. This was more or less difficult to bear – less in my case as my political involvement provided me with a social context that others didn't have. In part the student movement may have arisen from this anomic condition, as students began to build their own social world, being as detached as they were from an official discourse that they could hardly understand.

I am not complaining. As I said, Frankfurt was a place where you could learn to take scholarship really seriously. Nobody ever talked about jobs, or "careers". Jobs and careers would come as a side-effect, or they wouldn't. What was important was to learn how to think seriously about yourself, about society, about politics. Later in life I rediscovered some at the time more or less disjointed ideas that I was only then able to put into a context and understand – bits and pieces of "critical theory" and its effort to make sense of what was then going on in the world. Why was there no serious analysis of capitalism? Like many others, "critical theorists" (as I said, probably a cover name for those who had in the past been historical materialists, or Marxists) believed that postwar capitalism had been tamed by state intervention and by the rise of large monopolistic corporations, and had been turned into a Keynesian prosperity machine. So the problem was no longer economic, as in the Great Depression, because economically the thing had been fixed once and for all, but political – a problem of legitimation, i.e., of normative discourse, i.e., of an evolutionary logic of moral ideas.

You asked about Habermas. Now of course he himself is an object of study, students are writing dissertations on him, and there are now even scholarly biographies tracing his intellectual and political development. I cannot claim to be nearly as well informed. It is my impression that when I was a student in Frankfurt, Habermas went through his most productive phase, in a way that was truly breathtaking to watch, as he absorbed and assimilated into his own thinking what seemed to be just about everything ever written in philosophy, psychology, and sociology. For us as observers,

his lectures and seminars fell under a common theme, which was "There is nothing from which I cannot learn something." With hindsight I think he used these years to build up that immense supply of ideas on which he has been feeding ever since, extracting what he believed he needed from German idealistic philosophy, Marxist political economy, American pragmatism, Parsonian functionalism, developmental psychology, psychoanalysis, linguistics, and so on, ad infinitum. Much of this, I now think, was an effort to radically, profoundly break with what he perceived as Marxist economic determinism. But this was not really clear to me at the time. Now I think, colloquially speaking, that on his way from the older Frankfurt traditions to the sort of idealism that I find underlies the theory of communicative action, he must have thrown out the baby with the bathwater, eliminating almost completely what we call "the economy", or the material reproduction of society, or the social power associated with the ownership of essential material resources.

REC: On this point you mention in *Buying Time* [Streeck 2014a] that the sociologists at the *Institut für Sozialforschung* had learned from Pollock that under advanced domesticated capitalism, technocratic management had rendered theories of capitalist contradictions irrelevant. So the problem became one of legitimation, and the *Institut* started studying the political consciousness of students and other groups in society that seemed to be potentially critical or capable of producing crises of legitimation.

WS: Adorno was still very much interested in the sources of antisemitism and the authoritarian personality. Habermas, as far as I can say, was concerned about the rise of a technocratic consciousness that he feared would foreclose political discussion about a better society.

REC: So this last point brings in the issue of experts and intellectuals, while Marcuse introduces the so called *outsiders.*

WS: Marcuse was different; I realized only with distance how close he still was to his time as a student of Martin Heidegger. He was willing to challenge technology and rationality in a more fundamental way than Habermas was. Habermas, who later moved on to some sort of social-liberalism, allowed the rational world, technology, and the economy a legitimate place. Instrumental rationality as such was not to be dismissed or rejected. Rather it was a matter of how you used and controlled them. For Habermas today, I think, the question is one of democracy not allowing the technocrats that are needed to maintain the modern world to confuse their instrumental rationality with the substantive rationality of the "lifeworld". Whereas in Marcuse, modern technology-cum-capitalism was inimical to the good life due to the historically outdated social discipline that it imposed on people.

REC: Yes, because in essays like *The End of Utopia* [Marcuse 1967] he discusses with the students how the actual possibilities of technology in advanced capitalism already makes possible the overrun of natural necessity and the realization of freedom, given that the system is no longer facing material scarcity. Therefore, even in this case, the problem is no longer in the realm of contradictions, but it resides in how a certain power on technology makes accumulation for few specific social actors possible, while at the same time creating new forms of exclusion.

WS: This theme was present among the students, and they managed to invite Marcuse to fly in and speak to them. On all of this there is now historical scholarship, and those wanting to know more should consult it. Habermas, of course, was reluctant to talk about revolution, let alone about violence. For him, the political world was, or had to be, a world of discourse and dialogue, of collective reasoning, or in any case had to be conceived that way. Revolutionary violence was never productive, and probably unnecessary, certainly in Germany at the time. When the student revolt turned radical, initially perhaps meaning no more than trying to assign violence a legitimate place in a theory of revolutionary change, he was taken aback early on. And he was right in that he saw early on the putschist tendencies coming that were a disaster for the left.

REC: So he used the term "*Linksfaschismus*".

WS: Yes, in the heat of rhetorical battle, and confronted with language that may indeed have contributed to the rise of German terrorism in subsequent years. All of this was long ago, and I have only my personal memories to draw on in answering your question. Clearly there were developments in the student movement in Frankfurt in the early 1970s that resembled what you had in Italy in the form of the *Brigate Rosse*. In any case, you may want to note the difference between Habermas and someone like Marcuse who, given what he identified as excessive repression vested in "the system", was much more willing to condone revolutionary and perhaps even violent counter-mobilization. Ideas like this you also find in Barrington Moore, Marcuse's life-long friend and collaborator, who, looking at the United States of his time, at the end of the book on *The Social Origins of Dictatorship and Democracy* [Moore 1966], argues that the question of revolutionary violence cannot be taken off the agenda even and precisely in affluent, economically developed democracies such as the United States. He leaves this question open, whereas Habermas had, I think, very early and more or less implicitly, opted for social-liberal democracy, at a time when many of his students found Mao-Tse Tung's aphorism, according to which "political power comes out of the barrel of a gun," increasingly plausible.

REC: When you say "the students", which group are you referring to? Did you belong to any specific group, like the SDS (*Sozialistischer Deutscher Studentenbund*)?

WS: I never joined the SDS because in my perception at the time, it was much too upper-class in its social composition. I was interested in "the working class", real people as organized in trade unions and the Social Democratic Party, especially its left wing, which was in opposition to the national leadership and which was particularly strong in Frankfurt. In fact, at some point in 1968, I was about to join the then-illegal Communist Party (*KPD, Kommunistische Partei Deutschlands*). If I had I would have ended up with two party cards at the same time: one from the SPD, which was legal, and one from the KPD, which was illegal. Comrade Brezhnev rescued me when he invaded Czechoslovakia and friends of mine there were expelled from the CP and even had to go to prison.

REC: Concerning your intellectual trajectory, when was the moment in which you decided to start to specialize on industrial relations? Does this have to do with this cultural and political environment in Frankfurt?

WS: It had both a political and an intellectual reason. To make a contribution as a social scientist, I had to find something where I would not feel overwhelmed by the intellectual powers that were ruling Frankfurt – a sort of niche-building exercise. Nobody knew much about industrial sociology, so I began to focus on trade unionism and business associations, on workplace participation, corporate governance, etc. Some of my friends at the time were trade union officials who were also activists in the local SPD. I often had lunch with them at the canteen of the metalworkers union, *IG Metall*, which was an important source of social contacts as well as material support. On such occasions I became curious how such organizations really work. For a long time from then on, my intellectual and political agenda became to explore the possibilities for a democratic society with strong trade unions to domesticate the capital-labor relationship through institutional regulation: what sort of institution was necessary for this and what sort of political mobilization? So you can say that my work on democratic corporatism, on trade unions, on production systems, on markets and the relationship of different political economies to material production was an attempt to contribute to an up-to-date, if you will "laborist" concept of social democracy. How to defend and extend the configuration of postwar democratic capitalism in such a way that it remained redistributive in an egalitarian sense – a reformist program that tried to preserve and develop the idea of democratized capitalism into the 1970s, 1980s, 1990s. In this period, I was working as a leftist empirical social scientist, with a policy orientation that was not necessarily connected to the grand theories. I wanted to be useful.

REC: So it was a moment in your intellectual trajectory in which you turned to empirical research.

WS: It was empirical research in a political perspective, in touch with reformist policy-making and in particular trade unions, even later, in the late 1980s when I returned to the United States. The idea was to use what we know as scholars to help make this society pragmatically and practically better than it might be without reformist intervention. This is the starting point of the article in *Stato e Mercato* that you mention [Streeck, 2014b].

Looking back at my own career, I would like to add that I was lucky to be among the first generation of social scientists whose careers were genuinely international, in the sense that we were, from early on, part of an international cohort of colleagues, first fellow students, then fellow professors, a cohort which shared perspectives, observations, results, conclusions. In 1972 I had had enough of the increasingly neurotic Frankfurt scene, with Bader-Ensslin-Meinhof and their friends having become a collective obsession, so I went to study in the United States, supported by a very comfortable scholarship, hoping to get away from what was left of the Frankfurt School after Adorno's death and Habermas' move to the Max Planck Society. Ridiculously, however, when I arrived at Columbia, one of the first things they showed me was the office where Adorno and Horkheimer had worked after their arrival in the United States!

Back to what I was about to say. During this time and later on, I settled into an international group of people who got to know each other in the 1970s, such as Colin Crouch in Britain, Marino Regini in Italy, a little later Philippe Schmitter in the U.S., Robert Boyer in France, Jelle Visser in the Netherlands, and others. They all shared the idea that democratic politics could and should be used to inject an element of egalitarian interest representation of labor into an otherwise liberal democracy and what still was a capitalist political economy. I suppose this was and still is the *Stato e Mercato* program. We continued to work along these lines for several decades after the excitement of the 1970s was over, after the terrorist turn of the radical wing of the student movement and the liberal-democratic turn of "critical theory". This is now becoming a long story. Let's cut it short and say that in the early 2000s, with the progress of financialization, as I say in *Stato e Mercato*, many of us felt forced slowly to accept that this program had had its time and that it was about to lose its political significance in a world of declining growth and increasing inequality – to accept the disappearance of labor-inclusive corporatism on a broad front and the arrival of "post-democracy".

The work that I did from then on returns to the notion of capitalism. It looks at the global system of advanced capitalism, the family of rich capitalist democracies as a whole. It draws on previous work in comparative political economy, although for me now, the commonalities of national capitalisms have become more important than the differences. For a while, many of us were intrigued as to how Germany,

as a relatively large country, managed to be relatively egalitarian well into the 1980s while at the same time being economically successful. But this ended in the 1990s and thereafter, when German capitalism increasingly became like neoliberal capitalism, bringing back older, fundamental questions about capitalism as such. For me it was the moment when I began to adopt a longer-term perspective in institutional change now conceived as capitalist development, which implied that the social-democratic institutionalism that we had cultivated for so long might have had a problem, or you could say: might have suffered from a theoretical problem, which was that it was not sufficiently rooted in a theory of capitalism.

REC: In this regard you mention the notion of "late capitalism". In which sense was it a useful but ambivalent notion?

WS: In the 1970s the idea of "late capitalism", or *Spätkapitalismus*, implied that capitalism was on its way out because in a democratic society it could not pass the test of legitimation. Under democracy, in the long term, a social order would and could achieve legitimacy essentially only if it was a post-capitalist social order: one that was able to satisfy newly arising, "post-materialist" human demands for autonomy, participation, self-realization. The 1980s, a decade later, were a time when critical political economy in the strict sense was almost dead. By this I mean that economic crises and questions of economic power were seen as unimportant for the stability of capitalism, whereas the real problem was for the capitalist way of life, the *Lebenswelt* as reshaped by the requirements of continuing capital accumulation, to be accepted as, well, legitimate. So critical political economy was replaced with comparative political economy, which came down to reasoning about what one can do differently in different countries to make them more equitable – more capable of achieving legitimacy with a more demanding citizenry – while preserving their economic prosperity. How can one jointly attend to demands for both economic efficiency and social justice – this was the political core of historical-institutionalist comparative political economy. The kind of political economy that was coming out of economics at this time was efficiency theory pure and simple, and the work of Peter Hall and David Soskice on "varieties of capitalism" [2001] began to intersect with it. With hindsight I regard their approach as economistic efficiency theory dressed up as historical-institutionalist social science.

In my view, the construction problem of the critical theory of the 1970s that presaged the march into "varieties of capitalism" was what they bought into the Keynesian idea of the ability of the State to turn capital into a wealth-production machine to be operated by, essentially, technocrats, based in the state or in large corporations. This was a view that was widely shared among the social-democratic parties of the time – social-democratic in a generic sense. It might have been recognized as mistak-

en already in the 1970s when the revolt of capital and its handlers against the postwar order of "democratic capitalism" began. In a metaphor I have sometimes used, the 1970s were a time when capital had had enough of having to serve as the milk cow of democratic politics, reasserting its true nature as a predator – a hunter for limitless profit – and demanding to be let out of the Keynesian-democratic cage for the adventure of taking care of themselves in the wild.

When the social-democratic project began to falter, where I thought I could make a contribution was by bringing back to the debate the long-forgotten or underestimated notion of the historicity of capitalism and its endogenous dynamic of capital accumulation. In other words, the formation that is called capitalism is not a machine, but has a will of its own – or more precisely: it is a social configuration driven by collective imperatives imposed and enforced by a ruling class. As such it is given to certain behaviors that we cannot choose or not choose, as long as we live in a capitalist society. One of these behaviors is for capitalism continuously to expand its range. It has to grow. My friend and colleague Joel Rogers and I were among those who in the early 1990s rediscovered Polanyi, when we were developing an economic sociology program at the University of Madison-Wisconsin. Central to our reception of Polanyi was his concept of a "double movement". While this is not a complete theory of capitalist development, it has at its core the idea of a historical movement that is endogenous and goes in a certain direction – commodification, marketization – unless you do something about it and it is checked by a counter-movement. The correct critique of technocracy, this is to say, is not the reintroduction in theory and practice of free democratic discourse to guide technocracy into the right direction. The real critique is to recognize that what you are dealing with is not technocracy at all but a live animal that wants to eat you – an alien that wants to subject your collective and individual life to its strange obsessions – and you have to do something about it.

REC: Basically you reintroduce an element of agency, criticizing the fact that the theories of late capitalism were almost a form of Neo-Functionalism.

WS: Yes, absolutely. It is an immensely complex issue as we are dealing with the question of whether we can have, or have to have, a theory of societal evolution. That is, if societies are historical phenomena and they develop in a particular direction, what drives that development and determines in what direction it proceeds? Is the driving force a dynamic of capital accumulation or of economic rationalization or of moral discourse, or indeed progress? What sort of irreversibility do we have to assume, if any at all – in technology, democratic participation, market expansion, moral consciousness? Essentially it may be an issue of materialism versus idealism, but playing the two against each other may be a mistake. One has to see material tendencies and moral reflection as two movements modifying and correcting each other.

I think this is exactly what someone like Polanyi has been trying to conceptualize: he explains how there is an underlying material dynamic and then there is a political, religious, ideological, humanistic response to it, hopefully capable of governing the material dynamic to which it responds.

REC: How do you think the analysis of the morality of capitalism presented in Luc Boltanski and Eve Chiapello's book [1999] fits in here? They introduce the multiple dimensions of morality created by capitalism to justify and legitimate its existence, but also its capacity to subsume some critical elements present in society – as exemplified by the so called "1968 artistic critique".

WS: I don't believe in total subsumption. My fundamentally optimistic belief is that capitalism can never encompass the totality of human existence. There's always something more there. And that "something more" is going to be rebellious. In fact you can see it even today, in a consumption-controlled world where large numbers of people are deeply dissatisfied although there is now huge, highly sophisticated machinery at work specializing in making them feel good. What makes people feel dissatisfied is not just material deprivation. Sometimes dissatisfaction expresses itself in such terms, but often what is really behind it is that people hate being pushed around, and that working as an "event manager" or buying one pair of running shoes after the other is, in the longer run, not enough to make you happy. In this sense I think Boltanski and Chiapello are overstating their case. The book is convincing to a point, as it points out how flexible capitalist strategies of integrating "human resources" into the accumulation process are and how inventive they can be if they face a new generation. They know they have to do something to suck fresh blood into their machine, but I don't think they can suck them in one hundred percent. If this were possible it would indeed be the end of history. Perhaps this is the Marcusian world of one-dimensionality. Marcuse, like Heidegger, would claim that you can do whatever you want in order to domesticate the human existence, but there's always something more than you can grasp. Essentially I believe that, too.

REC: In fact, this Heideggerian idea of the irreducibility of ontology in the face of scientific and technological conceptions or domestications is also one of the main foundations of Max Weber's sociology. Its view of the disenchantment of the world as a natural outcome of the process of rationalization of the Western world was a pessimistic or even desperate view, but at the same time Weber also recognizes the disruptive character of the lifeworld (through phenomena such as social movements, charismatic leaders, etc.).

WS: Yes. Rationalization can only go so far. There will always be a struggle for a space where individuals are liberated from social constraints. No institution will ever be able completely to subject its subjects. As sociologists know, this applies even to

total institutions, such as prisons. In fact, prisons have been found to be privileged spaces for human creativity, for example with regard to how to bring in drugs or guns. Or think of the complex underground societies that existed in concentration camps, in spite of barbaric efforts to destroy them.

In my work on institutional change with Kathleen Thelen [2005], we conceive of institutions in the Weberian sense as *Herrschaftsverbände*, with rules and norms enforced by a staff endowed with specific means of power. The struggle is over the creation of the rules and their application. Even the simplest rule, if applied to a specific situation, needs interpretation, and in this sense following a rule is always a creative act. You can apply a rule in different ways and you can fight over its meaning. Every situation to which a rule is to apply is different – it is particular while the rule is general. When a rule is made, not all of the specific situations in which it is to be applied can be known in advance. As a result of the unpredictability of the concrete situations in which a norm has to be applied, norms (meaning also social structures) evolve all the time through their interpretation "on the ground", through their inevitably creative application.

This is one way of saying that there are always spaces of uncertainty inherent in social structures that make these structures dynamic. On a larger scale you can say that any institution is contestable and therefore will be contested, and social arrangements will turn critical when they meet a situation that was not anticipated when they were made. Applying this to the history of capitalism: at a certain point it exists in a certain way, changes the world, and then, once the world has changed, must change as well through reform, or be changed through revolution – but it cannot be static, regardless how hard one fights to keep it as it is.

REC: On this last point, how do you introduce the role of intellectuals, who nowadays have to face difficult obstacles? In a conference at the EHESS in Paris you were talking about the functions of critique. You were basically affirming that nowadays intellectuals are not asked to intervene in the public sphere by political parties or others – because they need electoral strategists, public relations specialists and all these technocratic professions that are used for very specific practical goals like running a political campaign or winning elections. How do you think it is possible nowadays to practice a critical role as intellectuals and to whom does this intellectual have eventually to speak?

WS: This is a big puzzle. Is anyone listening? Who is being listened to? As a minimal definition of the role and the social space of critical intellectuals, we still have some influence in our classrooms, although there are significant efforts to take it away from us – I'll just mention the Bologna Process and university reform in general. The range of ideas we can discuss in academia, and the way we can discuss them, is

narrowing under the pressure of economization. Our first task, I believe, is to defend with claws and teeth the freedom and indeed the practice of academic discussion. In the last chapter of his *General Theory* [1936], Keynes asks himself whether what he thought he had found out about the nature of the capitalist economy and what one could do about it would actually be applied in practice. People now in power, he says, have learned what they believe they know when they were between 20 and 30 years old; later they remain beholden to it and are unlikely to return to learning. As academic teachers it is our privilege that we can reach those that are now in their formative years and make them familiar with the latest ideas, hoping that they will in a decade or two be in a position to apply them. Leaving aside the question of whether we have that much time today, using the university as a basis for disseminating new ideas requires a university that doesn't exclusively consist of examinations and the acquisition of social entitlements. Such universities are rare today, and they are becoming rarer. They must be defended and extended, meaning that the life of a productive social scientist will have in good part to be devoted to organizing resistance to strong and growing pressures for ideological and intellectual uniformity.

REC: In your opinion, what are the journals in which it is possible to publish innovative and critical research?

WS: In American sociology we have seen interesting developments taking place in recent years. Many articles in the *American Journal of Sociology* now have an historical background and understand that the United States of America is not as a matter of course the universal model of modern society. Moreover, the economy, and indeed the political economy, is given an increasing role. Fewer and fewer sociologists today seem to be willing to abide by the peace treaty that Talcott Parsons negotiated with the Harvard economics department, defining the turfs of the two disciplines in such a way that they didn't get into conflict with each other – in effect depriving sociology of some of its most important and most foundational themes. Also, journals such as the *Socio-Economic Review* (of which I was chief editor for five years), *Economy and Society*, and others like them are wide open for work in political economy. I'd also like to mention journals outside of the narrow range of scholarly publications that are worth considering, such as the *New Left Review.* I was surprised how many people in the social sciences read work published in the *New Left Review,* as compared to mainstream sociology and political science journals. Basically I think that one has to be attentive to these possibilities and indeed that we should learn early how to reach people who are not members of our discipline – to speak to an audience that doesn't consist exclusively of social scientists. This is what I tried to do in *Buying Time*, and apparently it seems to have worked.

REC: So you mean that you made an effort to make your book readable for non-specialists, for social actors who are affected by the actual situation of contemporary capitalism.

WS: When writing, I always try to be as clear as possible. When I write a text, I revise it ruthlessly, at least three times. And I try to be merciless with myself, going over each sentence to detect and clear up any unclear or foggy phrasing. I read my writing with the eye of a detective: is there something I myself have not completely understood, which means it cannot possibly be understood by someone else.

REC: Is this approach maybe an outcome of the fact that you feel sad about having missed too many of Adorno's classes?

WS: That was such a long time ago. What is true is that I don't want my students to pretend that they have understood something if I have failed to express clearly what I wanted to say. After a seminar session I sometimes remember points in the discussion when with hindsight it seems to me that I have papered over a gap in my own understanding in order to be able to say something. I associate such situations with specific expressions on the faces of students in the audience, some of them looking blank while others make an effort to pretend they have grasped something that they couldn't possibly have grasped because it was not clear enough. There are also students who have convinced themselves in response to unpleasant seminar experiences that something can be recognized as particularly sophisticated based on the fact that they found themselves unable to understand it. So, in Latin, *clare et distincte* is the way you should make yourself work hard to write and, hopefully, speak. Then the gap between professional writing and writing for an educated public is not so dramatic. If you discipline your professional writing to say only what you have really understood yourself, you can express it in a way that it is intelligible not just for specialists.

REC: As a last question, something that I didn't plan to ask you but that came out of our discussion today. You said you studied ancient Greek and Latin for several years. It seems that your sociological and intellectual formation is linked to what we could ideally call a European *Bildung.* As a German sociologist, you have a classical background – a link with the Latin or Mediterranean world that seems to date back to Goethe's *Italienische Reise* or Weber's tour in Italy – do you think this should be an important part of the background of a European sociologist?

WS: I wish it were. For me at least, being European is inevitably connected to being able to feel a sense of the interconnected European history when looking at a particular landscape or cityscape in Europe. This is helped by at least some familiarity with the foundation of European languages in our common cultural mother tongue, Latin. When someone asks me where I feel emotionally at home, my answer is Europe in the borders of the Western Roman Empire. There's another angle to this, which

is that I am convinced that if you want to do good sociology, you have to read a lot of history. At some point, when I found myself moving away from functionalism and policy research and toward the inherent dynamics of social change and development, it was a confirmation that it is important to know about the history of the societies we study as well as the history of the theories of such societies. Incidentally, one should not forget how much of sociological theory draws on the history of the Western Roman Empire, especially its decline and fall – not least Max Weber of course. I am far from being competent in these areas as a scholar, but I do like to read as much as possible on this period in my free time (which was supposed to have increased since I became emeritus) just to enrich my repertoire of subjects, puzzles, and examples.

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References

Adorno, Th.W.

1970 *Ästhetische Theorie.* Frankfurt am Main: Suhrkamp. [En. Tr. 1997, *Aesthetic Theory.* Minneapolis: University of Minnesota Press].

Adorno, Th.W. and Horkheimer, M.

1947 *Dialektik der Aufklärung.* Amsterdam: Querido. [En. Tr. 2002, *Dialectic of Enlightenment: Philosophical Fragments.* Translated by E. Jephcott, edited by G. Schmid Noerr. Stanford, CA: Stanford University Press.

Boltanski, L. and Chiapello, E.

1999 *Le nouvel esprit du Capitalism.* Paris: Éditions Gallimard [En. Tr. 2005, *The New Spirit of Capitalism.* London, New York: Verso Books].

Crouch, C. and Streeck, W.

1997 *Political Economy of Modern Capitalism: Mapping Convergence and Diversity.* London: Sage.

Habermas, J.

2013 "Demokratie oder Kapitalismus? Vom Elend der nationalstaatlichen Fragmentierung in einer kapitalistisch integrierten Weltgesellschaft." *Blätter für deutsche und internationale Politik* 5: 59-70. [En. Tr. 2013, "Democracy or Capitalism? On the Abject Spectacle of a Capitalistic World Society Fragmented along National Lines." *Reset DOC - Dialogue On Civilization. The Web Magazine for all the Tribes of the World* (July): http://www.resetdoc.org/story/00000022337

Hall, P. and Soskice, D. (eds)

2001 *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage.* Oxford: Oxford University Press.

Keynes, J.M.

1936 *The General Theory of Employment, Interest and Money*. London: Palgrave Macmillan.

Marcuse, H.

1967 *Das Ende der Utope. Vorträge und Discussionen in Berlin.* Frankfurt am Main: Verlag Neue Kritik. [En. Tr. 1970, *Five Lectures: Psychoanalysis, Politics and Utopia.* Translated by S.M. Weber, edited by J.J. Sapiro. Boston: Beacon Press].

Moore, B.

1966 *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World.* Boston: Beacon Press.

Schmitter, P. and Streeck, W.

1981 *The Organization of Business Interests. Studying the Associative Action of Business in Advanced Industrial Societies.* WZB Discussion Paper IIM/LMP/81/13, Köln.

Streeck, W.

2013 "Vom DM-Nationalismus zum Euro-Patriotismus? Ein Replik auf Jürgen Habermas." *Blätter für deutsche und internationale Politik* 9: 75-92.

2014a *Buying Time. The Delayed Crisis of Democratic Capitalism.* New York: Verso Books. [Or. Ed. 2013, *Gekaufte Zeit: Die vertagte Krise des demokratischen Kapitalismus.* Berlin: Suhrkamp].

2014b "Taking Crisis Seriously: Capitalism on Its Way Out." *Stato e Mercato* 1: 45-68.

Streeck, W. and Thelen, K.

2005 *Beyond Continuity. Institutional Change in Advanced Political Economies*. New York: Oxford University Press.

Crisis and Critique of Social Sciences

Wolfgang Streeck in Conversation with Riccardo Emilio Chesta

Abstract: Retracing Wolfgang Streeck's scientific path, the following interview illustrates some key nodes in critical political economy to finally focus on the general state of contemporary sociology. As a specific stream of a scientific niche, critical political economy addresses indeed relevant questions to both empirical research and sociological theory. Rooted in the so called "critical theory", Streeck explains how every analysis of the institutional frameworks of contemporary capitalism cannot be detached from a historically grounded and a theoretically informed macro-sociological research. This peculiar articulation allows moreover to investigate the relations between social sciences research on diversity of capitalism and its political salience for democratic capitalism. Moving from personal experiences until general assessments on the state of the discipline, the interview finally aims to shed light on the practice of sociology as a Weberian *Beruf* – a professional and intellectual craft – and to elucidate its possibilities and limitations in the working and living conditions of contemporary academia.

Keywords: Critical Sociology; Political Economy; Democratic Capitalism; Neoliberalism; Intellectuals.

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Wolfgang Streeck (Lengerich, 1946) is Emeritus director of the Max Planck Institute for the Study of Societies in Cologne. Considered as one of the contemporary heirs of the "critical theory", he studied sociology at the Goethe University Frankfurt with Theodor W. Adorno, Max Horkheimer and Jürgen Habermas. Moreover, he pursued graduate studies in the same discipline at Columbia University between 1972 and 1974. Recent publications include *Buying Time. The Delayed Crisis of Democratic Capitalism* [Verso Books, 2014] and *How Will Capitalism End?: Essays on a Failing System* [Verso Books, 2016]. More information about him and his scholarship are available on his personal website: https://wolfgangstreeck.com/

Filippo Barbera, David Stark

Diversity and Worth. A Retrospective Account. David Stark in Conversation with Filippo Barbera

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Interview

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 become 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 Jànos Lukàcs. 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 and 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 striving 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.

References

Boltanski, L., Thévenot, L.

2006 *On Justification: Economies of Worth*. Princeton, New York: Princeton University Press. [Or. Ed. 1991, *De la justification*. Paris: Gallimard].

de Vaan, M., Vedres, B., and Stark, D.

2015 "Game Changer: The Topology of Creativity." *American Journal of Sociology* 120: 1144-1194.

Grabher, G., Stark, D.

2009 "Commentary. Frequently Asked qQestions." *Environment and Planning A* 41(2): 255–257.

Pinch, T.J., T.J. Pinch,

1988 "Reservations about Reflexivity and New Literary Forms: Or Why let the Devil Have All the Good Tunes?" Pp. 178-197 in S. Woolgar (edited by), *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*. London: Sage.

Sabel, C.F., Stark, D.,

1982 "Planning, Politics, and Shop-Floor Power: Hidden Forms of Bargaining in Soviet-Imposed State-Socialist Societies." *Politics and Society* 11(4): 439-475.

Skocpol, T.

1979 *States and Social Revolutions: A Comparative Analysis of France, Russia and China.* Cambridge: Cambridge University Press.

Stark, D.

1980 "Class Struggle and the Transformation of the Labor Process: A Relational Approach." *Theory and Society* 9(1): 89-130.

1990 "Privatization in Hungary: from Plan to Market or from Plan to Clan." *East European Politics & Societies* 4(3): 351-392.

2009 *The Sense of Dissonance: Accounts of Worth in Economic Life.* Princeton and Oxford: Princeton University Press.

2013 *This Place, These People: Life and Shadow on the Great Plains*. New York: Columbia University Press.

Diversity and Worth. A Retrospective Account

David Stark in Conversation with Filippo Barbera.

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.

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

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Laszlo Bruszt, Claus Offe

Claus Offe in Conversation with Laszlo Bruszt

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Interview

Theorizing Crises and Charting the Realm of the Possible

Claus Offe in Conversation with Laszlo Bruszt

by Claus Offe *and* Laszlo Bruszt

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1. Conversation

Laszlo Bruszt: Let us start with questions about your intellectual trajectory. How this was influenced by the fact that you made your doctoral defense in 1968 and in Frankfurt?

Claus Offe: I had graduated in Sociology, an entirely new field in German academia at the time, at the Free University of Berlin in 1965. Subsequently, I was fortunate enough to be offered a teaching assistant position by the young and not yet widely known professor Jürgen Habermas who had just accepted a chair in philosophy and sociology at the University of Frankfurt. I had been very impressed by the book of Habermas, *Strukturwandel der Öffentlichkeit* [1962], and I still consider myself very lucky to have had the opportunity to work with him (for a total of no less than ten years, as it turned out).

I was somewhat familiar with the intellectual scene of Frankfurt from reading what Frankfurt School professors wrote in the 1950s. I was also active, since 1962, in the German League of Socialist Students (SDS). I was even on the board of that organization which was headquartered in Frankfurt. SDS was a small group of aspiring radical intellectuals which focused, mostly in a study group format, on contemporary events (such as post-colonial liberation wars e.g. in Algeria) and on various currents of western Marxism. These included the psychoanalytical tradition, represented by Adorno and Marcuse, and the classical writers of the interwar period such as Georg

Lukács, Otto Kirchheimer, Karl Korsch and many others. I still own a mimeographed copy of *History & Class Consciousness* as such books were not available in print at the time. Only a very limited selection of Marx's writing was available on the West German book market. They could be cheaply purchased in East Berlin bookstores. I got involved in contentious issues of organized science and university reform, a topic on which I co-authored my first book [1965, with a preface by Habermas]. German social science was at the time deeply divided between emigrant who had returned after fleeing Nazi Germany, on the one hand, and former Nazi intellectuals who had re-educated themselves to be loyal supporters of post-war German democracy. On two prominent authors of the latter category I had written my master thesis.

LB: The old Frankfurt school, was it what you could read?

CO: No, that was not possible. Neither the famous journal *Zeitschrift für Sozialforschung* nor *Dialektik der Aufklärung* were accessible. The rather absurd situation was that all these materials were locked up in a room in the basement of the Frankfurt Institute building so that nobody had access. This was true until the late 1960s, early 1970s. This absurdity was due to the fact that the Institut für Sozialforschung was licensed and supported by the American occupation forces. So the two senior persons, Adorno and Horkheimer, were terribly afraid that their theorizing could be used for political purposes which would annoy the Americans in the context of the incipient Cold War. There may have also been a timidity of students drawing unwelcome political conclusions from the philosophically radical writings of the heads of the Frankfurt School. This latent conflict of the School with itself culminated in 1967 when radical students tried to occupy the Institute building, yet were prevented to do so by the police that Horkheimer and Adorno had called in. While these two heads of the Frankfurt School showed fearful reluctance to engage in debates with the students, Habermas was the one who openly engaged in the conflict, trying to make a modicum of political reason prevail in this highly turbulent confrontation. He was a practitioner at that early point in time of what he later came to call "communicative action".

Anyway, so thank God, I had managed to finish, in the summer of 1967, my PhD thesis, which was a critique of meritocracy in the context of organization theory. Also came out as a book, which became quite popular, with five editions printed in German; it was also published in an English translation [1975].

LB: How did you get into his circle?

CO: I mentioned that I was one of the four authors of a 500-page book that came out in 1965 under the title *Hochschule in der Demokratie*, with chapters on the legal, philosophical, historic, and sociological aspects of academic institutions. The common question was: What is wrong with the German university, how can it be

reformed and its better traditions preserved? After the manuscript was ready I asked the young philosopher in Heidelberg, Jürgen Habermas, to write a preface, which he did. He also contacted my supervisor, Ludwig von Friedeburg. As a consequence, Habermas offered me my first job as a teaching assistant. I was 24 years old, happily accepting the best job that I could ever dream of.

I was teaching courses and seminars with him. The informal division of labor was that I was "in charge" of macro-social phenomena, including some political economy issues I had studied in Berlin, whereas Habermas was strongly interested in psychological (including psychoanalytical) issues at the time. I also had the opportunity to introduce him to a person the name of whom he had never heard before: Niklas Luhmann. Myself and others of my cohort felt at the time that Luhmann with his incredibly prolific writings was about to become an outstanding and intellectually dominant figure in German social theory. By late 1969, the two were engaged in writing a book-length controversy which strongly influenced German theoretical debates in the 1970s and beyond.

What I came to understand during my years in Frankfurt (1965-1969) is the need for social scientists to familiarize themselves with the great American and British traditions of social thought. Believe it or not, English sources were virtually absent from the reading lists of my student days. Even after that had slowly changed, the idea of reading English remained unpopular with the students. Authors such as Parsons and Bendix, Dahl and Lipset, Hunter, Bell and Mills were still far from being part of the canon of required readings. I was, again, very fortunate in being granted a post-doc fellowship starting in the fall of 1969 to spend eighteen months at UC Berkeley and Harvard, which allowed me to compensate for some of the parochialism of German academic training in the social sciences.

LB: This was a starting. The reason why I have asked this question was to find out how this kind of starting affected the way you ask questions, the type of questions you ask or you approach the research of capitalism.

CO: There was one major Marxist intellectual in German academia, the law professor Wolfgang Abendroth, who had sponsored the *Habilitation* of Jürgen Habermas after the latter fell into conflict with the heads of Frankfurt School, in particular Horkheimer, and moved from Frankfurt to Marburg. Abendroth's writings on German post-WWII polity, economy and class politics brought me to appreciate the intellectual as well as political potential of a Marxism-inspired analysis of contemporary developments. Other than that, I had read some of the major classical writings of Marx and Engels, such as the Paris manuscripts, the *18 Brumaire*, the first volume of *Das Kapital* and later the *Grundrisse*.

LB: In the courses in Frankfurt you have not read Marx?

CO: The basics of historical materialism were supposed to be known by the Frankfurt School students. They served as a background orthodoxy in these circles yet were not taught, analyzed or challenged. I do not think there was much relevant work applying Marxist theories to contemporary developments. The closest thing to such a bridge came from books such as those written by Braverman, Baran, Baran and Sweezy, Dobb, which, however, were not widely read.

LB: So Frankfurt School *per se* did not have a lasting impact on your way of thinking?

CO: No, I would not say so. The "Frankfurt School" is not a canonical body of thoughts and doctrines. In its classical period from the late 1920s to the mid-1940s, the cohesion of the School was based on the shared question: What are the social forces that immunize people against an adequate understanding of the irrationalities of capitalism-cum-administration? With the tentative answer being: *Kulturindustrie*! It is always an exciting experience to read *Minima Moralia*.

I started, I should have mentioned perhaps, I started as a student musicology at the university of Cologne before I went to Berlin in 1960. I did one year of basic training in musicology, and then I shifted to sociology. And in my first year, I read a lot of the musicology writings of Adorno, with the greatest excitement and respect. That what was he was really strong in writing about – Bartok, Mahler, "contemporary music" as he called. But it was tangential to my later studies and interests.

LB: And Habermas, how would you formulate his more long-term effect on your intellectual trajectory?

CO: You cannot but learn a lot by osmosis, as it were, by working with and talking to a person who is such an exceptionally broad, rigorous and at the same time politically ambitious scholar and intellectual, as he still is today. At the same time, I must confess that some of his philosophical writings sound to me as coming from a different planet. I do not follow them, I do not see the intellectual motivations behind it. My range of knowledge and interests is simply much narrower than his. His strong interest in psychology and psychoanalysis was also rather tangential to my field of competence. At the same time, we share a strong interest in the fates of European integration, another field where I have learned a great deal from him.

LB: Actually – this is I wanted to ask, later somewhat – your book *Europe Entrapped* seems in a way to be talking to him.

CO: Yes, that is not wrong. I think I had a much more skeptical perspective on Europe. I remember in the early years in Berlin after 1995 I participated in discussions where I defended the point of view: It is hopeless anyway and nobody is going to accept this. And from my own today perspective that was superficial and a bit arrogant. But then I learned more and more.

What he was never strongly interested in is the issue of 1989, what he then called slightly dismissively a "catching-up revolution", *Nachholende Revolution*. He has never visited the region, except for Poland, I think, where he had one conversation with Adam Michnik. But other than, he was not interested. And he was never interested in anything that has to do with industrial sociology, organization, bureaucracy, public administration. And what he later called, also dismissively, the "media-controlled subsystems" of political power and capital. So I would say, we complemented each other to an extent. That certainly applies to my work on the transitions in Central East Europe and my interests in the formation and implementation of public policies. However, our interest in democratic theory and in understanding the causes of contemporary democratic malaise is a point of strong convergence.

LB: I will come back to that. It seems to me that in a way in your studies, or your writings on Europe, you seem to have converged in several ways to his way of thinking.

CO: Yes, I think so. And here we have large area of shared views.

LB: Before getting to the issue of Europe, I wanted to ask you a more general question. You dealt with so many different areas in your works, but two keywords come to my mind that nearly everywhere come up in your work: "contradictions", one of the keywords I find, but please correct me if I am wrong, and the other is "crisis".

CO: I don't think that my use of these concepts is in any way idiosyncratic or mysterious. It is even hardly original I use the idea of "contradiction" as a heuristic: goal oriented, presumably "rational" social action which complies with the rules governing a particular institutional domain leads to outcomes that are not just "unanticipated" (as in Merton), but positively counter-intentional and as such preclude the continuity of such action. The "tragedy of the commons" is a case in point, as is the failure of beneficial collective action (as analyzed by many authors from David Hume to Mancur Olson). Marx's theorem of the tendency of the rate of profit to fall follows the same logic. As a consequence, stability and sustainability of the institution in question is being challenged. I found the sequence of events where outcomes are diametrically opposed to intentions useful as a model that allows the analysis of, among other things, welfare states and the monetary regime of the European Union. A synonym of "contradictory" is "self-defeating", as in Michels' "iron law" according to which mass democracy results in the formation of oligarchic parties. Albert Hirschman has analyzed the "rhetoric of reaction", versions of which are commonplace on the political right as well as the left. Karl Deutsch has proposed to define power as the privilege of not having to learn, which makes power holders dumb and their power fragile, as we can see in the fates of state socialism.

A crisis in my understanding is simply the accumulated results of such contradictions, a point at which it becomes an open question and a decision is due on

whether a society can maintain and defend its institutional patterns or must face the consequences of their breakdown. For instance, the capitalist class and its state has so successfully resisted wage increases that it now faces an underconsumption crisis where there is not sufficient demand to actually sell what has been produced.

I read in the early 1970s something that perhaps unduly impressed me, something on the theory of social work, an American author. His question was: What is the aim of social work? And the answer he proposed was to enable them to practice fate-control, the plain opposite of which is the contradiction of addictive behavior.

LB: Fate control?

CO: Yes, fate control. And I think it is a nice term what capitalist democracies cannot do really, practice fate-control in the sense of "being masters in their own house" and being able to control the critical contingencies to which they are exposed, thereby enforcing their own stability and identity and avoiding the non-linear course of ever new unpleasant surprises and self-inflicted damages.

LB: One alternative way of thinking about this non-linearity of social change is that crisis might be, at least in the best of the worlds, functional for social change, for social learning. In the Soviet Studies Jadwiga Staniszkis had this concept of "regulation by crisis", that is that the socialist regimes are run by one crisis to another, learning a little now and then, or Philippe [Schmitter], our common friend, had this neo-functionalist idea that crisis might be giving birth to something positive.

CO: Yes, the neo-functionalist message that crises stimulates learning. In democratic theory you have the similar idea that party competition generates an ongoing learning process. I am not so sure that this is always the case. Sometimes crisis is of such a nature that it kills the forces of reason and cooperation and destroys learning capacity. The functionalist theory of European integration, the theory that crises necessitate remedial institutional innovation, does not have much support in present-day European realities. That theory is too optimistic, I don't think it always works this way.

In view of the demise of European state socialism at the end of the 1980s it occurred to me that Soviet-style state socialism (so-called "scientific" socialism) fell victim to one fundamental defect: the lack of capacity for self-observation and self-monitoring, which is a precondition for the capacity for self-correction. Mechanisms for self-monitoring are an independent media system, the division of powers, an independent judiciary, autonomous social science – a number of subsystems that watch each other The system lacked early warning mechanisms and the readiness to respond to their signals. I don't think it is axiomatic that economies and polities of the OECD world are adequately equipped with such mechanisms, which is to say: an analogous collapse is not to be excluded as a possible outcome of crisis.

LB: To me it seems that you and Luhmann are two completely diametrical opposing worlds, that is the world of Luhmann is nearly autopoetic, quiet and nearly machine-like working world, and yours is full of contradictions, and crisis and conflict.

CO: Luhmann had the idea of autopoetic self-referential sub-systems rotating in their own codes. The economic system, the political system, the legal system, education system, they all have their own codes, they put constraints upon each other, but they are on the same level, and that is very contrary to any kind of Frankfurt thinking. Luhmann does not allow for any dominant mechanism that radiates into everything else, such as class conflict in the Marxist tradition or the overpowering and homogenizing force of "culture industry" plus administration in Adorno. The concept of "totality" refers to a dominant force that penetrates everything and that can only be resisted by the only thing that is possibly external to it, namely autonomous art. The self-centered work of composers and other artists is what is capable to oppose the totality of all-inclusive late capitalist domination. By implication, that means that other forms of opposition are irrelevant and illusory. That is how I see Adorno, and that has certainly been an influence.

LB: I would like to shift to the third topic that I wanted to discuss with you and that is your perspective on Europe. And not just about your new book. You started actually mentioning how you originally were not very much interested in the topic. What was your view, what made your skepticism about EU or integration?

CO: Well, in the mid-1990s it used to be popular among intellectuals to say that if Europe were a state, its democratic qualities would not be sufficient to be admitted to the European Union.

LB: Why?

CO: Because the EU polity is not based on a competitive party system, has no proper constitution, and lacks the idea of self-recognized citizenship of a "European people". We are citizens of Europe, but we do not think of ourselves as [European] citizens. To be sure, there is also a European parliament whatever its limits are. There is also the pressure of evident needs to cooperation beyond nation-states.

On the other hand, globalization (of finance and security, of trade and cognitive culture, of climate and crises) means that the idea of the autarchy and autonomy of nation states enclosed in their borders is largely obsolete anyway. Wherever we look, we see the evidence of long-distance causation. If the Trump administration were to turn to a policy of economic protectionism that move would immediately endanger tens of thousands of German jobs.

Take one example that I've recently read about, the consumption of cotton textiles, of which the average American buys sixty pieces a year. This pattern of con-

sumption is the direct cause of a huge soil erosion in Uzbekistan which is a major ecological problem there. It also causes the breakdown of the textile industry in West African states. For instance, in Zambia you see hundreds of people in the streets wearing T-shirts printed with YALE or the name of some college. These are "donations" arriving from the US by shiploads and sold for $1 in the second-hand market, thus killing the local textile industry.

Another example is, there is a number of diseases – Malaria not the only one but the most prominent one – that could easily be eliminated according to what I read about this issue. But pharmaceutical companies do not think it is worthwhile because in the places it is most needed the ability to pay for it is too limited. Here, the long-distance causation starts with inaction, or failure to act. You could well do something, but you don't because it is not profitable. I think the way to achieve the measure of "fate-control" is through supra-national integration, cooperation and regulation. The same applies to cooperation gains to be reaped by European integration, both at the level of markets and the level of policies. The huge problem, however, remains that companies know how to profit from border-crossing cooperation at the company level and through market integration, whereas national policy-makers are largely caught in their national horizon and the institutional framework of national democratic politics.

Interdependency requires joint management. I think there are encouraging indications in the EU that this is being slowly and haltingly understood, and not just concerning economic and financial issues such as the banking union. It also applies to the normative foundations of liberal democracy in Europe where not just the Commission, but also numerous civil society forces and member state governments aspire to perform an effective supervisory function concerning the enforcement of rule of law, media freedom, and academic freedom in all member states. For instance, in the spring of 2017 more than 40,000 people signed a protest resolution within a week after the intention of the Hungarian government became evident to close down the CEU. It remains to be seen to what extent national political issues can be effectively politicized in an EU-wide scope, and to what extent issues of social and economic policy making can also be taken up as items to be prioritized on a European (or at least Eurozone) agenda.

LB: You think of federalism?

CO: Yes, we need a kind of European federation which is, no doubt, enormously difficult to establish. We do not have the to get an institutional method to get it started. The American Revolution was a constitutional coup. The founding fathers made decisions in Philadelphia on matters on which they had no mandate to decide – and got away with it. You cannot do these kind of coups anymore. Any attempt to

emulate the US example would be rightly seen as an impossible bootstrapping act: The act to create a unity out of component elements that are not disposed to and cannot be forced to form such a unity.

LB: When I was studying political sociology, we were reading your and Jürgen Habermas' pieces in German, that was for us in Hungary political sociology in the early 1980s: crisis theories, *Spätkapitalismus*, and other similar things. Then, after the regime change, the debates about post-communist diversity, also some of your works on these dilemmas were very influential.

Europe Entrapped is very much in that line, thinking about contradictions, thinking about crisis. What really makes a difference, and I was wondering what is behind that, is that in this new book on Europe you put much more stress on suggesting a way out than in your previous work.

CO: Stressing crisis is perhaps a typical German inclination that can be denounced as a tic of intellectuals of my generation. On the other hand, and as I said before, the observation and explanation of things that "go wrong", of systematic irrationalities and counter-intentional outcomes is a core component of what exemplary social scientists have been doing, perhaps even the mandate of social science. But fulfilling that mandate does not categorically absolve them, in my view, from the task of exploring and charting the realm of the possible.

To enjoy observing and describing processes of self-destruction, to take pleasure and inspiration from doing so, is a somewhat immature intellectual attitude that many of my generation had indulged in for a while. It can amount to a kind of self-righteous *Schadenfreude*, a shallow radicalism that mistakes itself for a deep insight. Out of fear to appear naïve and blue-eyed, you conjure the inevitable doom of capitalism or, for that matter, European integration. Yet in my view it would be more helpful, as well as intellectually more demanding, to specify conditions under which doomsday scenarios can be avoided. There is always a fine line between wishful thinking and thoughtful wishing, and that also applies to a scenario in which the EU overcomes its crisis by mobilizing endogenous learning processes.

LB: How isolated do you feel in this approach, meaning on European integration? Who would you think now in European intellectual circles might be allies, or you see as thinking in a similar way?

CO: I think quite a few people. Of course, it is easy to be utopian. Paul Mason had the other day the "Option Six" added to the Commission White Paper.[1] And this is about a European welfare state with very solid foundations, and constitutions,

[1] "Commission White Paper Option Six: A Europe Of Democracy And Social Justice", available at: https://www.socialeurope.eu/option-six-a-europe-of-democracy-and-social-justice

and political forces. So the revival of social democracy on a European scale. I am not aware of any compelling impossibility theorem regarding such a way out, unlikely as it may appear today.

LB: That has very long traditions, it goes back to the Delors Commission?

CO: Sure, or even further, I mean the *Ventotene Manifesto* of 1941, put on paper by [former] communists such as Rossi and Spinelli, both incidentally brothers-in law of Albert Hirschman who much later came up with the idea of "possibilism" as a legitimate mission of the social sciences. As early as 1948, Hirschman foresaw a European monetary system in the context of the Marshall plan. There are a few amazing things to re-discover.

LB: But my question was: about intellectual allies in Europe?

CO: It is not so much individual politicians and public intellectuals whom I have in mind; it is rather the "objective" urgency of promoting a new start that is perceived by and imposes itself upon even quite mediocre minds. All of sudden it has become almost common sense among EU élites, in the first half of 2017, that a European treasury with taxing, borrowing and spending authority derived from a new Eurozone legislative body is an attractive idea. Similarly, it is widely understood (though not yet openly admitted) that the ban on debt mutualization in the Eurozone is plainly obsolete. Some mental roadblocks have been removed, and others may follow. Also, the presence of Trump and the presence of Putin may help to highlight the urgency of European élites getting their act together.

LB: What would be your list of the first three things to do?

CO: First, change the situation that is destroying the economies of the South, the olive-belt of Europe, as the British say. We must get rid of the Schäubles, and introduce debt-relief plus a major program for investment.

Second, we need to cope with the refugee crisis in a cooperative way, which also requires to force the Visegrad gang to behave in a civilized way, and not just in view of the refugee challenges.

Third, strengthen the "social dimension" at a European level, beginning with the Europeanization of unemployment insurance.

LB: To what extend is it realistic to expect these things to be done in the present political framework?

CO: The present political framework, if you refer to rightist populism and nationalism, it is a direct consequence of the qualitative and quantitative decline of social democracy. I mean, people do not get protection from state power, therefore they expect wrongly to get protection from state borders. Building fences, keeping everything out, foreign goods and foreign Gods out! My bottom line is always the decomposition or the decay of state capacity. We cannot coerce ourselves, as Europeans, to

do what is in the most direct, most obvious, most urgent interest of Europe. That is a contradiction: We lack the leadership capacity, the capacity to organize cooperation that is needed in order to address the most urgent and most obvious problems.

Looking back, we should have relied less on the grand coalitions and the conspiracy of silence about the real problems. Grand coalitions tend to bracket out the hard questions. And that is what we have seen now for eight years in Germany, and chances are that this is going to continue.

LB: Coming back to the broad question of reforming Europe, and this will be my last question, what do you think of the European polity? Can that be reformed, is that on the agenda?

CO: It must be. All knowledgeable people are fully aware that this needs to be done. But then, how to do it is the problem. And I think, the easiest ways to do it is to start from scratch, with the Eurozone rather than the entire EU. The present absurdity is that we have nineteen states without a central bank and a hyper-autonomous central bank, the ECB, without a state. The very term "debt" as applied to the monetary and fiscal conditions of Greece is a lie as it wrongly suggests that rescue transfers that have been made to that country can and will ever be paid back. It is a wide open secret that that "debt" must be written off as a loss, a giant loss.

LB: Last question: not too many people talk nowadays about the Ventotene ideas – the basic idea of the *Ventotene Manifesto* is federal polity: European level parties, European level competition, shared sovereignty, this kind of ideas.

CO: Yes, but I think there is still a federalist undercurrent in the European discussion. The key problem with all kinds of federalism is what is left to "subsidiarity" and what is to be centralized. That must be renegotiated. Many questions of social policy, fiscal policy, and economic policy must be moved up to the federal level. I realize that this is an enormous political challenge. While there is no reason to be euphoric about the prospects, both the disastrous economic facts and no less the emerging political forces of anti-European mobilization will teach élites what needs to be done and what to be prevented.

References

Adorno, T.W.

1951 *Minima Moralia. Reflexionen aus dem beschädigten Leben.* Frankfurt am Main: Suhrkamp.

Habermas, H.

1962 *Strukturwandel der Öffentlichkeit. Untersuchungen zu einer Kategorie der bürgerlichen Gesellschaft*. Frankfurt am Main.: Suhrkamp.

Lukács, G.

1923 *Geschichte und Klassenbewußtsein. Studien über marxistische Dialektik* [Eng: *History and Class Consciousness: Studies in Marxist Dialectics*]. Berlin: Malik-Verlag.

Mitsch, W., Gerhardt, U., Offe, C., Preuss, U.K.

1965 *Hochschule in der Demokratie. Kritische Beiträge zur Erbschaft und Reform der deutschen Universität.* München: H. Luchterhand.

Offe, C.

2015 *Europe Entrapped*. Cambridge UK: Polity Press.

Theorizing Crises and Charting the Realm of the Possible

Claus Offe in Conversation with Laszlo Bruszt

Abstract: In this conversation with Laszlo Bruszt, sociologist Claus Offe discusses the key moments of his personal intellectual education and the main aspects of his scientific work. Starting from a peculiar interest in musicology, Offe reconverts to social sciences in the 1960s while getting involved in students activism with the *German Socialist Students* (SDS) and later becoming teaching assistant under the supervision of the young Habermas. Retracing his intellectual trajectory from the early works on education and democracy, it is possible to pinpoint the specific critical sociological route elaborated by Offe focussing on the macro-sociological analysis of systemic contradictions and crises reciprocally affecting contemporary capitalism and democracy. This perspective is then particularly suited to discuss one of the most salient political problems of our time like the crisis of the European political union and the possible reforms for its internal democratization, with an emphasis on its capability to attain integration through social justice. The interview concludes with a critical rediscovery of the arguments of the *Ventotene Manifesto* to overcome the key challenges hindering the realization of the European project.

Keywords: Frankfurt School; Habermas; Contradictions; Crises; European Integration; Intellectual Trajectory.

Claus Offe was (until his retirement in 2005) Professor of Political Science at Humboldt University, Berlin, where he held a chair of Political Sociology and Social Policy. He earned his PhD (Dr. rer. pol.) at the University of Frankfurt (1968) and his *Habilitation* at the University of Constance. From 2006 to 2013 he taught at the Hertie School of Governance where he held a chair of Political Sociology. Previous positions include professorships at the Universities of Bielefeld and Bremen, where he has served as director of the Center of Social Policy Research. He was awarded an honorary degree by the Australian National University in 2007 and another one by the New York University Tirana in 2015. Since 2016, he is Nonresident Permanent Fellow at the Institute of Human Sciences in Vienna. His fields of research include democratic theory, transition studies, EU integration, and welfare state and labor market studies. He has published numerous articles and book chapters in these fields, a selection of which is reprinted as *Herausforderungen der Demokratie. Zur Integrations- und Leistungsfähigkeit politischer Institutione*n [2003]. Book publications in English include *Varieties of Transition* [1996], *Modernity and the State: East and West* [1996], *Institutional Design in Post-Communist Societies* [1998, with J. Elster and U.K. Preuß], *Reflections on America. Tocqueville, Weber, und Adorno in the United States* [2006] *Citizens in Europe* [2016, with U.K. Preuß] and *Europe Entrapped* [2015].

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Craig Calhoun, Riccardo Emilio Chesta

Sociology and Its Public. Craig Calhoun in Conversation with Riccardo Emilio Chesta.

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Interview

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Craig Calhoun in Conversation with Riccardo Emilio Chesta

by Craig Calhoun *and* Riccardo Emilio Chesta

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1. Introduction

In this interview, Craig Calhoun retraces his peculiar trajectory that, from his first interests in realist cinema and anthropology, brought him to become one of the most original contemporary public sociologists.

Discussing his multidisciplinary education between Columbia, Manchester and Oxford he reassembles his own particular intellectual path that mixes his youth activism in the peace movement and engagement in the Western Marxist debate of the 1970s, while at the same time studying Hegel, German classic philosophy and Continental social theorists.

But it is through these apparent digressions that his peculiar way of practicing social research took shape. From his first anthropological inquiries on the Tallensi in Ghana and on the historical sociology of working class movements, Calhoun's incursions into different disciplinary fields do indeed constitute his critical approach to social sciences that always tries to trace back the specific and contextual outcomes of empirical research to general historical-sociological dynamics.

In Calhoun's view, realist cinema and the art of documentary are not simple occasional curiosities or forms of escapes, but a way to observe and analyze reality common to the sociological gaze. The art of constructing objects of investigation as it is practiced in the documentary films of Frederik Wiseman is in this sense comparable to the act of choosing the lens as theorized in the sociology of Pierre Bourdieu, the

two being similar moments of constructing social objects of study in their relative fields.

It is particularly in the British academic context that Calhoun encounters Bourdieu's theory of practices and thanks to which he composed his particular way of doing sociology that mixes the European critical tradition, British social anthropology, and American empirical research.

But far from being a linear and coherent strategy, this interview also shows how intellectual life is made of serendipity and unexpected encounters. In a telling anecdote, the discovery of Bourdieu's *Outline of a Theory of Practice* [1972] by the student Calhoun is described in terms of an intellectual revelation at a train station bookshop in Manchester.

Moreover, during the conversation Calhoun outlines how, since the first researches on Nineteenth-century working-class radicalism, he has tried to put together elements of Habermasian theory of the public sphere with Bourdieusian theory of fields, American sociology of social movements with Thompson's moral economy, in his project to reconstruct a culturally and historically grounded sociology.

In this interview Calhoun identifies the requirements for a critical sociology that, avoiding to be reduced to an abstract theorizing or a new academic specialization, can fulfill its role of scientific discipline and public utility. Fragmentation in sub-disciplines and increasingly atomistic analysis, as well as the diffusion of a "star system" and a mediatic intellectual, are indeed two opposite symptoms of a crisis in the professional scope and public mission of contemporary sociology.

Finally, without reducing social sciences to a mere form of academic expertise or a mediatic discourse among the others, Calhoun here outlines the idea of a sociological practice that should be able to critically and synthetically reflect on its own limits and possibilites, while applying its scientific instruments to the relevant social transformations of our time and to better inform the public.

2. Conversation

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Riccardo Emilio Chesta (REC): Let me start from a personal scientific question. What is interesting about your profile is that part of your educational training in cinema studies. You somehow described how your passion for documentaries, or realist films, shaped your sociological imagination. I would like to know how exactly it shaped your choice to become a sociologist.

Craig Calhoun (CC): I came to public sociology very early, interested in a way of exploring social issues that could be shared more widely than the usual sociological work was from early on. And then so, I would say, a path which I started with an interest in wider circulation of thinking about society became more conventionally academic and then returned to the enduring interest in a broader circulation. And then I am interested in documentary and realistic cinema.

I am also interested in more fictional work and the interplay between imaginative work and empirically-grounded descriptive work. And getting something on the relationship between science fiction accounts of dramatic changes of artificial intelligence and the actual corrections of what's going on. Not just that they predicted it but how did they influence. What was the relationship between the producers of artificial intelligence and science fiction? And how much does doing something creative depend on being able to imagine something that doesn't exist? As well as documentaries did. My engagement in cinema led me directly to anthropology and only indirectly to sociology.

There's a stronger tradition of ethnographic films such us documentary films in anthropology, the media and at the interplay between the two than there is in sociology. And the fact that most of anthropology has focused on the exotic distant, others encourage thinking about individually as well as analytically. I think that visual sensibility is good. It's not just that we should have films of sociological questions, is that we should do work that enhances a visual understanding, a visual source of knowledge. And that is part of not restricting ourself to what is expressed in words or in statistics.

That's also in relation to Bourdieu and his critique of the scholastic fallacy. Not merely a technique for communicating, but a technique for watching and listening and getting outside of the projection of academic ways of seeing the world into everybody. So, the kind of documentary films that particularly interested me involved lots of watching and listening. Shooting many more feet of film that you actually edited into the final film because you were recording what was going on to be able to see and to situate.

Frederick Wiseman often started by going around, hanging out, in a fact doing ethnography but with cameras, without filming them at the beginning, partly just to get everybody used to the camera in the room, partly as he was coming to see what he wanted to film. That is a very important moment in sociology that is not given enough attention, the moment of constructing the object of study, of figuring out what you want to look at, of choosing your lens literally in the sense of cinema but metaphorically in sociology.

People tend to discard that the standard canons of the scientific methods of research do not pay much attention to public formation. They tend that has to be as-

sumed and they ask what definition, what method, what theory... But actually problem formation is probably the most basic and it is also crucial to this project of a more public sociology that we think about that from the time of problem formation, that we not let just a question of communication after we have done a research, but that is part of how we think what is significant to study.

REC: In this case you address a critique to the proliferation of methodologies, technical gadgets in contemporary social research because basically they miss the ontological part, the premise, or the idea that the method is used depending on the way you decide to look at the object. This idea of constructing the object and the act of choosing the lens is clearly part of the epistemology of Bachelard and Canguilhem, and ultimately the foundations of Bourdieu's sociology. How did you basically decide to reconvert to the sociological field? Which authors were inspiring you?

CC: I'll answer the question, but let me first quibble with the question. You imply a completely intellectual transformation influenced by great theorists, and I was. But this was happening biographically in the course of my life and so the influences are not all just abstract or intellectual things.

There are many things when you ask about cinema. Well, if I'd been incredibly talented in it, I might have been doing that! [*Laugh*] But I was more talented in the social sciences than at the employment of cinema. So the cause is one of being interested in engaging the nature of the social world and how it works in a political and social change perspective on it. Of course I went to university in 1969, so it's in the later part of the 1960s movements and all the things that were going on, the peace movement and everything else.

The idea of social engagement is not anything distinctive to me. It was in the air, I was active in student politics and, as you said, with films. I fell in love very quickly with anthropology, and it was really an experience of falling in love. Taking a class, and just being enthusiastic, like "Yes, this is how I see the world," and my harnessing... my eager desire to see the world, to understand more through anthropology. There could have been something else I could have fallen in love with, but it was a combination of having a great first exposure to it, with opportunities to do more things in it and how it fit with my pre-existing dispositions and desires.

But it was very much anthropology, not sociology. I have always maintained that the differences between the two are more differences of style than substance, they could be much more closely related and in my formation they are very closely related. So I very strongly engaged with anthropology but with a specific version of anthropology – sometimes called British social anthropology – and also with social theory.

So most anthropology at this time, did not engage that much with social theory in a more philosophical end of things which also interested me. I continued with anthropology but became more interested in social theory. What drew me to sociology was partly this interest in social theory, including anthropological theory that has been able to explore a wider part of social theory. As it happens, that transit has not been as big a part of sociology as I would wish, so it was a rather specific angle to it and it was driving me also to philosophy.

The same interest in widely-understood theory would draw me to read Hegel and attend the lectures of Charles Taylor, that would draw me also to some parts of sociology and I was very much formed by some features of anthropology, like trying to look at the interrelationships of all the different aspects of the social contexts, so holism in the classic ethnographies, which I critique a lot but that is something powerful about that idea to see all the different connections. The comparative moment, which most anthropologists and most sociologists don't do, trying to look across and ask therefore why should it be this way, why should Europe and the US being different, why should Africa or this African society or that group within African society be different. So, from early on I was interested in that issue of similarities and differences across settings.

I was also interested in history, so I would describe my formation as being interested in cinema, in writing, anthropology, sociology, philosophy, history... rather promiscuously moving around these different fields. I've had a hard time settling on one of them. The only one in the beginning that I just identified as the one I really wanted to follow was anthropology but I kept moving around and trying to connect all of them in a way all through my career.

The kind of anthropology that I was drawn to – the teachers, the early big influence on me and then Max Gluckman – represents the kind of anthropology that is not done very much any more! So in a way anthropology moved in a different direction, and I was more able to continue the same interests in sociology and in historical interests.

How I got to this involved physical moves. I went to Columbia University but then I went to England. This exposed me to different things and including the extent to which, as similar as England and America are, there were differences in what was going on... differences in social movements and fields. So – as in the US before – I got very involved in England in the Peace Movement. That was a deep commitment for me. The Anti-War Movement, the Peace Movement.

I would not have described myself as Marxist. I went to England and I got involved more in socialism and marxism and it changed my identification, this affected my intellectual life as well. It's not just I read Marx and I understood it. I got engaged

in the question of "What produces a social transformation?" and the subordinate question: "Why is it that huge movements, where people seem to be passionate, can fade away?"

So, part of my experience of the 1960s is the end of the 1960s. That as enormous as the social movements, and the participation, and the cultural change, and the demonstrations of the decade are, they fairly rapidly fade away and we wind up with a growing conservative mood and some people changed their affiliations, some stopped being active. That made me interested in looking at other historical movements and the way the change worked.

Something that shaped all the work that I've been interested in is trying to combine a historical perspective with a sociological perspective. Then, I found a sort of métier at that point, shaped by an engagement in social movements and public life and shaped by the move to England and the project of understanding something to which I was not quite native.

REC: So, apart from this early period of engagement in the Peace Movement, would you say that you were basically becoming a sociologist in a period of disengagement?

CC: Or a period of great engagement! I would say a period of disenchantment with established institutions but a great deal of social engagement, a very passionate social engagement, and I was very passionate about learning all of this. I felt I didn't have all the education I eagerly wanted to get. So, reading classical social theory like Durkheim, Weber, Levi-Strauss, Adams Pritchard... I was very energetically appropriating them in an intellectual training for myself, which meant that I was a sort of autodidact; that had great teachers, but that my training was not guided by the sociology curriculum – by what you should know in sociology – or the anthropology curriculum, or the philosophy curriculum.

I read huge amounts of each. I was able to go to classes with some of the great figures in each but I didn't follow standard course study. I was pulling together things with one thing that would have taken me to another. So, my reading of Marx, in England, reading all *Das Capital* very carefully, early Marx, then reading Marxist debates, and debates that extended into non-Marxist tour having dealt with Marx.

The 1970s was a time when Marxism organized in an interdisciplinary intellectual debate as well as being related to political movements. Some people were strong scientific Marxists. Some were humanists, reading early Marx. Intellectually it was a very stimulating world. In this context, where Marx was the big guiding influence, I read Kant and Hegel, and a variety of full works, and modern figures like Charles Taylor or Habermas.

So, through the lens of trying to have an understanding of social action, social change and the durability of social structures in the real world, I was reading everything I could to try to understand this academically but I came to Habermas and to Bourdieu through that context of reading Marx.

I remember buying *Outline of a Theory of Practice* [Bourdieu 1972] at a bookshop next to a train station in Manchester as I was about to take the train back to Oxford, actually starting reading it in the bookshop, sitting on the floor by the B section of the books and thinking "Oh, this is fantastic! This is exactly what I wanted to read all the way to the train!" staying up late at night, reading it the next day. This immediate sense of recognition and enthusiasm.

Now, what was Bourdieu putting together? Marx, classical anthropology, and interests in somewhat historical perspectives, change and confrontation between different cultures. The same things I was interested in. But also, a year or two before, I had encountered E.P. Thompson who wrote the great book *The Making of the English Working-Class* [Thompson, 1963] which shaped me a lot, a book of Anti-Theory. Thompson hated theory. But it was an account that I found enormously engaging.

I ultimately decided in an important sense that it was wrong, and wrote my first book in a way against it. But it was a huge shaping influence and the social history of that time, which for many others was more theorized history – for Hobsbawm among others. Thompson gave me a sort of *problematique* in a French structural sense. A sense of the problem to the stance. So, relating my own social movements engagement to them... here is movement in action, the formation of a hugely influential movement, a change in history. If I can understand this more deeply, I would understand a lot.

I just pursued that more and more, not just reading Thompson but studying the whole 1790s-1830s in England and trying to get that deeply, following the anthropological intuitions and Bourdieu's argument that you should try to understand one thing really well and let that shape how you read theory, how you pursue theory. So that was what I focused on, trying to understand as I went out and read philosophy or social theory or other things and try to see if they fit, if they helped me understand that.

REC: It is puzzling to me that someone like you, engaging with social movements, not only personally but also scientifically, then suddenly moves to an author that has been criticized – surely in a wrong way – until the 1990s for being focussed too much on structures rather than on agency or social change.

CC: That is true but there are not just two choices. That is, Althusser or Poulantzas were much more extremely structuralists than Bourdieu, and I was searching for something more middle-ground. So, the way I presented and read Bourdieu emphasizes the extent to which he was seeking the dialectic of objectification and

subjectification. Partly because that's what I wanted in it but it was throughout seeking that. It is a misreading to read it just as a structuralist. Though at some points he's almost just a structuralist but he also tries to do other things.

I didn't just become a Bourdieusian in a sense that I said "Oh, he was my favorite theorist!" I never quite liked people who say "I'm just a follower of one theory, I have my favorite theorist" and so on. So, to me Bourdieu was already compensatory to some readings of Marx. If I was most immersed in Marxism when I discovered Bourdieu, then Bourdieu opened up something in relation to Marx, including "embodiment", so that I wanted to think about agency and I wanted to think about movements I wanted to study. Or in "embodied knowledge", in things that people don't understand.

I was interested in how the sensibility of a large number of workers could be shaped when only a small number of them were doing anything like theory and the rest Marxist followers of theorists were engaged in something else. That was right, but they did have some understanding, some consciousness. My analysis in the end, somehow helped by Bourdieu, somehow by something else, had to do with attachment and how people struggled while being attached to their communities, to their crafts, their lives, and having them disrupted and destroyed by the coming of industrial capitalism.

So, very much in keeping with Bourdieu's interest in how the coming of the French colonial state and market disrupted the life of the Kabyle peasants. I was working on British craftsmen, English craftworkers for the most part, and how they experienced this. And this led me to a disagreement with Thompson and a partial disagreement with Marxism, although Marx also saw some of the same things, which was that these craftworkers were fighting to avoid becoming proletarians. They were not the proletariat fighting against the capital. They were fighting not to be forced into being capitalist proletariat, to maintain their artisanal societies and those local communities, and the resources in the struggle were in part the strength of their local communities, not a national class organization. That, I argue, will come much later.

So, in a sense I argue that Thompson was actually wrong. It was a fascinating book, but it was not the making of the English working class, it was the resistance of the English *artisanat*. That was influenced by Bourdieu's look at the Kabyle. The Kabyle were not fighting a Marxist revolution against France but they wanted to be Kabyle, to have some additional opportunities but without giving up their values in order to have these opportunities and then wind up at the bottom of the social order.

That was what connected me to Bourdieu. And what connected me at the same time to Hegel, and Kant. And the side of Marx and the early Marx that is in a way

most interested in "the social" and people's attachments and connections. So, there's this route that goes from Scottish political economy to Hegel and Marx back into other studies where an understanding of the dialectical fit of different parts of society together, the whole of its parts, is extremely influential. So my interest has for the same reason that I found mechanical scientific Marxism frustrating and actually wrong in important ways about movements.

REC: What really strikes me about this narrative is how your interests were not oriented toward a very specific scientific niche or specific sub-discipline. You move among different disciplinary sets (anthropology, history, sociology, social theory) but you also work empirically on different topics.

Let me pose you a question about the contemporary state of social sciences, in which the growing differentiation and specialization has of course some positives, since it creates more accurate knowledge with new cases and empirical evidences, but on the other hand it also produces the potential risk of a growing specific ignorance.

The fact is that we risk no longer being able to address issues or questions that have general importance. This is crucial when observed from the perspective of a young researcher. Nowadays academic constraints bring us to specialize even more and to have a specific and narrow technology of research that brings us to cut our sociological objects into always smaller pieces.

I would ask you how you think we can combine the exigences of an empirically and rigorously grounded research with the necessity of a broader attention to general sociological analysis in these specifically contemporary academic constraints.

CC: I'm not very disciplined by disciplines, but I have been enthusiastically interested in a variety of things from different disciplines and I have felt entitled to put them together the way I wanted. I won't say competent because sometimes my feeling has been that "I'm not competent."

I remember going to the first week of Charles Taylor's seminar on Hegel at Oxford and leaving with the conclusion that I simply can't do this! [*Laugh*]. Because I was basically a young Marxist with an interest in history, sociology and anthropology who wanted to know more about Hegel mainly because it seemed to be a precursor to Marx.

There is this intense discussion about the relationship of Greek philosophy to Hegel's Philosophy of Right. And there are two problems. One: I don't know much about Greek philosophy. Two: the class following Taylor owns fantastic linguistic abilities shifts so everybody talks in English, even about things I don't understand, then they seem to speaking in German, then they're speaking about the translation of Greek terms into German and I say: "Ok, I didn't have a Jesuit education!" I thought: "I can't do this!".

My response was to read as intensively as I could, mainly in English. I still don't know Greek and I still do not read in German. So I did the best I could with translations but I said: "Ok, I have to educate myself because I wanted to understand instead of leaving the class." But in order to do this I have to know a lot more. So I have to read Aristotle, because it was about Aristotle. I read Aristotle before Plato, because that was the way I found it, and that fits with Bourdieu, that fits with other things.

Since then, I have read Plato, and I've tried to fill the missing gap, so my experience is of reaching out from what little bit I knew these new things then each time discovering that in order to understand that I had to understand a hundred other things. So you read Marx, Hegel, Aristotle... and you're drawn into that. I never had a systematic education and maybe it's a fantasy that everybody else does. In the sense which I read Greek philosophy and then the History of Christianity, the Middle Ages, the Renaissance, I didn't actually learn it that way. I read a few scattered things like that as a student in a World Civilizations classes but then my main experiences in having to read these things was because each thing that is read opened up new things I wanted to know, and if I didn't find the thing I was reading to be very good, I put it down and I didn't follow it up.

If I found things that I was reading stimulating then I wanted to read the other things that went into it and it turns out that, in the case of Marx, that Hegel was really worth reading, not in fact, Dühring. There are a variety of figures in Marxism who are not such great thinkers.

Proudhon, on the other hand, turns out to be very interesting. Not right, not better, not as serious a theorist as Marx, but a representative of a kind of failed tradition. So there's a tradition that is very important in the real history of social movements that you can see with Proudhon or a few others that has no great academic figure. Sorel is probably the closest to a great academic figure. But the history of a kind of somewhat populist, some kind of syndicalist, a much more grounded part of the European workers movements has no great thinker. It's not Durkheim, it's not Weber, it's not Marx, no one who gets in the canon represents that. The Utopian Socialists. There are these very interesting thinkers who don't make the mainstream.

I've not forgotten that part of your question. By going out from the thinkers who are in the mainstream, who are great, in some way Marx isn't excluded from the US mainstream. He's clearly part of the intellectual mainstream. You find others who are great and that you want to read, who are part of the classic canon, and then you have to fill in the pieces you're missing, but then you find things that never make it into that classic canon. That's interesting. Why don't they? They may have ideas that are very important even if they did not do the great work that some of the others did.

My experience of this, moving out of anthropology, from films and other things into doing this, is that I kept being exposed to gaps in my own knowledge that I tried to fill incompletely. I had very little sense of a coherent narrative of the formation of this. It was not until I began to teach that I began to think about "This is the German tradition, this is the French..." I was searching to fill gaps and I was searching outward from some texts into what they cited until I began to teach the History of Social Thought, at which point it was for students who were beginning to be social scientists and it was important for them to get the sense of where things went, more than I had myself.

So I felt that I knew a lot about these thinkers but in a disorganized fashion, with an unclear sense of that. So I spent a lot of time then as a young teacher, trying to improve my map, my genealogy of my understanding of how these things fit together, and sometimes still reading once I missed the things I have. One of the ways I've been helpful to students as a teacher sometimes, is by helping them to see that overview, how these things fit together. So, more than teaching, if I am teaching Bourdieu, I'm completely Bourdieusian, but if I'm teaching Habermas I'm being Habermasian. And I can tell you that there are things I like about Habermas and things I don't, things I like about Bourdieu and things I don't, and we can go on.

I have my favorites but my role as a teacher is in part to help students see how much there is out there which could be helpful to them and to see how these things connect to each other in an intellectual field and in a history of formation of thought. But that came later for me. The original catharsis, the original enthusiasm is for one after the other and then having to spend some time thinking of this.

Once, when I took Charles Taylor's class on Hegel, I had to spend the whole semester reading things that influenced Hegel in order to follow the class. Lots of things I first read, like Aristotle's *Ethica Nichomacea,* I read first to understand Hegel's interpretation of the *Ethica Nichomacea.* So, out of order, it should be backward... and then again, later, in teaching, what I thought in a US course on Western Civilization was that I needed to figure out how the Pre-Socratic tradition related to Plato and Plato to Aristotle.

REC: Would you perceive yourself as an outsider or did you come to perceive yourself as an outsider?

CC: In a strange way, even though I was conscious of my limits, I often felt incompetent because I kept moving into discussions that I wanted to learn about that I didn't yet know. Many people are happy in conversations where they already know almost everything and they can feel satisfied with their knowledge, while I am happy with conversations where I don't know about and I learn a lot.

So I would move into a field that I didn't know about and I still do, like working on artificial intelligence now, as I am certainly not an expert. Moving to a field and asking questions, and learning, trying to read and to keeping up and making rapid progress, learning, even though at a price that I probably often look naïve at the beginning of a conversation. And that was how I put together my distinctive corpus. Eventually I found out that I did know something and the saving grace was that idea of knowing few empirical things well. Bourdieu writes exactly about that discipline.

For me first, I was set on course by Max Gluckman, one of my great teachers, who said exactly the same principle: "Alright, in addition to all of this theory eventually you will be old and you can only sit around to read. First, you should be doing something empirical." Really he meant doing fieldwork, but first, preparation for that. "I'm going to give you a topic," he said, "to write your thesis about Meyer Fortes and his studies of the Tallensi and you are gonna do a study of the Tallensi and you will know everything that is available to know, in print at least."

Eventually I went to Taliland[1] and I saw some things first hand, but mainly in print, about this one people in Africa. And I spent most of the year doing this. I wrote a very long thesis, which to my surprise was published. But the importance is not that we're great to know so much about the Tallensi, the importance is that I had something firmly in mind and when I read something I can say: "That makes sense with the Tallensi or not."

As I learn more, when I learn a lot about the British working class, or about countries I've lived in, I could say: "Does it make sense of this?" So there's always some empirical things that will allow me to say that I'm suspicious of this or that theory because it doesn't fit with what I know. Even if I know a little bit, at least it's a test, and then I can ask: "Is it true of someplace else but not of this place or is it not true at all?" So that's the way my thoughts worked with this small number of empirical anchors in this voracious reading of theoretical things.

On the way I became a sociologist, because sociology was the field that allowed me to do all of these best. Because you know, if you want to become a professional anthropologist you have different requirements, as well as if you want to become a professional philosopher you have different requirements, at the level of what you teach, how you do work. So I found sociology most congenial.

However, I also had an engagement in what was conceived as the sociological mainstream. I had the chance to study and work with some very distinct sociologists at Columbia like Robert Merton, Peter Blau and Robert Nisbet that represents three

[1] Ghana [Ed.]

different streams. Merton and Blau were certainly in the mainstream, in my later words. But it wasn't that I liked only the Marxist challengers and not the people who excluded them and constructed and come to the mainstream and left the Marxists out. I also thought that there was something interesting in that work and in this search for understanding the social against truly individualistic perspectives.

There's a lot offered by American mainstream sociology, it's a great intellectual tradition. Now, I only edited that book about it because it was the anniversary of that Association (the ASA – American Sociological Association) and the president asked me to, and the president happened to be Michael Burawoy, coincidently! I thought it had a lot to offer and the way in which that sort of central gravitational pole relates to the enormous diversity or plurality is really interesting and this is also the story of my academic life.

So I got a job in a very mainstream sociology department and moved on and I was able to be, and probably looked, to anyone younger than me in American sociology like a pretty mainstream figure at my age [*laugh*] but not very typical in my formation.

REC: Did you get a job in a mainstream sociology department because of your empirical research?

CC: I think it was only because my empirical research was, they thought, an attractive thing; that I had done some. But I don't think they read any of it. The job was described as a theory job and they wanted someone to teach the students theory classes – not to become theorists but just as part of the ritual of becoming a sociologist. You take some theory, some methods. And I was hired to teach theory since I was a more attractive theorist than someone doing just theory.

I think they were investing in empirical research and that I was also doing empirical research that was as good and they wanted to know that I could also teach theory. In an ironic sense I wasn't. I made myself competent to teach theory by trying to figure out how to teach theory. Realizing that I had to organize this as a class. Ironically, although I knew a lot about theories, I hadn't really thought through the organization of the material I began to prepare to teach.

I was always trying to also identify this and push historical sociology, and to some extent cultural sociology, and these fields that were not yet mainstream – they are more so now – and to make sociology in general, and sociological theory more integrated around the understanding of large-scale patterns in social change.

REC: So, this is the core of your engagement in creating a sociology that is either culturally and historically grounded, empirical and critical, addressing broadly general and publicly-relevant dynamics. This is visible in the creation of research institutions and networks like the IPK-Institute for Public Knowledge and the NYLON

you co-founded with Richard Sennett. Was it initially an effort to change the way sociology is practiced traditionally in mainstream departments?

CC: Yes, at a very immediate level, it was an effort to try to create the setting in which I could do the sociology I wanted to do and have my students do the sociology that I hoped they could do. So at one level it was not strategically designed to change all of sociology, but to change sociology enough to allow for the things I wanted to study.

But at another level of course it was, I had that ambition that work in historical sociology would transform the whole field and public sociology would change this. I don't think I was very strategic about it, like saying "Ok, if you have this, what should you do as a program." I was expressing tastes but also programmatically pushing the field a bit with others to share some of these orientations, with mixed successes.

I have institutionally though, even before the period you mention, been involved in it systematically at Chapel Hill. I had to create – and for a few years led – a program in Social Theory and Cross-Cultural Studies that linked sociologists with anthropologists and historians especially, but also a few people in other fields, like philosophers and communication scholars. So it was a project among young faculty members, most of them were assistant professors and got together across disciplinary boundaries and I was more the animator of this probably because I was more interdisciplinary, but also the others were interested in doing this, so there was a sensibility that was shared by the young teachers that we didn't want to be just conventional in our disciplines.

Later, in varying degrees, some of them became more conventional in their fields, and others retained that orientation. Many of those who retained that not so disciplinary orientation were pushed out, did not succeed in academia. Some of them did and have gone on to great successes. I always liked my discipline and all the others. It wasn't that I didn't like my discipline and I wanted the others but I wanted to be enriched by them. I wanted my students to learn things that were not just part of the standard. That relates to the question about specialization but also to my sense of critique.

One of the really important ideas in the critical theory tradition is that critique should not just be disagreement with something, saying what's bad about it. First, it should include the positive object of trying to establish truth as a sound basis for forward movements. The Kantian sense of critique is not against it, it is in order to understand the conditions of possibility – which carries always Bourdieu's sense of critical sociology – but second it should involve trying to understand other positions and how that fits.

I've always disliked people who just say "Oh, I'm Marxist, so I never read Weber" or "I'm a Bourdieusian, I never read Habermas." I just think that's anti-in-

tellectual, and unacceptable. I've always wanted to sort of engage this. Critique ought to be able to say what's useful and what's not useful in different perspectives. Move beyond them, but incorporate that, in a sense that Marx says that we never abandon a phase of history, we incorporate it.

In this sense we should be able to integrate what's useful from different perspectives, save why we think they don't do something well and work on that basis. With a specific object of understanding. Theories are not just good or bad. They're good or bad for understanding something. A great theory to understand Fordist factories will not turn out to be a great theory for either pre-industrial artisans or post-industrial computer software engineers.

You need to understand the theory in relation to an object of analysis and to be able to say what's good about it and incorporate that and what doesn't work in the new setting or what was mistaken even in the old setting. So, I have been not interested in being part of a school. What I have been interested in was in being able to promote a sensibility that would cross-cut schools. The irony is that critical sociology is one of the worst examples of having a narrowing insider discourse.

Critical sociology, all the critical social sciences in general, critical theory in the Frankfurt sense, all should have a broader orientation. So it's particularly paradoxical, ironic and disappointing that critical theory has become an insider intellectual game. It's so important to be potentially connected outside to empirical research and to public action.

So, when I think of other more or less contemporaries like Burawoy: he has an interesting vision of public sociology with which I partly agree and partly disagree. A very interesting personal work informed by Marxist theory, but he wears his Marxism pretty lively. It informs, it gives him ideas, but he goes after his specific objects and explanations or he pushes his students to them, in a sense. And there's not really a very synthetic theory of Burawoy in that.

At the opposite end, someone like Axel Honneth sometimes has very interesting agendas and pushes, and I'm very sympathetic and he's trying to bring Taylor into that critical discourse. But very much pushing towards a systemic alternative, like "here's my systemic theory as an alternative to that one." I like some of Honneth's work but stylistically I'm not very similar to Honneth.

I have found many things interesting about the Boltanski and Thévenot kind of work: understanding critique, understanding judgment, doing all of that. I don't think that what Boltanski has done – on his own and with Thévenot – in trying to produce a study of critique, as distinct from critical theory, is ultimately satisfying. It adds some interesting insights that have been used, just to say, by Michèle Lamont – that use that kind of work interestingly – but it doesn't do the same work. His studies

of humanitarian action or distance suffering are really interesting uses and insights to that, but I don't think of them as adding enough of a perspective in a strong sense, a real theoretical perspective.

Clearly, they all fit and deserve the label of being part of critical theory and in different ways as having engaged a broader public, which I want to say is exactly the fault of the insider game version of critical theory. Sometimes the engagement with broader publics is very incompletely connected to the deeper work. So, Burawoy's public sociology, Burawoy's Marxism, and Burawoy's ethnographic studies – he's an advocate for three things – I don't think there's any essential connection among the three things. They're all good things I'm sympathetic with – Marxism, ethnography and public sociology – but I don't think there's a strong internal correlation or connection in how they get done, possibly the fact that ethnography makes for better reading than quantitative work is connected to public sociology.

But then public sociology is untheorized in my sense. It's an expression of something with which I largely agree, but it doesn't include a very sophisticated understanding of the public or the discipline and what the pressures are against, and for, the public knowledge. In a sense, I agree more than I disagree but it doesn't give you any different understanding of Burawoy's work on the factory in Chicago or paint in socialism to know the public sociology. It's an addition, one more thing. That's probably true in some of my work too.

REC: On this important point, jumping onto another question, the idea of referring to a public relates to the issue of the sociologist as a public intellectual. There are different perceptions between US and European society concerning intellectuals. Of course, Bourdieu in France, Weber in Germany, Gramsci in Italy, Unamuno in Spain are related to diverse contexts and periods. However, I have the perception that in Europe the role of intellectuals is in crisis.

Firstly, there's a rise of anti-intellectualism linked to specific forms of populism – paradoxically, the term "intellectual" in itself was born as a right-wing populist insult addressed to specific thinkers during the *Dreyfus Affair.* But secondly, there's also an academic tendency to transform institutions that produce relevant knowledge in market-oriented machines.

This process is transforming the role of intellectuals into "experts" and triggering further processes of growing specialization, technicization, and probably neutralization of specific disciplines like, ultimately, also sociology and the social sciences. In your opinion, how are the social sciences affected by it? And how could it be done differently?

CC: Great question. It is crucial to do something now, the intuition is right, intellectuals are in crisis. The distinction between a "specialized intellectual" and

"general intellectual" doesn't fully help as much as I would like. It's not wrong, but simply there are other things to think about. But something that it points to is what is the place of research-based knowledge, knowledge that has some foundation in serious research, serious thinking. How do we distinguish that, how do we give authority compared to mere opinion?

In different languages, in very different traditions this issue is posed. So, for example, Habermas on the public sphere asks what is the difference between a rational, critical public sphere where reason is informed in public opinion and mere opinion or an administratively managed public sphere. This is an issue that gets discussed differently by Foucault and by other thinkers. But what is crucial now is that we have lost the authority that has most underpinned knowledge in the modern era.

People do not automatically assume that professors that have done research have authority and knowledge, so there are very weak institutions for granting authority. Since the 1960s a kind of anti-authoritarianism has been paramount, including on the Left, sometimes informed by romantic ideas of authenticity and autonomy, but nonetheless anti-authoritarianism. I think it has been pernicious. It doesn't mean we should have authoritarians, but it means that the defense against authoritarian governments is that there's some other basis for authority in society and if we destroy all of them, we get terrible consequences.

Public intellectuals are suffering in this, but are part of the problem too. I would love to see more renewal of public intellectual life but it would not be just a revival of individuals as stars. Part of what happened to the classic European idea of the intellectual is that it has become a star-system of iconic huge intellectuals. On a global scale, that created a politics of translation which decontextualize this. So you would know everything by Habermas and nothing by the people who Habermas was talking to in Germany. In some cases there were more systematic efforts to translate the larger fields, such as Thomas McCarthy's series on German Social Thought.

But in general, something got translated only in the first place because it was related to a star, and then it became even more star because of that. So, Derrida got a second life in France because he was a big theorist in America. This helps to undercut the intellectual fields of Europe by producing a mediatic star system of intellectuals. The recovery of a stronger intellectual discourse has to take more seriously the social, the collective intellectual fields, not just the iconic stars of these fields.

Now, to some extent, disciplines or interdisciplinary fields in academic life do some of that. They are more collective constructs. A weakness of the project of the public intellectual life has often been that it has celebrated the individual stars and left the socially organized. That wasn't always so. There are often times where there

are lots of political and literary magazines in which people are writing to inform each other and there's a more collective group.

Sometimes we remember groups like the *Collège de Sociologie* group and not the individual so much. But this is a problem. This made public intellectual life much more vulnerable to a takeover by a merely mediatic intellectual world and to criticisms that people were just giving their opinions and other opinions were as good, and losing sight of the fact that sometimes opinions can be grounded in serious work.

Universities contributed to this in an opposite way by first being so interested in the production of internal academic goods: journal articles, dissertations inside the fields, the proliferation of sub-fields. So, academic specialization meant that academic work did a less and less good job of informing the public, which left that to mediatic figures. There have been some efforts to reduce this, not just Burawoy in sociology, but various efforts, including my project at the Social Sciences Research Council focused a bit on this, or the Institute for Public Knowledge. Creating milieus of discussion which do not necessarily turn on star figures and allegiance to star figures which can use the media but are not just creatures of the media, to be picked up or dropped according to media fashion, but have their own intellectual coherence.

So, I will answer on this level very personally and then a little bit more generally. What I am proudest of in my course, are my students, not my publications, and I have always thought of myself in a way – first and foremost, just to say – as a teacher and organizer of academic work. I do of course love to think. I also produce good writings – I'm just not modest – all three of these are valuable and I want to call attention to the other two.

Particularly my Phd students; I'm very proud that they're doing a lot of things, they will do more than I could have done individually, collectively doing this, and they're always part of different discussions and different fields, and I agree or I'll disagree with things they will write. But, as an accomplishment – being able to help them launch their career, help them broaden their orientations, help them think beyond some of the narrow compartmentalization of much thought – that is what I would like best than what I have done.

Secondly, developing the IPK (Institute for Public Knowledge) or my earlier work in North Carolina, my continuing work now at Berggruen, I am trying to create institutions. This is becoming extremely important. It's extremely frustrating, so I like it least. I like teaching, I like researching more than I like doing this, but it's extremely important to do this because the old institutional settings for intellectual life that can inform public life are falling apart.

The media legacy, the newspaper publishers crisis is huge. Universities are in crisis, and universities have for decades been responding to increasing difficulties, but

on the one hand adopting the star system and on the other hand setting higher and higher standards for purely productivistic internal discussions and not resolving that contradiction. But also becoming more expensive, focused on the hierarchical status – and I say this as somebody who just spent years as being as a university president! Doing exactly that... I mean, in good Bourdieusian fashion, I know that you can't just break with the established role, you can do better or worse, you can't totally in this way or that way, but you can't say: "Ok, I'm not going to do all the things that the university president did, I'm not gonna raise money, I'm not going to hope that we have a higher ranking, I'm not going to hope to have more students."

You do those things but you're trying to shift the balance. So, saying that, there is a tyranny of the hierarchy for its own sake that all university presidents ask: "Well, have you moved up? You have been president for three years, has the university moved up or not?" Obviously you want to move up, not down. But it's so easy to lose sight that that is merely an indirect indicator. It's not the goal. And academia has made it harder and harder for it to contribute to intellectual life that can inform public life and we need to find ways to do it.

Now, people will still have jobs and careers in universities, although the crisis in employment is also real and many people are adjunct or temporary faculty and that's bad. But those jobs would not create in themselves the satisfying intellectual context. So, it's very important to try to create, in the media sense, the places – they will be digital or electronic places now, not conventional magazines – where people can have these discussions that are not totally dominated by capital or other instrumental goals, the settings in which people can come together – the meeting places, for activities which sometimes include long meetings. I've been a fellow of an institute for a sabbatical year and you're working with some unexpected people. So the Center for Advanced Studies in Behavioral Sciences in Stanford is a place where Thomas Kuhn met Robert Merton, where people who sometimes should know each other gets a chance to talk a lot. It's all within an élite, a very academic setting, but it's a way of doing some of this. So we need more invention of such institutions, somehow, to accomplish this**.**

There is no recovery of what we are nostalgic about simply through the celebration of some iconic individual stars. And there's no way to get there through academic specialization. We have to break out somehow and develop some other structures for that. The stars anymore would be mediatically structured. It would be somebody who has become the top popularizer in this. And to be a popularizer is a useful thing. This enables me to say a last thing about.

The reduction to experts is one kind of reduction. The reduction to experts and particularly in critical social sciences is a funny thing. I am sometimes called an

expert, but it's a funny thing. In what am I an expert on? It's different from being an expert on artificial intelligence, someone who may know about machine learning. The attempt to remake social science as *techné* in order to be powerful or influential with government is a problem. Economics has gone through this, in doing this, trying to present: "You, government, or citizens, have an economic problem, need us economists to give you a technical advice." But technical advice is only occasionally useful.

Economics is full of smart people and it has good knowledge but it does not give good technical advice because the problems are not just technical. Economics does its best, offering critical understanding of why a certain policy measure doesn't work or why people keep repeating that even though it doesn't work. Or what's going on in China and how the world will be different if China becomes the most powerful economy. So it's not that economics is not as useful. There's a misunderstanding to think it's a form of engineering, in which it will be useful the same way engineering is useful to making sure your bridges don't fall down. So it's better, or people in governmental policy have to have access to specialized economic knowledge. But it's a mistake to think that it is as technical as civil engineering contributions to bridges and even more, sociology or other fields.

There is a huge growth in technical expertise, largely in the corporate sector and not in public life. And one worry I have about public life is that there will be large parts of the world that will be inaccessible to it because of the proprietors of data, ownership and control of knowledge. So you have great, smart people at *Google*, *Microsoft* and *Apple* working on all these problems and their work won't really be public.

Now, that enables me to go to the second point which is about technicians and it's a contradiction in terms to say there's a technical expert of critical theory. The other side is – there needs to be a division of labor – the visions of public social sciences that assume that one person does everything, does fundamental original research, is a master of social thought, teaches, writes articles that are accessible to the broad public and serves as governmental expert, is first a fantasy. Secondly it's an illusion encouraged by the star system, what the government calls stars, whether or not they are experts. It is somewhat the enemy of new ideas, because it tends to be the older established people who nominate all those positions. But it has also undersold the socially organized process so it would be a good thing for all sociologists to write better than they do. But we should not imagine that all sociologists should be writing for popular consumption. Some sociologists should be doing their serious work because their contribution is knowledge. They should be thinking about how there is work actually useful to the public. How their work is actually important, what questions it answers. They shouldn't do this just to have more lines on their CV, but there should be some division of labor with people who are better at writing for the

public. In the same sense, we celebrate originality and we denigrate synthesis. We think that synthetic work is not original. In a sense, we reduce originality to discovery in a natural science model, but even more almost a prospector model, like somebody going out to look for gold or diamonds and who finds them. Very little scientific research involves discovery in that sense, even in the hard sciences. It involves a much more organized, structured work in relation to work that has been done before and most of the experimental work is testing, not discovering.

For us in the social sciences, what we need to recognize is that we make relatively few discoveries – it's nice when we do – but we have an incentive system that is overwhelmingly focused on getting people to produce ostensible original contributions to knowledge modeled on the idea of discovery: "I found a new fact!" and the new facts are mostly trivial and unimportant. Or: "Other people have studied this in these settings. I studied it in Borneo. Nobody has studied the complex forms of organizations in Borneo before and I found they are the same as everywhere else." That's not an interesting fact! Or whatever it is, like: "People have been studying stratification, I have found 1.5% difference in this model." All of our incentives are geared to that. Very little to synthesizing what we know.

Now, synthesis is a big category and it has been devalued. Think of analytical sociology. Analysis is good, it's tied to the original. Lots of the work that goes on is synthetic. Synthesis gives shape to knowledge. It can be very original work, and much of what we call theory is in fact synthetic, it's putting together and organizing knowledge in an area. It's important to know empirically what we know.

In America we tend to leave synthesis to the writers of textbooks. Until recently Europe didn't do textbooks and synthesis had a close relationship to theoretically-oriented grand intellectuals. Here, it's to textbooks writers, or to outsiders, to journalists. We need to value more the development of good, synthetic understanding, putting together the different pieces. So we have high quality specialized knowledge, that we tend just to let sit, as isolated specialized knowledge, with low quality synthesis, low quality of putting together the pieces because we just left it to amateurs or to low status textbook writing which has to be understandable to 18-year olds who cannot deal with all the issues.

I was once the author of one of the top line books. That's valuable labor but it tends to be left to status figures that are less high, and that's not the kind of synthesis entirely that I'm talking about that can be helpful, because it has to be sufficiently simplified and it really is a market phenomenon, it's really meeting a market. Where, intellectually, how should we think about what's happening to social inequality today? Now, some people work on that. They work and write. Like Piketty, he is not reporting just some facts, he's trying to tell you how to think about it. We have

an enormous shortage of high quality synthesis compared to specialized facts. This is also part of the crisis of the public intellectuals that we need that in order to be able to contribute well to the public because it's very seldom the case that what the public needs is a fact. And in fact we need some of this and we need to be able to present it in shortened, accessible forms; synthetic understanding.

I'll take it back to films, as a conclusion [*laugh*]. There may be a stunning scene in a film that offers you great insights. The work for a film is not the work of the actors alone. It's the work of writer, director, editor. And putting all of that together to make it a coherent whole. There is a kind of discipline to a film. It can only be a couple of hours, so you have to leave a lot of things out. Every film of any significance – not just documentaries – involves shooting much more than you put in, in the end.

In a film of a novel you've left out 90% of the novel, but there's a discipline to that. It's not all bad, it's not just loss, compared to writing long books, because the discipline is figuring out what is important and how to put those parts together, to tell a story that people would care about and they will remember. In the end, they only remember one scene; often it's the case with films that people remember visually one scene. But that scene became satisfying because the film was put together in a way that worked.

In the same sense, in the social sciences we are imagining public – public sociology, public political sciences, public anthropology and so – as though it's only actors, as though the films would be the project of only actors, as the celebrities. Not the whole and how this is put together and synthesized. So, we need for ourselves a synthetic knowledge that often looks a lot like a theory but a very empirically formed theory and for presenting to the world what is the best understanding you could have of the immigration problem based on scientific research, not "what did I find in my research."

The typical social scientist wants to tell you egocentrically the findings of their research. And so, in a field like migration – thousands of people working on it – your findings are probably only a small addition. What we want to know is what all of that field put together can tell us that is useful. So we need synthesis. And the less we value synthesis and give it credit alongside discovery and analysis, we won't produce a really intellectually serious social science.

REC: At this point I should ask you one last question. Who is your favorite film director? Or your favorite film?

CC: I suppose Bergman, but that doesn't mean I sit around and watch all the films of Bergman. I can go more contemporary. I have liked a lot of films of different genres. One of the films that made a deep impression on me is not even one of the best films by a film director that someone would like in general. I thought

Bertucelli's *Ramparts of Clay* [1971] was great. Now, it shouldn't surprise you that a Bourdieusian would like a film about Algeria [*laugh*], but it's a really good example to me of a work of film fiction that is offering a serious sociological analysis.

I don't really follow films now, I have to say, although I live in Hollywood, where there are people who have seen films before they appear in theaters! I have more favorite films than favorite directors. There are a few that I liked, that's why I say Bergman. Bergman is an intellectual filmmaker, a director filmmaker, an auteur filmmaker. However, if you would have asked me when I was a film student I would probably have said Truffaut.

References

Bourdieu, P.

1972 *Esquisse d'une théorie de la pratique, précédé de trois études d'ethnologie kabyle.* Paris: Edition du Seuil. [1st Eng. Trans.: *Outline of a Theory of Practice*. Cambridge: Cambridge University Press, 1977].

Thompson, E.P.

1963 *The Making of the English Working Class*. London: Victor Gollancz.

Sociology and Its Public

Craig Calhoun in Conversation with Riccardo Emilio Chesta

Abstract: In this interview, Craig Calhoun retraces his peculiar trajectory that, from his first interests in realist cinema and anthropology, brought him to become one of the most original contemporary public sociologists. Realist cinema and the art of documentary are not simple occasional curiosities or forms of escapes, but a way to observe and analyze reality common to the sociological gaze. He therefore discusses the aspects of this multidisciplinary education between Columbia, Manchester and Oxford. Supervised by the British social anthropologist Max Gluckman, his sociological path mixes the Western Marxist debate of the 1970s, Continental social thinkers and Pierre Bourdieu's sociology in new empirical inquiries on a variety of objects and fields like social movements, intellectuals, knowledge, power and the public sphere. In the final part, he identifies the requirements for a contemporary critical sociology that, avoiding to be reduced to an abstract theorizing or a new academic specialization, can fulfill its role of scientific discipline and public utility.

Keywords: Theorizing; Critique; Disciplinary Boundaries; Synthetic Sociology; Intellectuals; Visual Anthropology; Cinema; Public Sphere.

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The Mask and Identity. A Conversation with Alessandro Pizzorno*

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Abstract

An interview with Alessandro Pizzorno by Roberta Sassatelli.

Keywords: identity; Pizzorno; mask; recognition; reception.

Roberta Sassatelli (RS): Let us start with the context in which you wrote, more than fifty years ago your *Saggio sulla Maschera* (Pizzorno, 2005). You were in Paris, after a period of study in Austria...

Alessandro Pizzorno (AP): Yes, I wrote it among the books of the *Musée de l'Homme* library, masks in their cabinets. And then I brought it up in a seminar at the *École des hautes études* then directed by Ignace Meyerson, in which I took part regularly together with another seven or eight young scholars, including Jean Pierre Vernant. There were also friends who admired, outside that seminar and quite contrary to Vernant and Meyerson, the whole Parisian phenomenology debate. And I believe that the arrival of Pirandello on the Parisian theatrical scene was quite fundamental...

RS: Please, explain better...

AP: In the years between 1950 and 1953 in Paris, Pirandello was very current and as it gave a lot to the Italian cultural presence. I too saw *Sei personaggi in cerca d'autore* [Six Characters in Search of an Author] for the first time, in French. At a certain point, the *École*'s Italian doctorate students wanted to study Pirandello, and they asked me to do two lessons, also because they knew that I liked theatre, and so I studied Pirandello. At the time, I was part of Meyerson's group, which strangely called itself "Historical psychology," a name which today would not make any sense. However, in this work, there was a strong element of anthropology, or cultural psychology which, in reality, could have called itself anthropology. The *École*'s seminars were very good, an experience of sharing ideas and work, which however, was of an intensity I didn't later have, also because they were held once a week and we all had similar interests.

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Vernant was already working on his study of Greek tragedy (see Vernant, 1965), Francès was studying the sociology of music, another was studying dance, another *Gilgamesh*, all interests with different subject matter but methodologically similar, a methodology that was not orthodox but from many points of view innovative. However, above all, before beginning to present our papers on the progress of our research, we would spend a quarter of an hour, twenty minutes talking about what we had read over the week... something real had been created, there was a real exchange... I didn't ever do it again, but I always suggested it to whoever organized this type of seminar, that these were really moments of coming together. I even had in mind to do a paper on the festival but then Meyerson, who was a real killjoy, cut me down... and then there was Vernant and the theatre. In any case, these two occasions together, the seminars at the *École* and Pirandello, were what inspired me to be interested in the mask.

RS: A mix of coincidence or something more?

AP: It was clear that my interest in the mask, in my opinion, had deeper roots. I have always asked myself how, how did I become fixed on this subject? Note that, amongst other things, I was also the Paris correspondent for Radio RAI [the national public broadcasting company of Italy], and I generally sent pieces about the artistic scene, above all exhibitions. At a certain point, I started writing something that was then published somewhere (who knows where!) that was entitled *La danza come verità del corpo* [Dance as Truth of the Body] and which consisted of an interview with Catherine Dunham. I was really struck by this interview! You know that Catherine Dunham was a great dancer, who had however graduated in anthropology. She was originally from the Caribbean and she had graduated with a thesis on Caribbean dance. She had had great success in Paris and I had asked her for an interview. The fact of being able to write for Italian radio went down pretty well and she granted quite a long interview during which for at least half the time she was naked, having had her masseur come, and I did the interview as she prepared herself to go on stage... This is to say that then, my interest for the theatre and dance was very strong, it was an interest for the physical side of culture. This is what really moved me; I was part of a society which called itself *Dance et Culture* in Paris, and thanks to this I had free tickets, invites and so on, and I didn't miss a single dance! Neither a ballet, nor a performance, an ethical dance, with these African and Caribbean dancers. So, as you can imagine, generally this was my mix of interests at that time which were moving in a certain direction and really quite opposed to my very first law studies, which I had left for philosophy. Remember I had graduated in aesthetics with Pareyson.

RS: There was therefore continuity with the studies you had followed with Luigi Pareyson?

AP: Yes, to a certain degree, certainly. Let's say the central idea was strongly anti-Crocian: art was not an expression of sentiment but was rather communication. Art is both communication and a technique of communication for which dance, for example, is found in the technique itself and thus art is performed. The mask comes in here, with added theatrical interest. Not by accident and stirred up by *Sei personaggi*... Pirandello said that characters all present themselves with a mask, Pirandello's mask was one which defined them as certain types, that is they could be the mask of luxury, the mask of a mother, maternity... they were masks which represented their characters. Pirandello's theatre was one of recognition. The six characters are people who are looking for someone that recognizes them: they don't exist until they are recognized. Thinking about it again today, I would say that what I was looking for also had a more general nature. I thought positively about the mask (I believe I said this at the beginning of the work), not only as something which conceals, but also something which reveals...

RS: Like a kit to create an identity?

AP: That's right. The mask hides and reveals at the same time. But it does so differently, according to the situation and at different points during life. To begin with, you find a mask in the family. You think that no one has one. Then, you realize that the others are putting it on. And if they put it on, a little at a time, you begin to understand, above all when they are right in front of you. Thus begins the long and tortuous path of apprenticeship of the mask. You are observer and actor. You try to understand what is behind what the others are wearing and at the same time try to understand which one serves you best to protect yourself from others. And you put it on and you take it off. And when you are an adolescent, and you yourself are unsure of what there is behind the mask you decide to put it on, and you are

unsure of what you are really trying to hide, then you look for the most bristly or scary, or the gayest and ceremonially misleading mask possible to wear. You keep everyone else at a distance, while you watch yourself, little by little grow into the mask, which you have only partially created yourself, but which willingly or not you are forced to wear. The fact that I too, at that time was somewhat uncertain between hiding and revealing, probably played a part in the choice of this research, without being completely aware of it.

RS: In the study, there is much play between revelation and hiding. In the so-called tribal cultures, the mask would have the role of eliminating the person, or rather, of hiding it in order to show something...

AP: If there is something true in what anthropologists say about those cultures, it seems to stem from the institutional functions of their mask. The mask allows its wearer to be recognized. The spiritual role, above all, but also sometimes its political or military role. In our own traditional societies, which we have known about until recently, this role has been carried out by other signals, uniforms, seals, various symbols. In today's individualistic society, the metaphor of the mask refers more purposely to the dialogue of hiding and revelation of the individual.

RS: At a more abstract level then, the mask works as an existential analogy?

AP: Yes. We could be tempted to say that to whoever does not feel capable or strong enough to wear a mask in plain sight, then it serves to hide. Then the possibility of the mask matures in him, as the owner sees others recognizing him, and so for him accepting this recognition is the most obvious thing to do, less tiring, and so he will end up wearing that figure created by the recognition of others as his own choice of mask. But he still treats it a little as a mask. That is, something behind which he imagines he can still conserve a "hidden" place, to use for possible hiding strategies. It is probably in old age that this imaginary internal communication with one's consciousness stops and everything becomes a mask, silent for others, a repeated and apathetic silence for themselves. There are those who also have a death mask, solely for others and in any case useless. So, as they are useless, except of our needs as descendants, the remains that archaeologists bring back from their dig sites.

RS: Here, you are referring to what della Porta, Greco and Szackolczai, (2000, p. XVI) mention, that is identity as the history of recognitions obtained by the subject in the various contexts, he has found himself. Masks can be seen as a metaphor of recognition received or as declination of an identity that is being constructed...

AP: Precisely! This was exactly the point, insistence of the theme of identity, which is a theme which sociology has never really taken seriously, while for anthropologist this theme has always been absolutely central. Sociology, only really began to truly consider it in the 1960s and 1970s... It would be interesting to see precisely a philological history of the use of the term "identity" from Freud to Durkheim and Weber. None of the classics use this term, Why? Sociology starts with roles. Roles really are masks and more precisely, identity. But if we talk of a multitude of roles, then no one has for many years talked about multiple identities, which is a problem which is still of great interest today, that is that of the multiple self, for example in the collection of essays *The Multiple Self* edited by Jon Elster (1985).

RS: The mask hides while it creates, and creates while it hides. It is this dynamic — which lies between identification, fixing an identity in time and space, and fluidity, that is never fixing it — that has creative potential. The mask it seems, is a subjective technique that through fixation, creates the possibility to become, a dynamic which in modernity becomes endemic and daily, and not only in terms of synchronicity (face games or role playing) but also diachronic (construction of the self over time). Indeed, you insist on the temporal dimension. Here it seems that you are indicating "rhythm" — as a collection of practices which create differences in the flow of experience — as the principal technique to stabilize social "time"...

AP: The question of rhythm was important then for me, together with other experiences linked to the theatre and dance. Literature on the theatre in France in that period was very rich in this area, also in the sense of underlining movement, voice and dance. There was a theatrical instruction centre in which

actors rehearsed by acting with a mask. And it was the mask which had to express itself by moving, changing in order to see the light. The mask had to speak, it had to say something for the sole reason of being a mask that was to be seen, quite independent of talking openly... When I spoke of rhythm, or of things that swing, that mask, I was referring to something similar. This capacity on one hand of an actor to have the freedom to become wholly a mask and on the other to continue in reality, to be free compared to others, by means of movement, rhythm.

RS: This brings to mind that in that period, or only a few years later at the end of the 1950s, Goffman's book *The Presentation of the Self in Everyday Life* (1959), followed by Anselm Strauss's *Mirrors and Masks* (1959)...

AP: I never read Strauss, I knew it, possibly I didn't want to return to the mask. At that point I already had my own mask! On the other hand, Goffman seemed to assume that the theoretical problem had already been resolved... as such, given that things were as they were, he analyzed it magnificently in everyday life as a matter of ceremonies... In *The Presentation of the Self* (which was the first thing I read), the self is one which is presented but already exists, if you follow? It already has its own strategy, it defends itself, it does impression management, and therefore is someone who already exists as a person. It is not like the problem of the mask, which is a problem of the self that is not there yet! Goffman supposes that the self has already formed, and so he goes around the theoretical problem, which was really what interested me. Certainly, I was more interested in role distance (see Goffman, 1961), but even role distance is a strategy, even here it is a putting on and taking off of the mask, putting it on in one way or another, but the hands which moved it were always the same person! So, role distance was not enough. What is important is how can we distance ourselves from it? How can the self create distance? Goffman's answer is, "there are all these methods here...". The theoretical problem seems to me that of understanding how can we distance ourselves from the danger that a specific negative recognition or some undesirable measure from others humiliates us, depresses us, annulling the faith we have in ourselves. The weapon we have is that of facing down that negative judgement of ourselves with the recognitions, which, so to say, we keep in reserve, which we have stored little by little in our memory, after a process of mythologizing reconstruction, which feeds our self-esteem. If we analyze a person's internal processes, we risk having only a clinical image. But we would understand the same process better if we referred to a collective subject, a nation, or a state, a political party. Modern European states are constructed thanks to a convergent process of recognition. From within, there were the feudal powers, from outside there were the other states, which culminated in the Peace of Westphalia. But they had to feed this recognition, constructing myths made from history, archaeology and literature, the ethnic inventions... And then there were celebratory rituals, ceremonies, monuments and so on. It is more or less the same way that everyone built themselves from the inside. We can conclude that the production of masks is a function of uncertainty that we fell as to our identity. Think about nervous tics, idiosyncrasies, fixations, about the rituality of some of our daily behavior and so on...

RS: So, you are essentially saying that we shall consider a dimension of identity that can be arrived at through the mask and which Goffman does not fully grasp, a dimension which is diachronic and trans-situational — that is, which does not depend directly on the situation and cannot be reduced to it. A sort of biographical embodied identity. Is this so?

AP: I would put it this way; it is a bit like an autobiography that we try to write about ourselves, in order to have a hidden weapon, whenever we meet others so that we can escape the consequences of their judgement on the esteem we have of ourselves...

RS: Goffman also, in a way, hints at this autobiography. Indeed, he talks in precisely these terms in *Stigma* (Goffman, 1963), as it is there, right in front of incorporated signs that you cannot so easily free yourself of identification and its recognition. They cling to you... This is how certain observations in *Stigma* seem to be a bit germane to what you are saying in the essay on the mask. Above all because the mask seems to be a technique, if you like, rather than a strategy, a technique to fix personal identity, however public, over time and within the external circle of recognition. It is this which then is a glimmer of internal identity, as you call it.

AP: Indeed, Stigma is what I like most of Goffman... You have interpreted it precisely. If you like, it is as if I make myself strong at that moment when you put the mask on me with all the other masks that I have worn in my life... this is the case, I have a mask which fixes me, but I also have many other masks that you do not know...

RS: It's as if you are combining a series of things unknown to the bystander that really count! Beyond his supposed situationism, Goffman explicitly wants to look at the subject from outside and tries not to ever look at the possible internal motivations. He tries to put everything down to the specific local situation and almost seems to be afraid to touch on questions that have anything to do with psychology, a terror of going within the subject, also possibly out of respect or discomfort... However, it does not seem that you have this fear or am I wrong?

AP: Absolutely not!

RS: In effect, your writing always seemed to be very open to approaches from other disciplines. On this subject, in this your first work on the mask, which is clearly anthropological, you are not far from the work of the first British Cultural studies, I'm thinking of Hoggart and Williams who began to work precisely in those years...

AP: I skimmed Williams only much later, whereas I knew Hoggart well as he was part of the international work group on leisure time of which I joined a couple of times. He is also a very nice person, a typically working class Englishman, who was able to emerge thanks to the State school system, great company, above all when he drank, and he drank a lot. He had already saved himself from alcohol, and then wrote *The Uses of Literacy* (Hoggart, 1957). He was brilliant and also very good looking. I am talking here of the beginning of the 1960s, after which I lost track of him...

RS: As you were suggesting, you then confronted yourself above all with anthropological literature. In *Saggio sulla maschera*, which in the title sounds like that of Mauss (1925) on the gift — and possibly is closer theoretically to this work than to his work on the person (Mauss, 1938) which you cite with a certain degree of perplexity — you give great importance to what we could call material culture. You go over material details showing how much they are mediated by culture. I believe that the cultural mediation of materiality is one possible fertile lines of interpretation of your work on the mask, and therefore the theme of embodiment as cultural phenomena...

AP: Yes, in retrospect, I see the body as a cultural construction, I am thinking of my interest for dance. It was probably also an anti-intellectual reaction, isn't it?

RS: Indeed, at a certain point in the work, the material and symbolic characteristics of the mask as an object, or rather, to use your own words, as a "thing" capture your interest. You linger on the actual make-up of masks, their colors and also their sacred nature, which is attributed to them thanks to the fact that we are always considering the materials that we can define as being natural and so evoke realities outside the subject... Then, you finish the work talking of two means of creating. And you suggest that these are two forms of communicating of the human being which inevitably go beyond the material/symbolic dichotomy, one through the transformation of the material world and the other through the transformation of oneself. The mask in some ways is the link.

AP: The idea underneath — and I have to say I had not read the work on totemism by Lèvi-Strauss (1962) that came our much later — was that the mask was part of the means by which the individual fixed their identity so as not to think any more about it, so they became what they are, for us, animals, without any individuality. They are there, they are always the same. The mask is there and it's always the same. This then is totemism. I thought of the animal as one of those things that helped the individual see themselves in a definitive way, so as not to think any more about themselves... I do not exclude the fact that this was part of the period of an individual's first maturity, when we are afraid to be alone, always hidden behind the mask and try to understand how those managed when they put on the mask for once and for ever...

RS: You often mix anthropological-theoretical observations with your own experiences... So, the

mask becomes something that can't be taken off again. Above all, also in play, it isn't a fiction, it is a means, in a way genuine of constructing the self, is that so?

AP: Yes, above all, in old age the mask becomes fixed, initially the individual, takes it off, puts it on, on and off... until death it can be taken off, at death it can't be taken off any more. With each movement of the mask, each time the identity of the person is recreated. A few things about origin come to mind that I am writing for another book: I consider there are two essential concepts at the origin of democracy, these are the concepts of contract and of conversion. Contract and conversion are both means by which we are put into contact with each other, both are designed to change our relationship with others, and also to change others. With a contract, you establish a relationship with another person, but both parties change. By definition you change with conversion. What's more, I found something similar in Rawls' introduction, in his last book on political liberalism (Rawls, 1993). Here the problem is to see how we can get a group of people to agree, who for the moment have all gone beyond proselytism, that is the moment in which everything is created... Now, it is true that democracy is impossible if proselytism is forbidden (I'm thinking about certain forms of fundamentalism, which are obviously not democracies, which doesn't mean that this would exclude private happiness). Conversion is basically a change of mask: it is the moment of freedom! Identity can be seen in this view as a moment in which you abstain from converting, and conversion is a moment of final foundation. For this reason, it is not rational in terms of ends, given that ends are always rational in respect to an identity. Certainly, in the work on the mask, I did not talk about conversion and I did not have it absolutely in mind, but it is still a declination of a basic theme which in other ways I had in mind.

RS: Generally speaking, the central idea of your work *Sulla razionalità della scelta democratica* [On the Rationality of Democratic Choice] (Pizzorno, 1993), is that politics is the orientation to the change of needs more than to their realization. Now, what you have just said, brings in mind an aspect of your theory of participation, or rather the role of sacrifice. A great deal of the theory of collective identity tends to see things quite positively, underlining the creative moment, whereas you underline that to be able to create a collective identity, you need to abandon something, to sacrifice something. So we create, amongst others, a concept of time, a past, a present and a future. In other words, to a great extent you underline that collective identities form not in response to needs which are already here, but to propose new ones that de facto also implies sacrifice. This then connects with what occurs in masked rites: the sacrifice of one identity for the imposition of another, by means of the mask, which proposes a different universe of meaning and needs.

AP: In what sense do you mean new needs?

RS: I am thinking of the needs that are associated with a new identity, even collective identity, which are constructed through sacrifice of the previous identities. For example, if I become part of a critical consumption group, I have to give up something to make room for something else, such as fair trade products or recycling or self-production... and it is through these new needs that I fix my new identity as a present quite distinct from the past and as a project that builds the future.

AP: I haven't thought of that, but as you know, I was also interested in the phenomena of consumption... I could certainly see it explicitly in reference to identity, that is consumption as communication thanks to which you can be recognized each time in a different way. You dress in a certain way, then you have to go to a ceremony and change cloths and this change communicates how the new I is different from the others. But rather than consumption in itself, what is important is the changes in consumption...

RS: Yes, and the interesting thing in these changes is to read them not so much as purely cognitive-communicative processes, but rather as performative embodied processes. They are not a caricature of a dramaturgical model or a Veblenian status symbol, but rather as something that passes through more minute practices, both material and symbolic, and therefore which implies...

AP: ...that they self-change, self-reform...

RS: ...a notion of embodied communication in a strong sense, which cannot be separated entirely

from the subject. But maybe we are no longer talking about you!

AP: It would seem that you want to talk about you, but underneath it all you are talking about me... you have put a mask on me!

RS: And I want to talk about what is underneath it all! I would like to ask you also something else, more general. The essay is very elegant linguistically, it is a work that exudes literature, as well as philosophy... What do you think of the relation between sociology and literature? These are paths which have separated as Lepenies (1985) showed, which will remain distant as certain sociological positivism would augur, or are they reconcilable as some contemporary sociologists hope to achieve, in their own way, in cultural studies?

AP: It is certainly not an easy relationship, not so much for me personally, as I have never felt this as being problematic. I remember even among the Italians there were those who wanted to have a sociology which was also literature, and I understood this. I never thought like this but I did understand it. So effectively, when sociological autonomy did not yet exist in Italy, attention to literature was basically a means of seeing certain things and saying certain things. Think of how important it was in Italy, when we began to talk about sociology. Books like *Cristo si è fermato a Eboli* [Christ Stopped at Eboli], by Carlo Levi, Dolci's books, Scotellaro's research, and above all Danilo Montaldi's research, who was both sociologist and literary author at the same time. But the link with literature has generally been difficult. Even a sociologist who writes as well as Goffman has not been accepted by institutional sociology and social psychology. For example, when I had just arrived at Harvard, partially to introduce me to discussions, partially because I had just come from Europe, they included me into a committee to hire three or four new people. I had mentioned that in Europe, there was only one American Sociologist who enjoyed undisputed fame and that was Erving Goffman, but he was completely excluded, and I thought he was putting himself on the market. They absolutely did not want him there, as he was not part of mainstream sociology. It was a revelation, at most political bias could be overcome, although there was a certain political line. For example, there was a moment when there was a very respected candidate, very interesting but a declared Marxist. In the department, the person who was most right wing was Harrison White, guru of network analysis in America, yet he was fighting together with me for this Marxist, because he used a certain type of mathematics, do you get it? There was this type of block.

RS: And how did you navigate this type of block?

AP: I was never really in the block... Well I was a little, I also carried out empirical research, surveys, and so on, as indeed you have. But in Harvard, I was completely converted to the opposite, precisely because they needed someone who would deal with these things, a historical sociologist, and so on. You see, they needed this too. It was enough to control the department, and given that all the foreign students who want to do these other things, they were encouraged. In a seminar with Harrison White, which he recalls in his *Identity and Control* (1992), I developed a theory of identity and recognition trying to link it with the methodology of network analysis, of which Harrison was indeed the leader for American sociology.

RS: Let's get back to the mask. In your work, it is presented as an instrument or a technique that a human being can use or rather create to recognize oneself and others. Or better, if I can quote you, "one's only art, to recognize oneself," I am right?

AP: The form is probably wrong, as it is not the self which is recognized. I would say, to recognize someone, to be publicly presented. It is difficult to talk about, "oneself" at this point. For that which is acceptable for others at the end, possibly with difficulty, ends up with being accepted by them, inevitably, without exception, inescapably.

RS: This last passage is very important as it overturns the notion of performance as something which is merely superficial and introduces the question of power, control... It becomes, if not a Nietzschean *mnemotechniques*, then at least a Foucauldian subjectivation technique.

AP: Yes, yes, without doubt... Roberta, may I tell you something? I don't believe I explained it well in the work as I only knew afterwards. There are certain schools for stuttering children, or rather there

are stuttering children in certain schools where they are made to put on a mask. And when they are under the mask, the children stop stuttering. You see! It is something quite extraordinary, I may stutter, but when I put the mask on...

RS: The mask is heavy however... The idea of identity as consolidation over time, through tools that are visible and which can be publicly acknowledged, implies a cost — It is for this reason that we are close to Foucault but also Simmel, who each in his own way show the ambivalence of the imperative to self-construct. The subject feels in some way the weight of this mask that is no longer a tribal mask, no?

AP: I don't recall having thought so at the time. Even if there was a hint, and yet thinking about it... Yes, it is true, it is not excluded that I saw it. At root, this is the double nature of the mask, when it becomes the mask of someone, who wants to insist on their own autonomy and lives in some form of solitude. A mask that also hides continues to work on you until death. With death, certainly you can't hide any more (even if I knew a lady, who was attached to her own young beauty and when she died had put on her tomb a birth date which made her ten years younger), but right up to death, a small piece of this hiding is brought with you. Maybe it is both good and bad at the same time. But for who, at the end?

References

della Porta, D., Greco, M., & Szakolczai, A. (Eds.). (2000). *Identità, riconoscimento, scambio. Saggi in onore di Alessandro Pizzorno*. Roma: Laterza.

Elster, J. (1985). *The Multiple Self*. Cambridge: Cambridge University Press.

Goffman, E. (1959). *The Presentation of the Self in Everyday Life*. New York: Doubleday Anchor.

Goffman, E. (1961). *Encounters: Two Studies in the Sociology of Interaction*. Indianapolis: Bobbs-Merrill.

Goffman, E. (1963). *Stigma. Notes on the Management of Spoiled Identity*. Englewood Cliffs: Prentice Hall.

Hoggart, R. (1957). *The Uses of Literacy*. London: Chatto & Windus.

Lepenies, W. (1985). *Die drei Kulturen. Soziologie zwischen Literatur und Wissenschaft*. Munich: Hanser Verlag.

Lèvi-Strauss, C. (1962). *Le totémisme aujourd'hui*. Paris: Puf.

Mauss, M. (1925). Essai sur le don. Forme et raison de l'échange dans les sociétés archaïques. *L'Année Sociologique*, *seconde série*, *1923–1924*, *tome I*, pp. 30–186.

Mauss, M. (1938). Une catégorie de l'esprit humain: la notion de personne celle de "moi." *Journal of the Royal Anthropological Institute*, *LXVIII*. https://doi.org/10.2307/2844128

Pizzorno, A. (1993). *Le radici della politica assoluta e altri saggi*. Milan: Feltrinelli.

Pizzorno, A (2005). Saggio sulla maschera. *Studi Culturali*, *1*, pp. 85–109. (Reprinted as Pizzorno, A. *Sulla maschera*. Bologna: Il Mulino, 2008).

Rawls, J. (1993). *Political Liberalism*. New York: Columbia University Press.

Strauss, A. (1959). *Mirrors and the Mask. The Search for Identity*. New York: Free Press.

Vernant, J-P. (1965). *Mythe et pensée chez les Grecs. Études de psychologie historique*. Paris: Maspero.

White, H. (1992). *Identity and Control*. Princeton: Princeton University Press.

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Roberta Sassatelli is Professor of Sociology at the University of Milan (Italy). She has previously taught at the University of East Anglia (Norwich, UK) and the University of Bologna (Italy). Her research focuses on the theory of consumer action, the sociology of consumer practices and the politics of contemporary consumer culture as well as sociology of leisure and sport and sexuality and gender. Her last books are *Corpo, genere e società*, with R. Ghigi (Bologna: Il Mulino, 2018); *Italians and Food* (ed.) (Basingstoke: Palgrave, 2019).

Alessandro Pizzorno: European University Institute (Italy)
Alessandro Pizzorno (Trieste, 1924 – Firenze, 2019), Emeritus Professor of Social Theory at the European University Institute in Florence, has been one of the main Italian sociologists and intellectuals of the Twentieth century. After his studies in Philosophy, Anthropology and Sociology at the University of Turin, Vienna and the EHESS Paris, in 1953–1959 he became Director of the Center of Social Relations at the Olivetti in Ivrea. In his early academic career he held posts in multiple institutions abroad and in Italy, including the University of Tehran from 1959–1960, the University of Urbino from 1960–1973 and the Nuffield College at the University of Oxford from 1973–1975. He worked as the Director of the Department of Sociology at the University of Milan from 1975, before taking up a post at Harvard University in 1976–1985 and at the European University Institute in 1986–1994. Professor Pizzorno was a founding member of the Academia Europaea, and authored many significant studies on social and political theory, collective action and cultural sociology. Among his works are *Le classi sociali* (Bologna: Il Mulino, 1959); *Comunità e razionalizzazione* (Torino: Einaudi, 1960); *Lotte operaie e sindacato in Italia 1968-1972* (Bologna: Il Mulino, 1978); *I soggetti del pluralismo. Classi, partiti, sindacati* (Bologna: Il Mulino, 1980); *Le radici della politica assoluta* (Milano: Feltrinelli, 1993); *Il potere dei giudici* (*Il nocciolo*, Bari: Laterza, 1998); *Il velo della diversità. Studi su razionalità e riconoscimento* (Milano: Feltrinelli, 2007); and *Sulla maschera* (Bologna: Il Mulino, 2008). In 2004, he won the *Medal of the President of the Republic* and the *Pisa Literary National Award*.

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Times of Sociology. Eviatar Zerubavel in Conversation with Lorenzo Sabetta

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Abstract

This interview offers a historical reconstruction of Eviatar Zerubavel's work, from his pioneering studies of time to his not-yet-published analysis of "concept-driven sociology," running the gamut of Zerubavel's career and embracing a period of more than forty years of sociological research. The interview encompasses several major topics: the beginnings of Zerubavel's own intellectual path and his move from Israel to the United States; the nuts and bolts of sociology of time and cognitive sociology; the underlying theoretical framework of a transcontextual and comparative mode of social inquiry; an in-depth analysis of the last books which Zerubavel has devoted to the study of phenomena such as backgroundness and taken-for-grantedness; the range of his academic and intellectual relationships (especially the one with his mentor Goffman, but also his rapport with Peter Berger, Lewis Coser, Renée Fox, and Murray Davis, among others); the polymorphic connection between sociological theory and politics; the development of the so-called "Rutgers School of Sociology"; the issue of "public sociology"; the future of sociology and academic research.

Keywords: Social Theorizing; Taken-for-grantedness; Sociotemporal Order; Erving Goffman; The Social Construction of What?; Rutgers School; Concept-Driven Sociology.

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Introduction

One of the most intriguing characteristics of Eviatar Zerubavel's work is its boundary-crossing nature: it runs through two continents and spans the last four decades, spreading across a number of substantive themes (time, memory, cognition, classification, identity, denial, human geography, semiotics) and merging together several sociological traditions (as a heterodox follower of Simmel, Durkheim, Schutz, Goffman, and Berger). The multilayered result has been an original culturalist cognitive approach that digs into "the sociomental conventions by which we perceive, attend to, and disattend to features of social reality, classify and categorize the world, create meaning, construct identity, remember events and comprehend time" (Brekhus, 2007, p. 448). For Zerubavel, sociology is an engaged and largely viable analytical competency rather than a monolithic understanding of society and sociology, a theoretical vantage point which is not constrained to a specific field of inquiry. That being said, however, Zerubavel's approach is not only still strongly tied to certain specific fields of sociological inquiry, but it has also virtually founded at least one of them, i.e. the sociology of time, an area of study which was previously more based on the genius of personal insights than on systematic empirical research.

Anyway, it is not hard at all to identify a set of meta-level pillars that constitute the underpinnings of Zerubavelian sociology — even though the author needs no introduction, it still seems useful to summarize them in a schematic overview:

- a firm commitment to the comparative method, characterized by the quest for similarities among prima facie dissimilar phenomena, emphasizing formal and abstract properties of social facts instead of their specific manifestations. In a Simmelian manner, the "social geometry" (Zerubavel, 2007, p. 132) is indifferent to singularities and aims to transcend idiographic details, trying instead to reach the highest level of generality possible. As it has been noted, establishing connections between apparently unrelated things, "Zerubavel links the Berlin Wall, animal rights, Marxism, Escher's art, religious purification rituals, knock-knock jokes, and the Dewey Decimal System" (Kearl, 1992, p. 698);

- a constant focus on those aspects of social reality which are neither absolute nor postulational, yet not private and chaotic either, that is, aspects that are "not so different as to be utterly idiosyncratic, yet at the same time also not so similar as to be absolutely universal" (Zerubavel, 1997, p. 11). Remaining equally distant (and distinct) from both the subjective world of the individual and the objective world of nature and logic, Zerubavel's sociology is always about delving into the realm of intersubjectivity and its multiform incarnations: communities, norms, styles, traditions, socialization, conventions, culture. In this sense, it is clearly possible to talk about "Zerubavel's long-term project to understand human cognition through its integration in cultural contexts" (Ben-Yehuda, 2007, p. 1246);

- a dereifying mode of inquiry that carefully applies the sociological perspective to a number of elements so pervasively widespread and deeply embedded in our culture to appear almost inevitable. Time, of course (and the set of its corollaries: calendars, clocks, schedules, the week, etc.), but also thinking and cognition, perception and expectation — "the social gates of consciousness" (Zerubavel, 1997, p. 35) —, arriving eventually at the idea of a social structure of irrelevance, backgroundness and taken-for-grantedness, which are the basis of any social constructionist theses (see Zerubavel, 2015 and 2018);

- as a result, always aiming to "demonstrate where nature ends and social convention begins" (Zerubavel, 1981, p. 4) and unmask the prominence of certain social forces that have shaped what seems unshaped or uncaused, this approach has a strong inclination for establishing various groups of new social facts, therefore devising well-suited new (sensitizing) concepts: "sociotemporal order," "quantum mental leaps," "mental discoveries," "islands of meaning," "conspiracies of silence," "semiotic asymmetries" — a toolbox of metaphorical crowbars indispensable for seeing sociologically certain hidden (/not immediately apparent) dimensions, otherwise overlooked;

- a particular emphasis on the absence, the unsaid, the unnoticed and the unmarked. The accent is on the proverbial "dark side of the moon": to remember implies to forget, perceiving involves

neglecting, making distinction means leaving something indeterminate, discussing certain things implies being silent about others, underlining and stressing entails taking for granted. As Simmel noted: "If human sociation is conditioned by the capacity to speak, it is shaped by the capacity to be silent. (...) We rarely realize how necessary this capacity for silence is in the development of any regulated interaction; we rather take it for granted" (Simmel, 1950, p. 349). Talking about not talking and noticing the absence require to " 'see' and 'hear' the conventionally invisible and inaudible" (Zerubavel, 2018, p. 9) and call for "an open discussion of the very phenomenon of undiscussability" (Zerubavel, 2006, p. 16), therefore highlighting "social negative spaces" through figure-ground reversals;

- a consistent attempt to prioritize the role of theory in the making of social research. The idea is that techniques and statistics cannot run (or even begin to) without an appropriate theoretical foundation and that sociology should necessarily be driven by a theoretical input: theory is viewed as a prerequisite, considered as a must. Thematically and semantically driven, every Zerubavel book reveals his willingness to make the role of theory essential, necessary to every step of the inquiry, as the only way to trigger the "mental process of *abstraction* that allows social pattern analysts to focus on, and thereby uncover, generic pattern" (Zerubavel, 2007, p. 140).

There are elements, of course, quite resistant to any schematizations. What can be interesting about the sociology of Zerubavel is also his very elusiveness to categorizations as such (not qualitative nor quantitative, not relativist nor positivist, not perfectly European nor exclusively American, and so on). Or one can be attracted just to Zerubavel's attitude of a "sophisticated sleuth," a stance indispensable for discerning those elements which are "in full view, yet (...) unnoticed by almost everyone" (Melbin, 1983, p. 1338). A *sociologists' sociologist*, as one is tempted to say, particularly interesting from an analytical and theoretical point of view, even though his skill as a "tour guide" (the definition is from Viviana Zelizer), able to show countless aspects of social reality, makes him pleasantly readable by anyone.

Moreover, an additional feature of interest in Zerubavel's scientific path is that it intersects with the broader path of contemporary sociology and its recent history. Being a "marginal classic," neither mainstream nor niche, the peculiar sociological approach of Eviatar Zerubavel is even more telling about what contemporary sociology is, what it has been, and what it might have been. Thus, "the challenge is to connect Zerubavel's work to what most of the rest of us do. Again and again the reader is reminded of Simmel, who was sui generis and yet was quintessentially a sociologist" (Katz, 2016, p. 673).

The following text is the transcription of a video call interview made on 16th August 2018, revised and approved by Zerubavel himself.

16 August 2018

1 First Steps: From Israel to the US, Goffman's Mentorship, and the Standpoint of a Simmelian Stranger

Lorenzo Sabetta (LS): Let's begin with your education: how did you come to sociology? Were there, in retrospect, any sociological factors that might account for your earliest career choices?

Eviatar Zerubavel (EZ): It's actually rather random that I ended up in sociology. When I was very young, less than seventeen, I had one more year before the military service (compulsory in Israel), so I took a year of political science and sociology — sociology was actually at a department which was conjoined with anthropology, at Tel Aviv University. Three years later, after I finished my military service,

I just continued on automatic pilot. It was only during my last year, indeed my very last semester in college, when I read Mannheim's *Ideology and Utopia* (it was required for a course in the sociology of intellectuals) that I was really blown away by the fact that there can be sociological ways of studying thinking. I was very interested in the psychology of thinking, but it never occurred to me that you can look at it sociologically as well. Right after, I read also *The Social Construction of Reality* (I think the sociological book that influenced me the most) and at that point I didn't need any further persuasion to go into sociology.

Then, I discovered Goffman, and that sent me into graduate school at the University of Pennsylvania to study with him. But it's actually the two books that I just mentioned that clicked it: I don't know if without having read them I would have continued in sociology. I mean, I was already fascinated by the cognitive dimension of the human existence (social psychology and perception, for example) and by sociolinguistics. And the work of Benjamin Lee Whorf was the first academic work I discovered on my own, rather than as a required course reading.

LS: What year are we talking about here?

EZ: This was 1971, when it was clear to me that I wanted to enroll in the Ph.D. program at the University of Pennsylvania, where I arrived in 1972. And when I came there, as a matter of fact, what attracted me was not in the sociology department, since it was a department with strengths in demography and criminology. Instead, I was attracted by three specific individuals, three figures: William Labov (in the Department of linguistics), Dell Hymes (in the Department of folklore), and Erving Goffman (in the Department of anthropology). So, I ended up taking courses with all of them, but it was the encounter with Goffman the most fruitful from an intellectual point of view.

LS: I don't want to linger too much on your relationship with Goffman since your 2008 interview with Dmitri Shalin[1] is all about that rapport, but if you'd like to add something more, please.

EZ: Well, I came from Israel to Philadelphia in order to study with him, that was the main reason. Again, I wanted to also study with Labov and Hymes, but Goffman was my primary motivation. Ironically, I had to write a petition to enter his course because he was officially in anthropology (he didn't even have an office at the sociology department: his room was in the museum). He did not accept students from sociology, except if they convinced him that they should study with him: almost nobody made it, not to mention writing a dissertation under him. He would announce in his first class that he was going to accept automatically every student from anthropology, from ethology, from folklore, from communication, from linguistics. All the others had to petition, so did I, and it took a while to be accepted. I mean, it wasn't the warmest welcome, and it went on like that throughout. I was extremely attracted to him intellectually, but he wasn't kind to me either personally or academically, in the sense that he tried to dissuade me from studying what I really wanted to study (that is, time), but I stuck to it.

Broadly speaking, I would say that substantively, basing on my earliest works, you would never know that I even studied with Goffman, because I soon started developing my passion for the sociology of time, which he wasn't interested in and indeed tried to talk me out of it. Instead, methodologically (and not in the superficial sense of data, statistics, etc.) I learned to look at the world from the standpoint of Goffman. He had a unique way of looking, an ability to observe what falls in between the things that you see, establishing patterns that can't be reduced into substantive facts. That's what I learned merely from being around him. He was the co-advisor of my doctoral dissertation, but I was inspired especially by one of the courses that I took with him; a class of 15 graduate students, and the whole course was basically just watching slides (many of which he later used in *Gender Advertisements*), analyzing pictures of 2–3 persons interacting with each other. It was unbelievable — you think at a certain point "What more is there to see?" and then he would chime in and start saying what he visualizes there. It was so influential, just seeing how he looks at things.

This was 1972–1973, when he was doing the touch-ups for *Frame Analysis*; I didn't know he was working on it, but he kept using those analytical concepts then employed in that book. I was mesmerized by that. Anyway, I can say that my work on time was influenced "by him despite him," in particular for looking at social reality the way I learned from him. Only later, in the 1990s, as I deepened my

1. See Shalin (2008).

engagement with cognitive sociology, I connected much more also with the substance of Goffman's works, especially *Frame Analysis* and *Behavior in Public Places*.

LS: You have been working for most of your life in a country (on a continent) other than your own. How important was that for you? Have you experienced the pros and cons of the ideal-typical Simmelian stranger?

EZ: Absolutely, and it has explicitly come full circle in my latest book on taken-for-grantedness, because like a Simmelian stranger I just couldn't take things for granted! It has been very subtle, though, because Israel and USA are not that different culturally as when you contrast the United States with, say, Mongolia or Sudan. So, the first few months here I thought "This is pretty familiar, after all." The nuanced differences started coming up later, and not being a cultural insider was quite relevant precisely in terms of the impossibility of taking social reality for granted: on balance, a side benefit of being a foreigner.

As far as the cons are concerned, instead, they have been mostly related to language. It took me a long time to develop proficiency in English, and 46 years later sometimes I'm still not altogether proficient in everyday English, especially because I speak Hebrew at home with my family. Even this one, though, is a con only in the short run and, ultimately, is a long-term pro, because I'm sensitive to shades of meaning and subtleties of language in a manner that's much harder for many native speakers. I'll give you an example: in *Taken for Granted*, I discussed the term "white trash"; while Americans think they are saying something about the noun, I conceive this label as saying something about the adjective (Zerubavel, 2018, p. 3). It's tricky, the sense depends on whether you put the emphasis on the noun or on the adjective. The fact that English is my second language allows me to not take these commonplaces as a given.

I mean, you have to learn how to turn cons into pros. After all, it's the situation of being a Simmelian stranger: you are not an insider in either culture. I've been living here in the US for 46 years, but I'm not a native-born American; I've been living outside of Israel for 46 years, so I'm not *fully* Israeli anymore. Intellectually, it gives you certain advantages; existentially, it can be difficult at times, since you don't have the same sense of belongingness that nonimmigrants have.

LS: Is it therefore about making sacrifices, enduring existential pain and emotional hardship in order to yield a richer intellectual output?

EZ: Yes, that may be true, even though I'm not sure if it's pain as much as loss.

2 What Is Sociology All About? Between Insiderism and Outsiderism, Subjectivity and the Objectivity

LS: I would like to expand more on the theme of self-interpretation, since virtually all your works open with straightforward elucidations of the interplay between your sociological analysis and your own life experiences. You wrote: "Unlike many sociologists I know, I happen to regard the fact that I study human beings rather than stars, rocks, bacteria, or fish not as an inherent obstacle, but, rather, as one of the true fascinations of being a sociologist. Doing sociology has always implied further harmony between my professional life and my personal life" (Zerubavel, 1981, p. XVII). I am aware that right now there is a lot of hype surrounding this concept and it's easy to jump on the bandwagon, but would you say something about what reflexivity means to you?

EZ: To tell you the truth, I associate it mostly with Weber's idea of *verstehen*. There is a certain gain that you have as a sociologist: you can understand social reality from within. Being a human being, even if I study someone from a very different culture, you know, there's still the humanness in both of us and I can grasp how this person thinks in a way that, to me, is much more credible than my ability to comprehend a dog or a skunk. The flip side of that, which is what you can see — as is well-known in autoethnography — is the danger of losing the distance. You need some distance from the object of your analysis. Anthropologists tried to highlight it in terms of the tension between so-called emic and

etic perspectives; the risk is that you may end up attributing to your subjects illusory things that are not really there, just because you bring them from your own culture.

At the same time, studying the culture you're living in, what is needed is to take a step back and try to have a look from a distance. It's quite clear right from the beginning in my work on time, especially in *The Seven Day Circle* (Zerubavel, 1985), when I identified the seven-day week as a certain rhythm that seemed to me so pervasive, so ubiquitous in our daily life, in my daily life: I knew it from within, but I couldn't assume that I apprehended it necessarily in the same way other people did. I surmise that this tension "insider vs. outsider" plays a key role here.

LS: The efforts to identify and investigate those aspects of social reality which are neither axiomatic nor universal, yet not entirely personal and subjective either, is perhaps the most recurring leitmotif in your sociology. What are the roots of your lifelong commitment to a «supra-personal yet nevertheless sub-universal» (Zerubavel, 1997, p. 21) level of analysis? Is this space between the quirkily idiosyncratic and the naturally lawful the authentic domain of social theory, that is, of a truly "sociological sociology"?

EZ: You hit it on the nail. I do believe that theoretically, as you said, it is the most recurring leitmotif in my sociology, even though it took me almost 20 years (until Zerubavel, 1997) to explicitly articulate this point. It didn't come right away. It has been, actually, the influence of Émile Durkheim, on the one hand, and Sorokin and Berger and Luckmann on the other hand. The Durkheimian part is evident: if I had to say what's the first sociological text one should read, I would recommend the chapter from *The Rules of Sociological Method* titled "What is a social fact?"" (Durkheim, 1982, pp. 50–59). To me, it's a brilliant and articulate statement on what sociology is all about, unambiguously pointing out that sociology ought to be different from psychology (by the way, at his time psychology meant the individual; today it means the human).

So, firstly, you have to examine norms, you have to talk about traditions. When I started my research on time, I realized that there was the whole Bergsonian perspective that's highly personalistic, highly subjective (my sense of duration is not like yours, etc.). On the contrary, in Schutzian terms, my point was not about subjective temporal orientations, but about intersubjective temporal orientations — my article on the standardization of time is where this frame of reference becomes the clearest.[2] When I look at my watch and I see what time it is now, 11:27 AM is not a personal thing, since if you look at your watch, you'll see the same. However, and this is the Sorokin and Berger and Luckmann part, 11:27 AM is not an absolute time. It's something we share, but it's not objective, because objective would be the natural or the logical, and — in the case of time — the physical.

Thus, it was very significant for me trying to identify temporal behaviors that were not personal, but collective, and at the same time following social conventions and traditions rather than falling into the laws of nature. This is why I really think that the best characteristic of the work behind *The Seven Day Circle* was choosing the very research topic: not personal and so omnipresent but not natural.

After I did it in my work on time, I continued doing it when I studied thinking. Most remarkably, when I contrasted "cognitive individualism" with "cognitive universalism,"[3] highlighting how the divergence is not between subjectivity and objectivity, but between both of them, on the one hand, and intersubjectivity on the other. Psychology nowadays is moving more and more into cognitive science, but usually occupies both the positions of individualism and universalism. It's only sociology and social psychology that deal with what lies in between. Speaking of which, I define the social as the whole range that starts from an individual plus 1 (or a single person in a social context) and goes up to humankind minus 1.

3 Zerubavelian Sociology as an Analytic Literacy: Transcontextuality, Abstraction, and the Dichotomy Substance/Form

2. See Zerubavel (1982a).

3. See Zerubavel (1997, pp. 1–22).

LS: Still on the topic of sociological metatheory, there is another feature of your approach I wish to dwell on. As has been pointed out, your theoretical method is a broadly applicable perspective, «translatable across nearly any subfield of sociological inquiry» (Brekhus, 2007, p. 449). You yourself outlined the main points of this peculiar mode of inquiry: a theme-driven rather than a data-driven style, the search for similarities among prima facie dissimilar phenomena, a commitment to the comparative method, the emphasis on formal properties instead of their specific manifestations. Now, the question is, doesn't it seem like this type of approach is declining and losing momentum? Is it a sort of knowledge that can be reasonably expected only from classical sociologists or big names? How to reinvigorate and strengthen the kind of sociological imagination that produces works like "schedules and calendars in social life" rather than "schedules and calendars in Benedictine monasteries"? I'm asking also because I know your next book is going to revolve around this issue, i.e. what you have called "general theory."

EZ: One of the events that made me aware, most poignantly, of this sort of analytical competency behind my approach was when Wayne Brekhus (when he was studying with me) told me that my "Cognitive Sociology" class should be considered a literacy course, for it wasn't really a course about cognition and the predominant emphasis was on a metalevel dimension that cut across different subfields. I guess he was right. When I completed my dissertation, my other co-advisor, Renée Fox, asked me "Where's the methodological chapter?" and I was like "What am I supposed to write there? Do you want a chronicle of how I spent my time doing fieldwork in the hospital?". She replied, "Not at all, I'm referring to how you observed something unobservable like time by doing field research in a hospital." Eventually, I didn't include such a chapter, it was a cop-out. But I knew that I chickened out and then, less than a year later, I presented a paper at the 1977 Eastern Sociological Society meeting,[4] partly based on the work of Everett Hughes (technically my grand-mentor, since he was Goffman's mentor[5]) with him in the audience. I tried to explain how a study *in* a hospital is not necessarily a study *of* the hospital, since I had realized that in *Patterns of Time in Hospital Life* I couldn't have convinced anyone that the book was about time and not hospitals, because all my data were situated in that context. This led me to the comparative method.

Now, sociological comparative method, traditionally, goes back to Weber and aims to underline variations, differences. Instead, I appreciate the comparative method for the opposite reason, because I want to focus the attention on similarities, drawing examples selected from tons of different substantive contexts. Transcontextuality, to me, means to reason analogically, looking at things that while seemingly disparate on the surface, deep down can be regarded as similar to each other. It's the metaphor of superficial vs. deep, surfaces vs. essences (it comes very nicely in Hofstadter and Sander, 2013). I mean, you *can* compare apples and oranges, for both are fruits. My point here, and I think it was also the main argument of my book on classification,[6] is that similarity and dissimilarity, which are the bases of the act of classifying, are not absolute qualities: the question is what you emphasize, and not that two things are inherently similar to or dissimilar from one another.[7]

Moving on, you asked if this is something that can be expected only from big names. John Levi Martin used to give me precisely this critique, adding that it's unfair to expect it from students. From where I sit, this is not the case. Instead, I think it's something that can be cultivated. So far, I've chaired 20 doctoral dissertations and many of them did adopt this approach. Besides, I don't know if this mode of sociological inquiry is losing momentum. To be honest, I don't believe in these statements, I'm not really sure "where the field is going." Look: when I used Simmel's ideas during the preparation for my comprehensive doctoral exam in 1974, I was told that Simmel was passé, and that I was attaching myself

4. Then published as an article: see Zerubavel (1980).

5. In this regard, see Zerubavel (2011, pp. 120–121), on "quasi-intergenerational chains of mentors" and the use of genealogical diagrams to portray the flow of intellectual influence.

6. The reference is to Zerubavel (1991).

7. A recent example of "the social construction of difference and similarity" (Zerubavel, 1996a, p. 426) is provided by Asia Friedman (2013), with specific regards to human bodies and their sex sameness (to which social actors are mostly inattentive, almost blind) and sex differences (which are generally stressed, acutely noted, and accurately recorded). In this light, "the many physical similarities between the sexes that are there, ready to be acknowledged, are normally relegated to the background of our perceptions. (...) When norms of disattention become reified, sex differences begin to seem as though they are *actually* more salient than sex similarities, when they are only more *socially* salient" (Friedman, 2013, p. 14).

to someone dead in sociology; since then, Simmel came back more widely studied than ever.

In this sense, I don't like concepts such as "field," "literature": these are just essentializing expressions, given the multi-paradigmatic nature of sociology. If you take any of my books and open any page on the bibliographical references, you'll find people who have nothing to do with one another substantively, who have probably never appeared in a bibliography together, but they do appear in mine. I feel very strongly about eclecticism and intellectual pluralism, I find them liberating.

LS: Speaking of eclecticism, transcontextuality, degrees of abstraction, and formal approaches to sociological reasoning, it's really staggering how many topics you have dealt with, while maintaining a consistent style and strategy. How has your "sociological nose" — as Peter Berger (1992, p. 12) called it — for what's relevant stayed the same and how has it changed over your career? And since your works are at the same time both thematically heterogeneous and theoretically coherent,[8] what do you see as the deepest threads that run through and connect them?

EZ: We are back to the dichotomy substance/form. Substantively, there is a big difference between what I did in the 1970s and early 1980s working on time and what I have done since then studying cognition. But formally, analytically, those phases are fairly close to one another. In the book on "generally speaking" that I'm writing right now,[9] I insist a lot on the importance of Herbert Blumer's idea of "sensitizing concepts" (1954, p. 8). It's not an unsung theorization, it's celebrated, but not enough. Sensitizing concepts are vaguely defined concepts, but this is their strength, because they are like magnets, you put them in your mind and they keep attracting pertinently related data.

By the way, my notion of data is not that conventional. You know, my first research project on time was based on fieldwork in a hospital and, of course, nobody had problems with that. It was the same when I wrote later on historical stuff (like the calendar of the French Revolution), since people said: "Well, there are no primary sources, but secondary sources are fine: he's dealing with historical data." But when I wrote *The Fine Line* (1991), *Time Maps* (2003), *The Elephant in the Room* (2006) or *Ancestors and Relatives* (2011) my data were chosen from everywhere, and in doing that I was influenced by Goffman. He did fieldwork for his dissertation in the Shetland Islands north of Scotland, but he didn't deliver *The Presentation of Self in the Shetland Islands*. One of the things he did, in order to make it more general, was bringing a lot of data from fiction: a passage from Henry James acquired the same status as the field notes gathered during the ethnography. It must be noted, though, that I never heard from Goffman that this was something praiseworthy, he rather recommended that I should study only observable social facts, "in the flesh."

The Fine Line was my first book entirely based on examples that I saw around me and I read about, a book not specifically situated in a particular substantive context. It was kind of risky. At that time, I felt very defensive when other scholars attacked me methodologically. It took me several books and a couple of decades to start feeling comfortable with it. People used to say "You're using just your examples, you're doing cherry picking": ok, let's talk about the art of picking cherries, how to choose proper examples. I don't think it's not systematic or accurate, it can be indeed super-rigorous. When I worked on *The Fine Line*, I worked six years exclusively on that, gathering information about distinctions and boundaries everywhere I could spot them. I was very focused, and the art of focusing is exactly the art of picking cherries.

4 The Hidden and the Unmarked

LS: You always had a penchant for analyzing "those ubiquitous aspects of everyday life that are normally taken for granted and therefore overlooked" (Zerubavel, 1985, p. XIII) — it had been so since your pioneering investigations of that social aspect ubiquitous-and-overlooked par excellence, i.e. time. Although Goffman had weirdly warned you in this respect ("you need to study an 'animal,' something

8. With regard to the consistent continuity of Zerubavel's work, it has been observed — for example — that "*The Fine Line* does for boundaries what *Hidden Rhythms* did for time: it makes the invisible more visible" (Spain, 1993, p. 1079).

9. Partially based on a previous article: see Zerubavel (2007).

you can watch, and you cannot watch time"[10]), the very term "hidden" appears in the titles of two of your books. Furthermore, your last field of research explicitly explores the seeming paradox of the (sociological) relevance of the (socially) irrelevant. What are the reasons for such a persistent intellectual fascination? Where does it come from?

EZ: Well, there are two ways to answer this, how did this idea come in my own life and how did it come in my academic path. As you rightly said, the odd thing was that Goffman, my inspirer, tried to dissuade me from the analysis of what's socially overlooked and imperceptible. I came up in my second year in graduate school with a possible dissertation project about punctuality, I prepared the whole explanation of the research and how to design it, but he barely listened and immediately interrupted me: "Punctuality is about time, you can't study time, you need an 'animal,' something that you can actually observe" (by the way, that zoological reference was not random since he was peculiarly open to ethology back then). I asked him sarcastically "Do you mean an animal like frame, encounter or stigma?" and he told me that it wasn't the way to do it. At the time, I thought he was kind of modest, now I see he was very arrogant, because he did opt for that approach and he knew perfectly well he was excellent at that. And that was precisely what inspired me, the prospect of investigating aspects which have nothing to do with the material structure of social situations, but only with their cognitive and nonphysical scaffolding.

However, my attraction towards the concept of taken-for-grantedness was also influenced by reading Harold Garfinkel, and even though the concept of taken-for-grantedness was brought into sociology by Schutz, I got to it first from Garfinkel, who flipped the original idea. In fact, Schutz was interested in how the intersubjective order is established and routinized, while Garfinkel tried to un-routinize it methodologically — I was very impressed by ethnomethodology.

Anyway, what you don't know is that when I was a child, I wanted to be an archeologist, so I guess that digging for the hidden and the unrevealed was already there. Besides, my wife, Yael Zerubavel, is a well-known student of cultural memory and her first book was about uncovering archeologically the hidden roots of ancient Hebrewness.[11] Speaking of that, let me say a word about the "hidden." Things can be different in terms of what appears and congruous in terms of what does not appear, and Jacob Bronowski considered the quest for the hidden likeness among elements that are seemingly unlike as the essence of scientific discovery.[12] This is also what Arthur Koestler described as "bisociation,"[13] a process through which two habitually separated realms are put together: it's what happens very often in humor, but I feel it should be done systematically in sociology as well, it should be articulated as a full-fledged research method.

LS: I'm going to be still more specific about your last book on the taken for granted. Non-events, nameless cultural elements, activities performed in such a way as to disregard the fact that something is being performed, all of this seem to be the stuff the vast majority of social reality is made of. Yet, despite this ontological prevalence, they are background-like, less salient and unmarked. Or rather, as you pointed out, unmarkedness is precisely a function of statistical/experiential prominence and social dominance.[14] Isn't that a crucial point, that we have more labels and concepts for what doesn't really matter to us, having — at the same time — less interest in what runs deepest with us?

EZ: Let me start by saying that I'm intellectually indebted to my former student Wayne Brekhus on this matter, because he has beautifully shown on several occasions why the mundane needs to be studied

10. Quoted in Shalin (2008, p. XX).

11. See Zerubavel (1995).

12. Bronowski wrote: "I found the act of creation to lie in the discovery of a hidden likeness. The scientist or the artist takes two facts or experiences which we separate; he finds in them a likeness which had not been seen before: and he creates a unity by showing the likeness (...) All science is the search for unity in hidden likenesses" (1956, pp. 40–41).

13. Koestler coined the term *bisociation* to account for "the perceiving of a situation or idea, L, in two self-consistent but habitually incompatible frames of reference, M_1 and M_2 (...) While this unusual situation lasts, L is not merely linked to one associative context, but bisociated with two" (1964, p. 35).

14. See Zerubavel (2018, pp. 32–59). Since the tendency is to mark and emphasize what is less frequently encountered, "it is the statistically aberrant that usually draws our semiotic attention. A widely used term implies that, perhaps counterintuitively, the phenomenon it denotes is culturally atypical" (Zerubavel, 2018, p. 19).

and considered central in social theory.[15] In the beginning, *Taken for Granted* was conceived as being together with *Hidden in Plain Sight* and only later I decided to split the project into two separate books. The initial aim was to hold together three theoretical perspectives which are quite similar to one another: the phenomenological sociological tradition coming from Schutz (centered on taken-for-grantedness); the structural linguistic and semiotic theory developed by Roman Jakobson (which is about unmarkedness); and the Gestalt psychological approach on the figure/ground relationship (that revolves around the concept of backgroundness).

I soon realized that it was going to be an artificial and awkward synthesis, and moreover I started concentrating specifically on the notion of background as a metaphor for the irrelevant, gravitating towards the issue of attention/inattention. So, I tackled firstly the topic of the background and how people socially construct irrelevance, and only in 2014 did I feel ready to write *Taken for granted*, which is more about unmarkedness. I tried to underline the fact that unmarked phenomena are culturally redundant and because of that they are semiotically superfluous (you can find disabled parking spots, not able-bodied ones; marked bike lanes, not explicitly designated car lanes; vegan menus rather than omnivore menus). Therefore, this is captured lexically, in our vocabulary, since we have concepts and terms only for those aspects of social reality which are marked, while the unmarked ones can "go without saying." I used the semiotics' perspective for stressing the connection between cultural redundancy and linguistic superfluity. We have an expression such as "openly gay," but its nominally equivalent counterpart "openly straight" is almost nonexistent. The supposedly generic "normality" is often manifested in namelessness, and normality and nameability are inversely proportional. By doing that, unmarked identities are normalized while marked identities are abnormalized. There is a symbiotic relationship between these twin processes.

LS: It seems almost as if taken-for-granted information is transmitted precisely by not transmitting information at all: it seems useless to do so since taken-for-grantedness can remain at an implicit level (what is "needless to say").

EZ: Absolutely, not communicating is communicating here. That's the asymmetry. There is, however, the possibility of what I called semiotic subversions.[16] Even though unmarkedness implies a degree of cultural invisibility and anonymity (and its socio-political dominance depends on that), it's still feasible to foreground the hitherto taken for granted, for example giving it a name — introducing the term "cisgender" and naming an identity who was nameless before, means putting the phenomena of transness and non-transnsess on an equal semiotic footing.

5 How to Study Taken-for-granted Realities? Listening to the Sound of Silence

LS: Among the preconditions for the development of social constructionist theses about a specific X, Ian Hacking (1999, p. 12) considered as a sine qua non that "in the present state of affairs, X is taken for granted; X appears to be inevitable." Now, the realm of take-for-grantedness pertains to what is taken as a given by definition, therefore the most urgent and pressing to unpack (exactly because it's not perceived as urgent or pressing at all). But how to deal with the fact that "we lack positive evidence for our lack of attention"[17]? The primary characteristic of taken-for-granted knowledge is that «it is never articulated as a specific knowledge»,[18] under the assumption that "objects *are* something rather than *are seen* as something"[19]: what are the implications, especially methodologically, of this?

15. See Brekhus (1998 & 2015).
16. See Zerubavel (2018, pp. 60–91).
17. Chabris & Simons (2009, p. 38).
18. Schutz & Luckmann (1973, p. 104).
19. Rogers (1981, p. 140).

EZ: First of all, I have to tell you that I heard several years ago Peter Berger speaking vehemently against being called a "social constructionist." Therefore, if *he* didn't want to be associated with that, I really don't know how I should feel. I'm anti-essentialist, but not relativist, I consider myself "relationist" in the Mannheimian sense.[20] The concept of social construction, like any other concept, has been cheapened through overuse, but it's a useful reminder that normality and hegemony are just products rather than something out there, inherently valid.

Anyway, to answer your question, I think Garfinkel is the pioneer student of absence in sociology. As Jamie Mullaney has demonstrated, sociologists can surely see not-doings, e.g. analyzing certain identities which are based on *not* doing something.[21] I'm a great believer that absence can be studied, that is necessary to look not only at what is going on, but also at what is *not* going on — why is a person not saying something or why is he refraining from acting? How do you study silence, background realities, and unmarked facts? Indirectly, like in Garfinkel's breaching experiments.

I began working on this stuff since *Hidden Rhythms*, whose first chapter is about the cognitive implications of temporal regularities. For example, what happens when something happens at the wrong time or doesn't happen at the right time.[22] It was in this sense that I started looking at worries (something should be there and it's not) or surprises (something should not be there and instead is). Even back in my dissertation, I focused a lot on the behavior of doctors and nurses who were perplexed and puzzled when they saw other staff members on the job in a different moment than their habitual coverage time slots.[23] Their surprised mode of behaving taught me that there were a right and a wrong time in the hospital life. You have to teach yourself to look obliquely at absence, looking at failures of presence/wrong time, and through that learning about presence/right time.

By the same token, if there is plenty of protest and complaint when a black actress is cast in a certain role, this suggests that when a character's racial identity is not specified in a play or a script, white actors are far more likely than non-white ones to be cast in that role. And although there can be a silence about silence, a meta-silence that forbids to talk about the fact that is forbidden to talk, silence itself can be deafening and clearly pregnant: you can hear it through fear, embarrassment or euphemisms. You may wonder why there is an abundance of euphemisms surrounding a specific topic, and so noticing that there is something people are silent about.

To pinpoint and foreground bad Gestalten, failed expectations, mismatches, discrepancies, and asymmetries is the best way for exploring what is taken for granted; I guess the Garfinkelian manner of studying the implicit by violating the explicit is still valid. Actually, I have a forthcoming article titled "Listening to the Sound of Silence: Methodological Reflections on Studying the Unsaid."

6 Public Sociology and Critical Sociology

LS: What strikes me the most about the study of the background, culturally invisible, aspects of social reality is that it forces sociologists to develop esoteric criteria of relevance and significance — analytical originality becomes a necessity, making it impossible to rely on alien standards of salience (for example, journalistic standards of newsworthiness) or just follow what is popularly trendy and currently

20. See Mannheim (1946, p. 270).

21. See Mullaney (2005).

22. Establishing the existence of certain default expectations regarding the temporal profile of social situations, that chapter aimed to show how the taken-for-grantedness of normal daily life is temporally anchored: "The temporal regularity of our everyday life world is definitely among the major background expectancies which are the basis of the 'normalcy' of our social environment. The fairly regular temporal structure of our social life is responsible for the establishment of some solid temporal ground against which the occurrence of certain events and the presence of particular persons and objects pass as normal and unnoticeable" (1981, p. 21).

23. "It is precisely the surprised glances at the clock and such questions as 'What are you doing here?' — which are essentially responses to 'pathological' situations — that were most helpful to me in discovering the 'normal' cognitive order which they presuppose and which is usually taken for granted, namely, the sociotemporal order of the hospital" (Zerubavel, 1979, p. 130).

appealing to the audience. Speaking of which, do you think that the "public sociology" emphasis has gone too far?

EZ: Ever since my doctoral dissertation, I found more challenging and exciting intellectually to observe the everyday and familiar and to illuminate it from a novel analytical perspective.[24] I think that Wayne Brekhus' "Mundane Manifesto"[25] presents one of the best approaches to this whole issue: if the ordinary constitutes the vast majority of social interactions, one can't belittle the study of what's ordinary, pigeonholing it and considering it exotic.

As far as the notions of popularity and trendiness are concerned, I must admit I have reluctance in accepting their implications. I remember back in the 1970s, when I was working on my dissertation, several colleagues felt compelled to give me "friendly advice" to pick another topic because time seemed to them unfashionable — you know, time!, one of the immortal things in philosophy. And of course it wasn't à la mode, but only for sociologists and just because sociologists have not addressed the issue of time adequately. It has taken decades for the sociological interest towards time to develop over the years, at a slow pace. Today I see that there's a greater interest, but this was not the case when I tried to develop a full-fledged sociology of time.

However, what has been called public sociology is basically what is supposed to hold the attention of the audience, it's a manifestation of the persuasion that sociologists have to be relevant to their public. Now, in my last book on taken-for-grantedness, I eventually get to the "holy trinity" (gender, race, sexuality) that I had deliberately avoided in my past books — the truth is that I found it so disturbing that those were almost the only things that sociology should investigate. Anyway, am I a public sociologist? Am I relevant to the public? I don't know, over my career I've got letters and emails from people who are part of the "public," people touched and impressed by my work, but I guess that this doesn't make me a public sociologist in the orthodox sense of the term.

I think the whole question boils down to the fact that sociologists should be interested in "social problems." Sociologists are trained to point to race, gender, and sexuality. In many sociology programs, Social Problems is a must course. How can you not take a class on race or gender? I mean, these are important topics, but I always tried to speak for (and deal with) those sociological subjects which are not considered popular, trendy or public. After all, Gusfield has demonstrated better than anyone that social problems are what come to be constructed as social problems.[26] I think that sometimes the attempt to be "publicly relevant" can be kind of gratuitous; I don't feel the need to advertise my work as public sociology in order to make it significant. Nevertheless, I always tried to write in a style that could be accessible to non-sociologists, abstaining from using sociologese and being exclusionary to the broader audience. But again, I guess I'm using the attribute "public" in a different sense than Burawoy's one.

LS: Your last two works are probably the most political you have penned. Which is paradoxical and revealing, considering that they were supposed to be centered on the irrelevant and the unremarkable. Don't you find it sociologically fascinating, almost compelling?

EZ: I have to disagree. I beg to differ with a lot of my colleagues on what can be regarded as "political." For example, my 1982 article on calendars and group identity is on the first schism in the Christian Church and the dispute between Rome and the Asian churches[27]: if this is not political, I don't know what's political. It's not political economy, I'll grant you that, but it's still highly political. So is my 1977 essay on the French Republican calendar and the most radical attempt in modern history to challenge the Western standard temporal reference framework — isn't that utterly political? And also *The Ele-*

24. See Zerubavel (1979, p. XVI).

25. The reference is to Brekhus (2000).

26. See Gusfield (1980).

27. That piece analyzed the introduction of an Easter cycle that would be entirely independent of the Jewish calendar (Easter Sunday and Passover Eve never coincide) as a Church's attempt to establish its own peculiar identity as distinct from the Synagogue, thus demonstrating the functions of temporal order as a unifier and as a separator: "On the one hand, in accentuating the similitude among group members vis-à-vis others, the calendar helps to solidify in-group sentiments and thus constitutes a powerful basis for mechanical solidarity within the group. At the same time, it also contributes to the establishment of intergroup boundaries that distinguish, as well as separate, group members from 'outsiders' " (Zerubavel, 1982b, p. 288).

phant in the Room is as political as it could be, although I can't tell you how many audiences I have not been able to convince of this. I remember giving talks on my work on the history and meaning of the week and hearing ardent replies like "What about the politics of all this?". Well, the politics is that both Robespierre and Stalin tried hard to destroy the seven-day week and eventually failed, attesting to the tremendous resilience of tradition in general and of religion in particular[28] — it's just that this is not commonly considered legitimate part of the domain of politics.

7 The Issue of Contempocentrism and the Canvas of the Present

LS: Speaking of politics, Mark Fisher recently came out with a book about the idea that "it's easier to imagine the end of the world than the end of capitalism."[29] One of your latest articles is focused on essentialism and its five pillars: doesn't it seem like one of these, "eternalism" (which you defined as the "fallacy of mistaking the historically specific for the truly eternal"[30]) is becoming more and more ingrained in the way we look at today's world? Might we be burdened by the same chronocentrism that led the Young Hegelian Arnold Ruge, in 1843, to complain to his friend Karl Marx that he would never see a political upheaval in his lifetime, only 5 years before 1848, "the year of revolution"?

EZ: It's such an interesting question, related to the manner you look at history and time. When you are telling this anecdote about Ruge, you are telling it 175 years after the event itself, with twenty-twenty hindsight, but back then Ruge himself could not possibly envision 1848. You know, that's the whole phenomenon of trying to anticipate how the present will be seen from the standpoint of the future as a past. I see contempocentrism as analogous to ethnocentrism: you take the culture (or the moment) you're living in as the baseline, the standard against which different cultures (or times) should be compared.[31] The notion of "always" is the historicized form of essentialism. An example which impressed me is a speech of the second lady Marilyn Quayle (the wife of Vice President Dan Quayle) who, at the 1992 Republican Convention, tried to "remind" people how the US used to be prior to the 1960s and 1970s, before the sexual and gender revolutions, before the Civil Rights movement. She talked as if it had always been like that in the past, but it actually used to be like that only during the 1950s, just between WW2 and the early 1960s, for a very short time. It can be cunning and deceptive, though, because not having been around at the time when something was introduced, people tend to assume that it has always been there.

However, analyzing in retrospect is absolutely legitimate and has no bounds (ex-post can be one month later, one year later, one hundred years later). I'm an epistemic pluralist here. Simmel has this beautiful image of the distance from which you can choose to observe a painting.[32] If you want to focus on the brushstroke or the texture you would prefer maybe a few inches; if you rather want to see the composition you would locate yourself several feet away, if you want to assess how the painting fits on the wall of a gallery you would need to go back further. I guess it's the same for studying historical moments — there is no correct, Archimedean point of view about 1843.

LS: But are we today too close to the canvas of the present?

28. See Zerubavel (1985, pp. 27–43).

29. See Fisher (2009, pp. 1–11).

30. Zerubavel (2016, p. 73).

31. "The common 'contempocentric' tendency to mistake currentness for correctness» (Zerubavel, 1997, pp. 28–29) seems to be closely tied to the prejudice that what occurs recently is more high-impact than what went before and to the stubborn idea of an alleged 'uniqueness of the present moment' " (Kracauer, 1969, p. 63).

32. "We obtain different pictures of an object when we see it at a distance of two, or of five, or of ten yards. At each distance, however, the picture is 'correct' in its particular way and only in this way. (...) If the minute detail of a painting that we gain at very close range were injected into a perspective gained at a distance of several yards, this perspective would be utterly confused and falsified. And yet on the basis of a superficial conception, one might assert that the detailed view is 'truer' than the more distant view. But even this detailed perception involves some distance whose lower limit is, in fact, impossible to determine. All we can say is that a view gained at any distance whatever has its own justification. It cannot be replaced or corrected by any other view emerging at another distance" (Simmel, 1950, pp. 7–8).

EZ: You know, we are always near the canvas of the present, but it is important to keep in mind that the present is dreadfully fleeting, it's just a fraction, as soon as we start talking about the present it's already in the past, in the memories. Speaking of which, to me, it's particularly interesting to study how the past has been studied, a point of view which is not so much about the past as it is about the present. It's looking at the looker, just as in Frances Fitzgerald's book about American history textbooks.[33] When you analyze cultural memories the rememberer is often more interesting than the things which are remembered.

8 The Rutgers School of Sociology

LS: There is another facet of your life as a social scientist I would like to touch on in particular. It's difficult to overstate the scientific significance of training and mentoring new students, transmitting knowledge across generations, building Fleckian thought-collectives. In this respect, you did not just socialize an entire cohort of scholars to a distinctive sociological vantage point, but this very group of your former students identify themselves as "Rutgers School sociologists." Did all of this occur without planning, as time goes by, or was something deliberately constructed?

EZ: I have mixed feelings about what you're asking. Part of me wants to say no, this occurred without planning, but the other part would like to affirm that there has been a bit of strategic planning, a sort of groundwork. It has been unplanned in the sense that the very term "Rutgers School" is something introduced by Wayne Brekhus,[34] I have never thought of putting it this way. However, there's an element which may be similar to planning. As I said, I've chaired twenty dissertations. Once I moved in 1985 from Columbia to Stony Brook (and three years later to Rutgers), I came to a department that didn't have a "big" name. Students therefore had to publish more before going on the job market. Thus, when I was about to leave Stony Brook, starting with Christena Nippert-Eng, who was working on her dissertation with me, I said "Can we make a deal and not talk about a dissertation, but right from the beginning about a book?", meaning I could help save her the several years when people revise and revise dissertations, and try right away to produce something quite ready to be a book, you know, without the literature review section or the methodology chapter.

That was the first (in the late 1980s) of a lot of projects of the same type, thematically-driven works, not confined to a specific substantive setting. It's 30 years since launching this tradition, and in this respect you can talk of a certain planning. Not planning in terms of a "school," but rather as building scholars' careers. My advice is that of taking chances, but especially having the sense of "the big picture," embarking on projects that are bookable and thinking in long-terms. You know, when I graduated from the University of Pennsylvania, my first appointment was in an applied research job which I was not particularly excited about, but a few years later I was hired as an associate professor at Columbia. The difference was my publications: suddenly, the fact the I came from a certain university was not as significant as the fact that I published in *AJS*, *ASR*, *Social Forces*, and so on. My point is that a career is way more complex than getting one's first job, which can be just a step to something else — of course, if you write excellent stuff. That's exactly what I try to supervise: mostly, how to make decisions about what to write about.

9 Intellectual Relationships, Academic Milieus, and Human Connections

LS: Apropos of masters, pupils, and the way in which past knowledge is selectively transmitted: you have had the chance to be trained by or acquainted with outstanding scholars (Renée Fox, Erving Goffman, Peter Berger among others). What was the intellectual ambiance at the time? What sorts of things did you learn as their student that you might not have learned from their writing alone?

33. The reference is to Fitzgerald, 1979.

34. See Brekhus (2007).

EZ: I struck up a real friendship with Peter Berger that goes back to when he wrote a thankful letter to me because I had his son in my course "Symbolic Interactionism and Phenomenological Sociology" at Columbia University, even though at the time I didn't know he was his son (it's funny that he had not read his father's works before my class).

To answer about Goffman, I have to go back to what I've said before, about his signature way of looking at the social world. You know, two years after he died, some of us (Goffman's former students like Sam Heilman, Gary Alan Fine, Sherri Cavan — this last one, the "only one real student" that Goffman once said to ever had) had a session at the ASA annual meeting on what we learned from Goffman. What was amazing was that all of us mentioned the same thing, without preparing it in advance: the fact that we learned how to look, something that we would have never guessed from his writings only. Moreover, he never explicitly taught us anything about analytical gaze, sociological eye and similar stuff. We acquired this skill only by watching him in action: how to look at the most micro-micro-microscopic situations and visualize invisible dimensions which you couldn't have seen otherwise.

It's hard to put it into words; I'll give you an example of how I was shaped as an observer thanks to Goffman. During the fieldwork in the hospital for my dissertation, I noticed one day a nurse come out of the nurses' station, walk like 30 feet through the main corridor of the unit where there was a clock on the wall; she looked at the time, came back to the station and then sat down. Sounds very trivial, except that she was wearing a watch (this was 1975, when people were still wearing wristwatches), and she did all that journey regardless. Then I started noticing it again also with other residents, walking down the hallway and doing a 180-degree turn of the neck to look at the clock, and they were wearing watches as well. I realized that all of this happened only when they needed to coordinate themselves morally with each other. Arriving late or leaving early were moral offenses, violations of a moral code. And the moral is the social: if I come late, who cares what my watch shows, even if it says I'm early I still came late socially. That is, wall clocks have a social function. To my knowledge, apparently no one else has ever written about that before. I wasn't the greatest observer, though. I could spend a whole night there without noticing that one patient died, for example. But analytically, I was sensitized to see the distinction between what's on the clock and what's on the watch: Goffman may have dissuaded me from studying time, but he modeled my observational style anyway.

However, you didn't mention three other scholars who I consider extremely important in my development as a sociological theorizer. One person is Kai Erikson. We have a 42-year friendship which started when I was interviewed for a job at Yale, a job that I never got, but I got a friendship with Kai, a very stimulating collegial relationship. He's seventeen years older than me, so it has always been an intergenerational relationship and because of that it has played a key role in my life's path; you know, seeing how a senior colleague interacts with junior scholars. I mean, later I learned also to appreciate his work itself: the first chapter of his *Wayward Puritans*[35] has been super-important, it clicked it for me to write *The Fine Line*. But what I learned from the books is very different than what I learned from the person, i.e. his intergenerational generosity.

The second person I would mention is Lewis Coser, who I was hired to replace in Stony Brook, but there was a year of overlap. I came from a department, at Columbia, ruled by Robert K. Merton, and I found Lew Coser who was almost the same age as Merton but never behaving in that regal, imperious way. I mean, he was 35 years older than me (that's almost three academic generations); the generosity he showed to me as a younger colleague was incredible. Again, his sociological works influenced me a lot, but it was the personal "touch" that I adored.

The third person is Murray Davis. When I read his book *Smut: Erotic Reality/Obscene Ideology* I discovered a kindred spirit, someone who was equally influenced by Simmel, Goffman, Mary Douglas, Edmund Leach, that could combine symbolic interactionism with phenomenology, anthropology with the sociology of knowledge. When the journal *Contemporary Sociology* did a special issue on its twenty-fifth anniversary I chose to review *Smut* as the most neglected book of the last decades.[36] Then, I met him in person, right after I was denied tenure at Columbia, when I felt the world was collapsing on me. I was very close to leaving academia, filled with revulsion, but eventually, I decided to continue. Instead, Davis

35. See Erikson (1966).

36. See Davis (1983) and Zerubavel (1996b).

left academia after he was denied tenure at the University of California San Diego: officially rejected by academia, he kept writing great pieces of sociology such as *What's So Funny?*.[37] To me, it has been refreshing to see how someone can be motivated from within, in spite of all the injustices.

Erikson, Coser, Davis — for all of them it's way beyond the work itself, it's rather something in their personal style that I took from meeting them. You know, both with Erikson and Coser there was a big age gap: in a way, they have been sort of father figures — and I really needed that, since I've had a very bad father and a very bad mentor (as a father figure) in Goffman. That kind of support towards someone younger was crucial to me, and you don't learn this in methods courses.

LS: Still on the subject of personal experience, you've worked in different sociology departments, studying at Penn, then working at universities like Pittsburgh, Columbia, CUNY's Queens College, Stony Brook, and Rutgers. What was the experience of developing your sociology and intellectual style in these different environments?

EZ: Penn, Stony Brook and Rutgers were the sociologically important ones for me. The dimension on which I would compare them has to do with the tolerance towards exotic, heterodox, strange stuff. I'm saying this with a smile because I don't think that my works are inherently exotic or exoteric, but they have been treated as such within sociology for quite a long time. I got my doctoral degree 42 years ago and I have never been part of the mainstream: yes, sure, I got my accolades, but always without being in the mainstream. This is something that has its disturbing consequences (I paid for it), but I somehow chose it to be my way: therefore, the point is how this approach is tolerated by the environments in which you operate.

In the previous question, I forgot to talk about Renée Fox. She was very influential in my decision to study time: not only she accepted my decision, but she even encouraged it (in many other departments, this would not have been possible). Renée was the chair of the department and she put her mark on it, in terms of being particularly receptive and welcoming. Only later, over the years and after hearing other people's experience, I've realized how unusual Penn was during Renée Fox's time as chair. Stony Brook and Rutgers as well have been comfortable with my alleged strangeness, letting me shape many cohorts of students. It was Columbia that was very different and made me feel awkward with my sociological perspective and intellectual path.

LS: If I may ask, would you mind saying something more about your relationship with Robert K. Merton?

EZ: Well, there were several Mertons. You know, he was a magician when he was young. Magicians are known to manipulate realities so that what you see on the outside is very different from what there is on the inside. While the appearances were a lot of admiration for my work (he used to talk openly about what great work I did and so on), behind the scenes he acted very differently. There's no way I would have been denied tenure if he had supported me. Merton liked super-servile lackeys around him. He required a degree of personal loyalty that I was never ready to give him. I mean, I appreciated him intellectually. Actually, during my time at Columbia, the department of sociology was full of Simmelians — between him, Peter Blau, Viviana Zelizer (one of the few "lights" for me at Columbia), Allan Silver, it was a heaven for a Simmelian. But personally, it was different.

LS: Elias once said that "one has the impression that in recent times sociologists no longer expect that one can make basic discoveries in their field of work."[38] Do you share this feeling, or do you think, instead, it could be just a perspective bias? What recent underappreciated/underrated sociological works have really impressed you? Have you been influenced by other sociologists in recent years?

EZ: When you say "discoveries," I have to bring back a distinction that I once drew, the differentiation between "factual novelties" and "intellectual novelties."[39] The novelties that attract me

37. See Davis (1993).

38. Elias (1987, p. XXV).

39. "Whereas the ethnographer has been traditionally motivated by the desire to know *more* about the social world [discoveries of 'factual novelties'], the analytical field researcher would most likely be motivated by the wish to know about it *in different ways* [discoveries of 'intellectual novelties']. Whereas the former has been concerned primarily with the contents of social

the most are the intellectual ones. I read very little descriptive works or works related to a specific period/context/situation only. I much prefer those analyses that move between different settings, different theoretical perspectives, different epistemological paradigms. This is why I mentioned Murray Davis: I loved his work because it is the closest thing to what I think sociology should be. I don't believe one can be non-perspectival, but it's possible to be multi-perspectival, and this is what's so amazing about Davis' *Smut*. You asked about underrated sociologists and Murray Davis is absolutely the most underappreciated US sociologist, there's no doubt. With regard to underestimated sociological works, I'll tell you something: I've had several students whose books are still underrated — Christena Nippert-Eng's *Home and Work* (1996); Wayne Brekhus's *Peacocks, Chameleons, Centaurs* (2003); Jamie Mullaney's *Everyone Is NOT Doing It* (2005); Asia Friedman's *Blind to Sameness* (2013); Thomas DeGloma's *Seeing the Light* (2014). I think they are all underappreciated. It doesn't mean, of course, they are not appreciated at all (they got acclaims, jobs and so on), but I do believe they deserve much more appreciation. I mean, they are not the only ones, but just the ones I know best.

10 The Present and the Future of Sociology

LS: One final question: how do you see the future of sociology? What do you think are the great institutional issues confronting the discipline today? What sorts of theoretical questions do you think we should be focusing on in order to assure that sociology survives as a discipline into the Twenty-second century? And foremost: will sociology survive at all?

EZ: Well, to answer I would like to go back to a conversation I had with Peter Berger in the early 1980s. I asked him "How come you never attend sociological meetings like the American Sociological Association annual conference? Why don't you come? And why don't you publish in sociology's journals anymore?" and he said he doesn't like the discipline. I replied, "You are one of the pillars of this discipline, there are plenty of people who have come to sociology because of your work, including myself." He said, "It's all going down, it's too ideologized now." I told him, and this is my answer to your question, that I see a distinction between sociology as a discipline (if you like, as a field) and sociology as a perspective. I really think that as a perspective sociology is unique and irreplaceable. And I believe sociology should be better seen as sociologies, in the plural rather than singular form, because it is similar to an octopus, it does not have a hegemonic paradigm.

I don't even know if the mainstream (meaning the positivism) is the majority today. Sure, it's still dominant: if you want to publish in the *ASR* and *AJS*, you can publish things like my own only as tokenism, something that they need to showcase once in a while. Unfortunately, there's still the big split of "qualitative vs. quantitative" sociology, lexically helped by Glaser and Strauss (I did try to dissuade Barry Glassner from titling a journal *Qualitative Sociology*, as he did in the late 1970s). I find that's a silly distinction, but sociology today is even more fragmented, and the "field" is moving into several fields. In this sense, I'm not really sure about the future of sociology. It's easier to predict the future of academia, speaking of institutional issues. Things are very sad so far, and academia is not going to be the same: I couldn't even imagine its present situation when I entered it in the early 1970s. It is more and more corporatized, it has to do with the politics of science and little or nothing with intellectual dimensions.

You know, there's a lot of cynicism today in academic sociology, I share some of the cynicism but not to the point of, say, refraining from encouraging a young, shining undergraduate. A lot of sociologists would probably say "Twenty-second century? We don't even know if it gets to the middle of the Twenty-first century!". It's a dark view, which I don't completely share, but the reality is not promising.

References

Ben-Yehuda, N. (2007). Review of "The Elephant in the Room." *American Journal of Sociology*, *112*(4), 1246–1248. https://doi.org/10.1086/513538

life, the latter would most likely be concerned mainly with the formal patterns that underlie it" (Zerubavel, 1980, p. 29).

Berger, P. (1992). Sociology: A Disinvitation?. *Society*, *30*(1), 12–18. https://doi.org/10.1007/BF02719100

Blumer, H. (1954). What is Wrong with Social Theory?. *American Sociological Review*, *19*(1), 3–10. https://doi.org/10.2307/2088165

Brekhus, W. (1998). A Sociology of the Unmarked: Redirecting Our Focus. *Sociological Theory*, *16*(1), 34–51. https://doi.org/10.1111/0735-2751.00041

Brekhus, W. (2000). A Mundane Manifesto. *Journal of Mundane Behavior*, *1*(1), 89–106.

Brekhus, W. (2003). *Peacocks, Chameleons, Centaurs: Gay Suburbia and the Grammar of Social Identity*. Chicago: University of Chicago Press.

Brekhus, W. (2007). The Rutgers School: A Zerubavelian Culturalist Cognitive Sociology. *European Journal of Social Theory*, *10*(3), 448–464. https://doi.org/10.1177/1368431007080705

Brekhus, W. (2015). *Culture and Cognition: Patterns in the Social Construction of Reality*. Cambridge: Polity Press.

Bronowski, J. (1956). *Science and Human Values*. New York: Messner. https://doi.org/10.1111/j.1468-2273.1956.tb00909.x

Chabris, C., & Simons, D. (2009). *The Invisible Gorilla: And Other Ways Our Intuitions Deceive Us*. New York: Random House.

Davis, M. (1983). *Smut. Erotic Reality/Obscene Ideology*. Chicago: University of Chicago Press. https://doi.org/10.7208/chicago/9780226162461.001.0001

Davis, M. (1993). *What's So Funny? The Comic Conception of Culture and Society*. Chicago: University of Chicago Press.

DeGloma, T. (2014). *Seeing the Light: The Social Logic of Personal Discovery*. Chicago: University of Chicago Press. https://doi.org/10.7208/chicago/9780226175911.001.0001

Durkheim, É. (1982). *The Rules of Sociological Method*. New York: The Free Press. (Original work published 1895/1901). https://doi.org/10.1007/978-1-349-16939-9

Elias, N. (1987). *Involvement and Detachment*. Oxford: Blackwell. (Original work published 1983).

Erikson, K. (1966). *Wayward Puritans: A Study in the Sociology of Deviance*. London: Wiley.

Fisher, M. (2009). *Capitalist Realism: Is There No Alternative?*. London: Zero Books.

Fitzgerald, F. (1979). *America Revised: History School Books in the Twentieth Century*. Boston: Little, Brown & Co.

Friedman, A. (2013). *Blind to Sameness: Sexpectations and the Social Construction of Male and Female Bodies*. Chicago: University of Chicago Press. https://doi.org/10.7208/chicago/9780226023779.001.0001

Gusfield, J. R. (1980). *The Culture of Public Problems. Drinking-Driving and the Symbolic Order*. Chicago: Chicago University Press.

Hacking, I. (1999). *The Social Construction of What?*. Cambridge, MA: Harvard University Press.

Hofstadter, D., & Sander, E. (2013). *Surfaces and Essences: Analogy as the Fuel and Fire of Thinking*. New York: Basic Books.

Katz, J. (2016). Review of "Hidden in Plain Sight." *Contemporary Sociology*, *45*(5), 673–674. https://doi.org/10.1177/0094306116664524fff

Kearl, M. C. (1992). Review of "The Fine Line." *American Journal of Sociology*, *98*(3), 696–698. https://doi.org/10.1086/230075

Koestler, A. (1964). *The Art of Creation*. London: Hutchinson & Co.

Kracauer, S. (1969). *History. The Last Things Before the Last*. New York: Oxford University Press.

Mannheim, K. (1946). *Ideology and Utopia. An Introduction to the Sociology of Knowledge*. London: Kegan Paul.

Melbin, M. (1983). Review of "Hidden Rhythms." *American Journal of Sociology*, *88*(6), 1338–1339. https://doi.org/10.1086/227832

Mullaney, J. (2005). *Everyone is NOT Doing It: Abstinence and Personal Identity*. Chicago: University of Chicago Press.

Nippert-Eng, C. (1996). *Home and Work: Negotiating Boundaries Through Everyday Life*. Chicago: University of Chicago Press. https://doi.org/10.7208/chicago/9780226581477.001.0001

Rogers, M. (1981). Taken-for-Grantedness. *Current Perspectives in Social Theory*, *2*, 133–151.

Schutz, A., & Luckmann, T. (1973). *The Structures of the Life-World* (Vol. 1). Evanston: Northwestern University Press.

Shalin, D. (2008). Eviatar Zerubavel: Studying with Erving Goffman. *Bios Sociologicus: The Erving Goffman Archives*, 1–16. Available at: https://digitalscholarship.unlv.edu/goffman_archives/78

Simmel, G. (1950). *The Sociology of Georg Simmel*. Glencoe, IL: The Free Press. (Original work published 1908).

Spain, D. (1993). Review of "The Fine Line." *Social Forces*, *71*(4), 1079–1080. https://doi.org/10.2307/2580132

Zerubavel, E. (1977). The French Republican Calendar: A Case Study in the Sociology of Time. *American Sociological Review*, *42*(6), 868–877. https://doi.org/10.2307/2094573

Zerubavel, E. (1979). *Patterns of Time in Hospital Life: A Sociological Perspective*. Chicago: University of Chicago Press.

Zerubavel, E. (1980). If Simmel Were a Fieldworker: On Formal Sociological Theory and Analytical Field Research. *Symbolic Interaction*, *3*(2), 25–34. https://doi.org/10.1525/si.1980.3.2.25

Zerubavel, E. (1981). *Hidden Rhythms: Schedules and Calendars in Social Life*. Chicago: University of Chicago Press.

Zerubavel, E. (1982a). The Standardization of Time: A Sociohistorical Perspective. *American Journal of Sociology*, *88*(1), 1–23. https://doi.org/10.1086/227631

Zerubavel, E. (1982b). Easter and Passover: On Calendars and Group Identity. *American Sociological Review*, *47*(2), 284–289. https://doi.org/10.2307/2094969

Zerubavel, E. (1985). *The Seven-Day Circle: The History and Meaning of the Week*. New York: The Free Press.

Zerubavel, E. (1991). *The Fine Line: Making Distinctions in Everyday Life*. New York: The Free Press.

Zerubavel, E. (1996a). Lumping and Splitting: Notes on Social Classification. *Sociological Forum*, *11*(3), 421–433. https://doi.org/10.1007/BF02408386

Zerubavel, E. (1996b). Review of "Smut." *Contemporary Sociology*, *25*(4), 462–463. https://doi.org/10.2307/2077072

Zerubavel, E. (1997). *Social Mindscapes: An Invitation to Cognitive Sociology*. Cambridge, MA: Harvard University Press.

Zerubavel, E. (2003). *Time Maps: Collective Memory and the Social Shape of the Past*. Chicago: University of Chicago Press. https://doi.org/10.7208/chicago/9780226924908.001.0001

Zerubavel, E. (2006). *The Elephant in the Room: Silence and Denial in Everyday Life*. New York: Oxford University Press. https://doi.org/10.1093/acprof:oso/9780195187175.001.0001

Zerubavel, E. (2007). Generally Speaking: The Logic And Mechanics of Social Pattern Analysis. *Sociological Forum*, *22*, 131–145. https://doi.org/10.1111/j.1573-7861.2007.00010.x

Zerubavel, E. (2011). *Ancestors and Relatives: Genealogy, Identity, and Community*. New York: Oxford University Press.

Zerubavel, E. (2015). *Hidden in Plain Sight: The Social Structure of Irrelevance*. New York: Oxford University Press. https://doi.org/10.1093/acprof:oso/9780199366606.001.0001

Zerubavel, E. (2016). The Five Pillars of Essentialism: Reification and the Social Construction of an Objective Reality. *Cultural Sociology*, *10*(1), 69–76. https://doi.org/10.1177/1749975515616905

Zerubavel, E. (2018). *Taken for Granted: The Remarkable Power of the Unremarkable*. Princeton: Princeton University Press. https://doi.org/10.23943/9781400889464

Zerubavel, Y. (1995). *Recovered Roots: Collective Memory and the Making of Israeli National Tradition*. Chicago: University of Chicago Press.

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How Did I Become a Historical Economic Sociologist? Viviana Zelizer in Conversation with José Ossandón

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Abstract

In this interview, Viviana Zelizer discusses three dimensions that characterize her distinctive sociological approach to the economy. First, Zelizer explains how she made archival historical analysis her sociological method. Second, she talks about the relational character of her objects of study. Third, Zelizer discusses the part played by gender politics in her work.

Keywords: Economic Sociology; Zelizer; Sociology of Money; Historical Methods; Gender.

Acknowledgements

The interview was conducted in 2010, when Viviana Zelizer visited the Universidad Diego Portales in Chile. The original conversation was in Spanish and video recorded (the video is available here: https://estudiosdelaeconomia.com/2011/01/04/para-una-aproximacion-relacional-e-historica-de-la-economia-una-entrevista-con-viviana-zelizer/). It has not been published before. It was especially transcribed and translated for *Sociologica*. I thank Macarena Barros and Francisca Lillo Razeto for their help with the transcription and translation. I am very grateful for Viviana's help in this process. I am also very grateful for the way in which Viviana has advised and encouraged me and many other researchers of my generation over the years. I hope I can learn a bit from her generous model of how to inspire and help younger scholars.

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1 Introduction

When I conducted this interview, the idea was to use it as a means of introducing Viviana's work to a wider readership.[1] Accordingly, instead of focusing on the most recent work, the conversation covered her oeuvre more generally. This is why, or so I hope, it works, still today, as a good introduction to some of the key aspects that make Zelizer's approach to the study of economic life so particular. The interview covers three main dimensions in Zelizer work: how she uses historical archive methods, the construction of relational objects of analysis, and the gender politics implicit in her more recent books.

2 Sociology and Historical Archives

José Ossandón (JO): We, sociologists, tend to classify ourselves in terms of the methods we use. There are the "quantitative ones" that conduct surveys, do network analyses, structural analyses, modelling, etc., while "qualitative sociologists" are generally associated with in-depth interviews and ethnography.

Viviana Zelizer (VZ): And I am neither of them. [Laughs].

JO: Indeed. But, if one has to classify your work in one of these two categories, it would surely be qualitative. However, you have chosen historical archival work, and not observation or interviews, as your favourite method of data collection and inquiry. The first question is: how did you become an archival researcher?

VZ: The main method — up to the book *The Purchase of Intimacy* (Zelizer, 2005) — had been historical, based on archival research. It is, basically, the approach a historian might use. But the theoretical part is different, the questions that lead the inquiry are different from the ones an orthodox historian might pose. It all began by accident. Although I would have probably still relied on qualitative methods, like interviews, something like that. I was a PhD student at Columbia and in the second year, I was awarded a scholarship from the NIMH (Social History Traineeship Program, National Institute of Mental Health) designed to train students of history in social scientific methods and social scientists in historical methods. There was a group of five graduate students in history and us, two women, the only sociologists, who were trained in historical methods. The scholarship lasted four years, they covered all your expenses under the condition that the thesis was to be written on American history. To be honest, until then for me, history consisted of boring dates about one particular event after another, dates and events, which I would forget anyway. But, then I discovered social history, and I was fascinated by it. As a result, I did the thesis on life insurance, with the question of why there was so much opposition to life insurance in the United States in the nineteenth-century (Zelizer, 1979). It was social history. I don't want to talk too much about it, but this was in a period when historical methods in sociology were not yet established. Someone told me "you're never going to get a position with that kind of research!" Only later on did it become fashionable. I can tell you more about the method if you're interested, or should I stop?[2]

JO: Yes, please continue. How did you go on using archival historical methods and what are the advantages you see in it?

VZ: There are two features of how I applied the historical method. The first one and obvious is that it is done through analysis of historical documents. And, the second one, which was not deliberate but emerged from the investigations is a focus on social conflicts. In the case of insurance, the debate was over whether it was morally acceptable to insure human life. And, then, in a way, every book was born from the previous one. Also by chance, when I was writing the book on life insurance [*Morals*

1. Even though Viviana Zelizer was born in Argentina, her academic career has taken place in the US. It was only in the late 2000s that her books started to be translated into Spanish (*The Purchase of Intimacy* was published in Spanish in 2009 and *The Social Meaning of Money* in 2011).

2. Since 2011 Zelizer has been conducting for the first time personal interviews for a study of the college economy (Zelizer & Gaydosh, 2019).

and Markets: The Development of Life Insurance in the United States (Zelizer, 1979)], I discovered a note about a controversy regarding children's insurance. This became the clue that led me to the second book [*Pricing the Priceless Child: The Changing Social Value of Children* (Zelizer, 1985)], which then expanded to other topics related to childhood. This book was also about a social conflict (this time, it was over the valuation of children's lives) and drew from historical data. Then came the book about money [*The Social Meaning of Money* (Zelizer, 1994)]. When I studied children's history, I became fascinated by the whole issue regarding allowances, children's allowances, and combined with my earlier interest in "death money" connected to life insurance led me to the study of money. Now, when I started to write *The Purchase of Intimacy* (Zelizer, 2005), the plan was to pursue an historical analysis in a period I was already very familiar with, from 1870 to 1930. I expected to focus on cases of legal disputes regarding issues concerning negotiations of intimacy. And, I have a collection of cases from that period! But then I realized that doing such a detailed historical analysis was not what interested me at that time. So, the method changed again. This time the legal part, case selection, was more opportunistic and strategic. I read hundreds of cases and then picked the cases that really portrayed the theoretical problem of the mixture of intimacy and the economy and what happened when there were legal conflicts, how the law saw this mix. So, of course, there is historical data, but there are also important changes in the method. What I have never done are quantitative analyses. It is not my style, it's not how I think.

3 Relational Approach and Economic Sociology

JO: You started to publish your first books in the late 1970s and early 1980s. This was a context when economic sociology was associated mainly with a structural approach, with a focus on embeddedness and the study and modeling of social networks. We could say that an "objective" or positivist stance characterized the most influential sociological studies of the economy. Within that scenario, your work appears as a very different type of research, deeply qualitative and case based. Now, one would generally expect someone who uses qualitative methods to be more focused on how actors *think*, their sense-making or their actions. However, your focus is different. Particularly in your last few books, you have started to pay more and more attention to these little social formations, that is, the "circuits of commerce." But, I would say, even in your previous work on social conflicts, you made controversies, moral frictions and tensions your object of inquiry. You seem to be more interested in relations or relational objects (circuits or tensions) than in what people think. Thus, one might think, in this context, that there is another way to connect your work and the other economic sociology that focuses on networks. For example, lately, people speak of a "cultural turn" within the sociology of networks, notably in Harrison White's (2008) latest work...

VZ: Definitely.

JO: ...where networks are not so "cold" anymore, but where scholars pay more attention to how stories matter, and how these stories might create symbolic barriers and how actors manage to switch and construct their identities in the crossings between these different stories. So, my question is whether, in this context, you see different, and perhaps better, conditions for a dialogue between your work and other economic sociologies?

VZ: Your question is perfect, because it places me exactly where I am. I discuss a little bit about that in the introduction of *Economic Lives* (Zelizer, 2011). Placing me as a historical character, which is not that interesting, but since we are talking about this... When I wrote my PhD thesis — which later became *Morals and Markets* — I didn't have a clue that I was contributing to economic sociology. There was, as you know, the old economic sociology of Weber and the classics. But the "New Economic Sociology" had just begun in those years, with Granovetter, and, just as you say, in a form that was emphatically structural, and by decision of those who contributed to that, it was an explicit choice to differentiate themselves from Parsons, they didn't want to speak of Parsonian values and culture. Now they have changed, just as you say. In those times, what I was really interested in was the question of how human life and economic value are mixed. And, I think, but I am not sure, so I can't prove it (so, I didn't

write it in the introduction of the book), that a great part of my integration in economic sociology, now more mainstream, has a lot to do with Richard Swedberg. Swedberg, who is someone who has expertly codified the economic sociology terrain, categorized me, at the very beginning, as the only, basically, along with Paul Di Maggio, "cultural ones." So, it wasn't a sort of a "Oh! I'm contributing to economic sociology!" kind of decision. On the contrary, it happened slowly, and, clearly, what I was doing was peripheral, not just in relation to the structural methods, but also in relation to the topics of research. The main topics, which have continued to be so, are the firms, companies, corporations, are not the kind of world I that have explored. That's why, in a certain way, there's room for this last book [*Economic Lives*], because my thematic focus has been different too. Now that you have said it, it's completely right. Despite the fact that I love to understand the way people think, both in reading academic work or even in ordinary interactions as when I listen to a taxi driver who tells me has been married for sixty years, I have not focused on individual stories. That is true, and more and more so, but what you've made me see is that also from the beginning and not just lately, my focus has been on social relations, social connections. In a way, I overdid it, I put much more emphasis on that. One person who influenced me very much, after I had written several books, was Charles Tilly and my friendship with him. He was a relational absolutist. Our conversations led me more in that direction. But, as you said, there was already something of it, in the sense that it wasn't an individualistic psychological analysis of what was going on, which could've perfectly well been the case. What I can see now, in a certain way, is that there is a world of young people in economic sociology that understand what I'm doing. In some kind of way, they are moving towards the cultural side or towards meaning and content of social ties. Harrison White, I agree. The others... Well, I would also say that Mark Granovetter has written about the cultural side. He is a brilliant man who understands that that is important too, but, because of strategic intellectual reasons, he adopted such an emphatically structural method. About circuits, yes, but I don't know if all of that makes me closer to the type of study of firms... Theoretically, it should put me closer, but, thematically, it is so different from what they're doing. But what I do observe is that younger researchers take the concepts of "commercial circuits" and "relational work" and understand them. They use them and make these concepts useful as a way of thinking social and economic processes they are observing and don't know how to explain. That's what I see. I don't know, you do what you can.

4 A Feminist Politics?

JO: My last question. Especially in your book *The Purchase of Intimacy* (Zelizer, 2005) you emphatically argue against what is perhaps the most influential stance in the social studies of the economy. What you call "hostile worlds," where money and the social are conceived as separate spheres, and most of the attention focuses on how money commoditizes, transforms, and pollutes the social world. You point out the limits of such an approach — as you also discuss the limits of what you term the "nothing but" approach, where the assumption is that actors follow the same goals wherever they are situated. Now, my question is about the politics in your work. My impression is that, while you clearly stand against the politics attached to the hostile world stance (namely, analysts that write from a critical position towards the commoditization of the social), there is in your work a, perhaps more subtle political stance. What I see is that it has a lot more to do with gender politics, and how the expansion of money does not always mean more subjection or domination, but, it could even be the other way round. The stories you tell, for instance regarding how money is managed at home and how domestic relations in relation to money at home have changed over time, narrate a political history, but, perhaps, a less explicit political story. My question is whether this is true, and how central is this political stance in your work?

VZ: Yes. It could be that due to my own gender identity I haven't become an explicit political scholar. Or, it could also be the influence of American sociology that is not political in general. I mean, if I would have pursued a career in Buenos Aires... Well, I wouldn't have [laughs]... In those times, sociology was a problematic field and I think the specialty was actually banned for some time. But, yes, there is an implicit political agenda. And in some kind of way, that agenda is more alive now than in the previous books. What I mean is that the concern with market dominance or expansion blocks more detailed and

important questions of how to build fairer markets. I understand that saying "to build fairer markets" sounds good. The challenge is how to do it. But that's another agenda. My agenda is to dispute simplistic causal assumptions. One might feel good when saying that money is awful, that markets oppress. You can easily say that, but neither markets nor money are going anywhere. We need to understand how they work, when are markets and money destructive but when do they enhance welfare and solidarity. To think like that, at least, opens up your mind to start considering those options. I have a very interesting quote, from a Brazilian economist, who is actually very critical of the system, but, who said something very similar. This is the theoretical step. Then, the next step is, of course, what many people are trying to do, but the direction is different.

JO: And, in that sense, gender...

VZ: Exactly. The purpose of changing direction goes beyond gender, but, within my work, what I can clearly see, and that has made me closer to more practical things, is the gender issue and, above all, the issue of paying for care. I've had had many conversations about that with, above all, experts in law, most of them feminist scholars, who are trying to reform laws so care can be better recognized — not only in divorce cases, but also in labor cases; and not only for women, but also for men. It applies to anyone involved in care work, anyone who for instance has to take a break from work for four days because one of their parents or kids is ill, and is fired afterwards. What is interesting is that *The Purchase of Intimacy* has become relevant, especially to female experts, but some male experts have also written about this, taking steps through legal instruments, not political ones, to influence real social changes, and not only in theory.

JO: In that sense, your work does contribute to a more public discussion...

VZ: That's what I was going to tell you. The most intelligent review I have read about the *Purchase of Intimacy* was published by a law professor from Maryland in *Law & Social Inquiry*. She shows how the theoretical framework can be applied to the issue of markets for genetic materials. After reading the review, I started to better understand some parts of my own book. Have you realized that sometimes that happens with our one's work?

JO: Well, those were my questions. Thank you very much Viviana.

VZ: Thank you.

References

White, H.C. (2008). *Identity and Control: How Social Formations Emerge*. Princeton: Princeton University Press.

Zelizer, V. (1994). *Pricing the Priceless Child: The Changing Social Value of Children*. Princeton: Princeton University Press. (Original work published 1985).

Zelizer, V. (1997). *The Social Meaning of Money*. Princeton: Princeton University Press. (Original work published 1994).

Zelizer, V. (2005). *The Purchase of Intimacy*. Princeton: Princeton University Press.

Zelizer, V. (2011). *Economic Lives. How Culture Shapes the Economy*. Princeton: Princeton University Press).

Zelizer, V. (2017) *Morals and Markets: The Development of Life Insurance in the United States*. NYC: Columbia University Press. (Original work published 1979).

Zelizer, V., & Gaydosh, L. (2019). Class on Campus: How Students Manage Everyday Inequalities. *Princeton Alumni Weekly*, January 19. https://paw.princeton.edu/article/essay-class-campus

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Lessons and Current Challenges for Urban Sociologists. A Conversation with Robert J. Sampson

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Abstract

In this interview, Robert J. Sampson discusses main lessons and current challenges for urban sociologists, starting from his personal experience and perspective. The interview recaps his important works on factors and events that can determine criminal behavior, the important *Project on Human Development in Chicago Neighborhoods*, that he led with his innovative theories and empirical results on violence, race and ethnic segregation, inequality, order and disorder in urban environments, and the shifting structure of community network. The interview also reflects on the process and encounters that led him to the formulation of a theory of race, crime and urban inequality with William J. Wilson (1995). Our discussion spanned over areas of research interests of Professor Sampson, including crime, disorder, life course, civic engagement, inequality, "ecometrics," and the social structure of the city, with particular attention to neighborhood effects. Finally, the interview deals with current challenges for urban sociologists, focusing on two main problems: poor quality of data and limit of funding.

Keywords: Sampson; neighborhood effects; urban sociology; social structure of the city.

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1 Introduction

Robert J. Sampson is the Henry Ford II Professor of the Social Sciences at Harvard University, founding director of the Boston Area Research Initiative, and Affiliated Research Professor at the American Bar Foundation. He was a student of Professor Peter Blau and Professor Travis Hirschi, who later became his dissertation advisor. In 1993, together with John H. Laub, Sampson published *Crime in the Making: Pathways and Turning Points Through Life* (Harvard University Press), where they offered empirical data, theory, and a historical perspective through an outstanding longitudinal study on factors and events that can determine criminal behavior. This study was particularly important, as at that time there was — especially in the US — a strong debate about the usefulness of longitudinal studies on criminal behavior. Age was also considered invariant because offenders commit fewer crimes as they age (Hirschi & Gottfredson, 1983) and the most predicting factor of crime was argued to be low self-control (Gottfredson & Hirschi, 1990). *Crime in the Making* received the Distinguished Book Award from the American Society of Criminology. In 1994, Sampson became the scientific director of the *Project on Human Development in Chicago Neighborhoods*, a collaborative project that has produced important empirical results and theories on violence, race and ethnic segregation, inequality, order and disorder in urban environments, and the shifting structure of community networks. At the University of Chicago, Sampson met and worked with William J. Wilson, which led to the formulation of a theory of race and crime and urban inequality. In 2003, Sampson accepted a faculty position at Harvard University, where he became chair of the Department of Sociology in 2005. In 2012 he published *Great American City: Chicago and the Enduring Neighborhood Effect* (University of Chicago Press), a capstone book that received the Distinguished Publication Award from the American Sociological Association in 2014.

Professor Sampson's research and teaching cover a variety of areas including crime, disorder, life course, neighborhood effects, civic engagement, inequality, "ecometrics," and the social structure of the city. with particular attention to neighborhood effects. I had the opportunity to interview Professor Sampson in May 2019 while he was at Sciences Po in Paris, at the Centre d'études européennes et de politique comparée. The interview was informal, and topics ranged from personal and professional pathways to methodological questions to new challenges for urban sociology.

2 On the Shoulders of Giants: Sampson's Masters

Niccolò Morelli (NM): I would like to start with a personal question. Why and how did you decide to study sociology and what prompted you to move to criminal justice for your Ph.D.?

Robert J. Sampson (RS): In my undergraduate studies, I was really interested in philosophy and psychology. Then I took a sociology course that opened my eyes to thinking about the world from a more social and structural perspective compared to psychology. I think it also had to do with personal experiences. Specifically, I grew up in a small industrial city along the Mohawk River in upstate New York. Along that area, there were a series of cities whose livelihood came from textile mills, which fell apart roughly after the 1960s and 1970s. When I was growing up from the mid-1950s on, I witnessed this incredible decline. I'll give you an example: the population of Utica that was over 100,000 in my childhood is now around 60,000. So, it lost 40 percent of its population within a couple of decades. I lived through much of

that and saw a lot of social change. It's a little bit retrospective, but I'm thinking back in my mind to these issues of population decline and social change, what's going on in cities today, and psychological sentiments. I raise these because I think in some ways I've always been interested in cities and different kinds of neighborhoods within cities. For example, as I look back, the small city where I grew up had all the classic manifestations of urban inequality, such as an Italian neighborhood, a middle-class white neighborhood, an upper-class professional neighborhood, a segregated black ghetto, and an immigrant area. In the police force, there was a fair amount of tension with certain communities. There were all these things that were precursors to what I later studied. At the time, Cornell had a strong program in the sociology and philosophy of science. Although I was interested in that in my undergraduate study, I grew up in upstate New York, and I came from a lower income household, so resources were an issue. As a resident of New York State, I could attend the State University of New York for basically free. I heard about the criminal justice program at Albany and I saw it as a very vibrant interdisciplinary opportunity, so I decided to go there. This was at a time where the study of crime was coming into its own as an intellectual enterprise, due primarily to the social changes that were unfolding in cities since the mid-1960s. Hirschi was one of my mentors, and an important intellectual influence there. Another influence I come back to is Peter Blau; he and Hirschi are key influences on me to this day in terms of structuralism and social structural perspectives.

NM: The influence of Hirschi and, especially, Blau clearly emerges in your writings. Specifically, I am thinking of all the papers and books that you, together with Laub, published on the importance of longitudinal studies in order to better understand criminal behaviors in response to the works of Gottfredson and Hirschi. How did these two authors, Hirschi and Blau together, influence your studies and your thinking?

RS: Hirschi provided an intellectual turning point for me. I took a seminar from him on social deviance, and I read his book, *Causes of Delinquency* (1969). It was very interesting — it was bold and made strong claims, and it was written in an unusual way for academics. I found it intellectually exciting. And I also read *Social Sources of Delinquency* (Kornhauser, 1978). It was published in 1978, but the legacy goes back to the 1960s, when Kornhauser was very influential in Hirschi's work. Her structural differentiation argument was very compelling and led me into this whole literature of neighborhoods, ecology, and urban differentiation. I started reading scholars like Hawley (1973) and everything about the Chicago School — for example, McKenzie, Park, Burgess, Shaw, and McKay — who were influential in Kornhauser's works and on social disorganization theory. At about the same time, in 1977, Blau published *Inequality and Heterogeneity*, a major book that he had been working on for a while; he was on the faculty in Albany in the Department of Sociology. Blau was not by any means a criminologist; he was a sociologist. But his argument in that book was incredibly bold because it was a general theory of structural differentiation in society and what he perceived as the differentiation of what he calls "graduated parameters," such as income, how they were distributed in society unequally and how they are correlated with the nominal categories of gender and race. This was early on, before Douglas Massey started writing about racial segregation. Blau was writing about how when the differentiation between racial groups is correlated with income it creates certain types of structural inequalities. The book was a formal theory, deductive, and with many hypotheses that he derived from various starting positions. He was mainly interested in explaining intergroup relations, the social integration of society, and how different groups came into contact. As I was reading Blau's book, I realized that he was talking about everything, from marriage to crime, that is influenced by structural differentiation, so I put

together what I learned at Albany and from the Chicago School and started simultaneously observing as a sociologist and a criminologist. I wear two hats — three hats actually — I would say. The urban sociological hat, the criminological hat, and a third that combines crime and life-course thinking. Those were the things that dominated my thinking.

NM: Another sociologist who has influenced you — from what I can see in your work —, is Wilson; you met in Chicago and later at Harvard, right? I'm really interested in what you wrote more than twenty years ago towards a theory of race and crime (see Sampson & Wilson, 1995) and also your update, which I understand is your latest work (see Sampson, Wilson, & Katz, 2018). It seems that this collaboration has been very interesting for you and for your work. What is your thinking about those more than twenty years of work together?

RS: Good question! You're correct that Wilson's work, particularly, *The Truly Disadvantaged* (1987), was another key influence. That came later, but it fit my thinking very well, in the sense of going back to the late 1970s from reading Blau and Hirschi and trying to put together the Chicago School and urban neighborhood theory. While I was working through those issues, race was always there, but I hadn't really quite theorized it until the mid-1980s and later. I wrote a paper on neighborhoods, victimization, and race (see Sampson, 1985), making the argument that race was confounded within multiple disadvantages and that a lot of the interpretations we had been given in the past were misleading. Then in 1987, *The Truly Disadvantaged* comes out and that was to me like *Causes of Delinquency*, one of those books that I started reading and couldn't put down. What I liked about it was that it was a macrosocial perspective, but it was also trying to understand how the macro is mediated, in this case, by changes in neighborhood structures. Wilson was trying to understand massive change, deindustrialization and the outmigration of the black middle-class, in turn changing the nature of class stratification and leading to an increase in the concentration of poverty, particularly in black neighborhoods, which then has specific social influences. You see there the early seeds of the theory of race and inequality, and the idea that the causes of variability with respect to crime are similar in the sense that the proximate causes are the same to all racial groups and not unique to black culture. I took from Wilson the idea that joblessness in the black community, particularly among males, was a key structural cause of family instability, which then linked to my work on supervision and single parent families, particularly informal and peer-control culture (see Laub & Sampson, 1988). One of the pathways I considered, whereby employment and structural characteristics of cities were related to crime, I analyzed both white and black crime rates and then I tested the difference between the race-specific equations and found that there was a real similarity in the basic patterns but tremendous differences in the kinds of exposures. Compared to where you have a characteristic that has different effects but similar levels in each community, here you have similar effects but with very different exposure levels. It's kind of a typology about what's going on. Wilson was published in 1987 and then Massey and Denton in 1993 published *American Apartheid: Segregation and the Making of the Underclass*, which is reacting to Wilson saying that it's not just about deindustrialization and how the migration of blacks progressed but also that you must consider racial segregation as an independent structural force. On the argument between Massey and Wilson, which was the subject of a public debate at the University of Chicago, it turns out they are both right, they just emphasize different features. Lincoln Quillian later had a brilliant article in the *American Sociological Review* (2012) testing out what he called, three kinds of segregation. Basically, he showed that Massey and Wilson are both right in the terms that you did have the outmigration of the black middle-class because of civil rights laws in the U.S. (Wilson, 2003), that opened

new opportunities. The black middle-class, like anyone else, wanted better housing and many moved to the suburbs, but that meant that you had the poor left behind. But blacks are still moving into a segregated environment and that segregation has structural causes. Anyway, Wilson read my paper in the *AJS* in 1987 and wrote to me about it. He liked it and wanted to meet at the American Sociological Association conference. This is how my relationship with Wilson started. Slightly later, the University of Chicago was doing senior recruitment, hiring multiple people. I got a call from James Coleman, another person I admire.

NM: This is really interesting because, of course, we know James Coleman for his theory on social capital, and even if you rarely speak about it, in the collective efficacy theory, speaking about interpersonal trust, neighborhood relationships, you can *feel* social capital even if it is not cited. So how did you speak with Coleman, and how close are you to his theory of social capital?

RS: You're right. In 1988, he published in the *American Journal of Sociology* his article on social capital. I'm not a big social capital fan, in terms of the language, but I found Coleman's work very different from Putnam, who later popularized social capital. In the article in the *AJS*, what he was saying was that neighborhood social organization in the form of social capital and intergenerational closure were important and could affect the future. I mention this influence because the collective efficacy theory was a product of thinking this all through. I viewed his article as integrating the Chicago School idea of social control with the idea of activation of social ties. The thing I got out of Coleman was social action and the way that social ties are activated for intended outcomes or behaviors, and that is how he would talk about it, sort of agentic. If I'm in a neighborhood and I can trust the group to take care of my kid or to leave my car or window open, I essentially have a grounded trust in the neighborhood. That's a real property that benefits not just me, but the collective. That's what I call collective efficacy, which was the intellectual result of Coleman's intervention in my way of thinking, but it was also definitely connected with my long-standing belief in the importance of neighborhoods, social control, and neighborhood social variations.

3 From Park and Burgess to Chicago School in the 21st Century

NM: So, you were saying that you met Coleman, and this was the beginning of your Chicago experience. But at that time, there were also several scholars who were conducting impressive research on urban dynamics, especially in segregation studies. And you also got more in touch with Wilson and in that period you also started working on race, crime, and urban inequality. How vibrant was Chicago at that time, and how did you start your reflections with Wilson?

RS: Yes, I received a call from Coleman when Chicago was doing a senior recruitment, hiring multiple people at that time. On the faculty, were, in addition to Coleman, Wilson, and Massey, a very high-powered group. I was offered a position and went there in the fall of 1991. A key factor for me was that both Wilson and Massey were there; it was no-brainer, intellectually I fit right in. I was talking to Doug and Bill, not daily but they were colleagues, and so I began to think more about racial segregation. John Hagan, the sociologist, was doing a volume, *Crime and Inequality* (1995), and he asked if I would write a paper. I thought it would be an occasion to think more systematically and theoretically about these different literatures. I was talking to Bill, and I said, "What do you think about writing this together?" It became

the chapter entitled "Toward a Theory of Race, Crime and Urban Inequality." (Sampson & Wilson, 1995).

NM: You mentioned the importance of a relevant group of researchers in Chicago when you joined the University. The Chicago School is probably the most studied sociological school on both sides of the Atlantic. What did you learn from the Chicago School? What are the relevant questions that Chicago posed? And what did they not consider?

RS: The importance of context, understanding social change, the important role of neighborhoods, the idea of stability and change for the reproduction of urban equality. These are some of the concepts I derived from the Chicago School. I think that there are many things we can learn from the Chicago School, but we shouldn't be beholden to them; it's not like it's some magical holy book, it's just that it does present a systematic guide, particularly at the time, of how to study cities. The University of Chicago Press just released a new edition of *The City* by Park & Burgess (1925/2019), and I wrote the foreword in which I address these questions: What was the Chicago School about, and what did they get wrong? I think what was important about the Chicago School was more about the kinds of questions it asked and the fundamental principles that were behind it (Sampson, 2019a). The study of cities needs to take seriously, again, urban social processes and the effects of the city on the mind and cognitive processes. A lot of what was in the original Chicago School was in Wirth's (1938), and even Simmel's (1903/2012), works about how urbanization and the city affect mental processes. Fischer, in *Toward a Subcultural Theory of Urbanism* (1975), later wrote that Wirth was wrong — urbanization isn't producing alienation that is psychological withdrawal and where city-dwellers have fewer ties; Fischer proved that, yet urbanism does have a negative effect on trust in the public, what Fischer called the "public sphere" (1981). That kind of linkage of the structural and mental is interesting and yet got somewhat lost. So, I tried to get into that connection more in *Great American City* (2012), around the idea of stigma and social perceptions of disorder. In particular, I have worked with Stephen Raudenbush on the factors that matter most in perceived disorder, and we discovered that while observed disorder predicts perceived disorder, neighborhood racial and economic contexts are more important in driving higher perceived disorder. (See Sampson & Raudenbush, 2004)

NM: I would like to know more about *Great American City* because I think that it might represent a *summa* of your Chicago experience. What does it represent to you? It was a way to show the results of the Project on Human Development in Chicago Neighborhoods that you led, a way to open the debate to the new questions raised by those results. Is that right?

RS: It might be useful to know some of the historical context. I started to write the book about five years before I finished it. I originally felt there was a need to pull together the pieces of the Project on Human Development in Chicago Neighborhoods in a book form. It's a common strategy in large research projects but as I began to write, I was dissatisfied because it wasn't yet saying what I wanted to. I felt there was something beyond the specific papers from the project, so I stopped, stepped back, and started to think more broadly about the idea of neighborhood effects and all the things we've just been talking about — the Chicago School, the role of context, the city — and for better or for worse, it became a book about basically everything I had done and thought about. It unified, or, in other words, it gathered together and expanded a common intellectual idea. Then, once I realized that, it became much bigger as a project, it expanded. I felt I wanted to weave more things together, so there were neighborhood components, there's the structural piece about segregation and the reproduction of inequality,

and there was the part where I started to talk about how neighborhoods are linked in a higher-order social structure, which is an analytical sociology way of thinking about units of linkage crossing levels. For example, how does individual residential mobility between neighborhoods work? How does mobility create the neighborhood ties which in turn create the larger social structure of the city? That was a new way of thinking for me, and I had to pull that all together. I also inserted myself as an observer of Chicago because I was there, and I felt that while I wasn't an ethnographer a key part of my thinking, even in quantitative work, has been about observation.

NM: I would also like to reflect a little bit on the methodological aspects of your book and your work in general. I think it is interesting and important to underline that you made your analysis on Chicago neighborhoods using both qualitative and quantitative tools. For example, you used census data and survey data, but also you adopted videotaping for describing neighborhood interactions. It seems that you follow a mixed-methods approach. But this idea of mixing together quantitative and qualitative tools, is it a theoretical idea, a methodological one, or just a choice of tools? Is it right to say that you used a mixed-methods approach in your book?

RS: Yes. I think that's right. Mixed methods have gotten a lot of attention these days, but it's not clear sometimes what it means. For me, it's more of a theoretical idea that you come at a problem in multiple ways. The cookie-cutter approach is typically "well you do a quantitative survey or something, you talk to some people to get their perceptions." But for me, *Great American City* wasn't an ethnography and it wasn't purely quantitative; it was also going back and forth between knowledge of places like Bronzeville, Hyde Park, Cabrini Green, and the Robert Taylor Homes. These all have historical meaning in Chicago that relate to my concepts. One of the main components of the study was called "systematic social observation," where we videotaped streets. That's where a lot of the disorder stuff comes from. We were asking people about their environment and how they were subjectively perceiving it, hence their cognition. But then we're independently looking at and rating neighborhood contexts in a more systematic way. I was always interested in observation, and it also became clear to me that I needed to put myself more into the study as an observer, which then led to a very detailed analysis of the places. The beginning of the book is based on a walk and immersing myself into the community and narrating, as it were, place and the idea of spatial differentiation. You can see it, smell it, feel it — I wanted the reader to know what it's like to walk through very different neighborhoods of Chicago. Then, I came back to it at the end after many years of study as a sort of postscript or aftermath, revisiting some of the neighborhoods that were going through changes. Cabrini Green, which in the early part of the Twentieth century was known as "death corner," was an Italian slum and then it became a black concentrated poverty area and yet they were both high crime areas over the course of a nearly a century.

NM: Another thing that I find interesting is that you have rarely analyzed European cities. Why? Do you think that there is too much difference, or the reason relies on what you said before about Chicago, that you want to give meaning to places, and you are not confident with European contexts?

RS: Comparative research is hard, especially in multiple countries. I think that is the frontier now. The way I viewed it was that it's hard enough for me to do just one city! So, my hope would be that there would be a lot of studies and new investigations based on neighborhood data on other cities globally, such as mega cities like Mumbai or Shanghai. I think it's just a

matter of time before that will happen and my bet is there will be a continuing debate about contextualization, globalism, and theories of American cities. I just think the current state is in part because a lot of the early work was done in cities like Chicago, but the kind of factors that are important in terms of inequality — segregation, social disorder stigma, collective efficacy — are present in many other cities too. That's my hypothesis. (See Sampson, 2019b)

NM: The importance of research teams emerges clearly in your work on Chicago and also in many other studies. However, big research groups are more and more uncommon. In your Chicago experience, and later on, you led big research groups. What did this opportunity give to you in terms of approaches, understanding, and research hypotheses?

RS: It's true that in my career I've tended to concentrate on relatively few projects over a long period of time and work, particularly in the Chicago project, with teams. I guess what I would say is that I really enjoy mentorship, particularly graduate students. There are three kinds of research teams I've worked with. The Chicago project involved a large research team that went beyond just graduate students. There were a lot of people who collected data and investigated multiple records. I was part of this larger collaborative group; we each took charge of different things. There was another research style team that I've done my entire career, which is with small groups of graduate students. More recently, I've been involved in research teams that involved postdocs who work in a model like a science lab. For example, the research that we discussed on urban mobility involved two postdocs. That's a different kind of teamwork, where the postdocs have their own research project but then they are also involved in a collective ongoing research project. They also tend to interact with graduate students. I also have a workshop called the "urban theory and data lab," which involves works-in-progress and discussions of ideas; it's basically a small group, two or three postdocs and four or five graduate students. So, a relatively cohesive group that is heavily focused on research and progress. I think that research teams are an effective tool and a rewarding one because a lot of these projects involve data collection and analysis at a large scale. That's not something that any one person can do — it really does require a collective enterprise.

4 Current Challenges for Social Research: Poor Quality of Data and Limited Funding

NM: In your recent article on urban mobility and neighborhood isolation (Wang et al., 2018) you used Twitter data. This drives my attention to big data. In the current sociological debate, everyone is speaking about big data, but few are working on it. What do you think about this new tool? Do you think it is reliable data for social research? How do you manage to mix traditional data sources with new datasets? How can it help urban sociology?

RS: There's a lot of talk about it as you say. A lot of strong claims are made on how big data is changing social science. Some say surveys are dead because it's difficult to get people to agree to surveys anymore. My view is that big data are simultaneously an opportunity and a threat. They must be thought through very carefully. The threat part is that these data are, for the most part, generated not for research purposes. It's not original research; they're not collected with research in mind. They are like any administrative data set. They are for the companies, and what that means is that the quality, the meaning, the reliability, and the validity of data is something that needs to be carefully analyzed and interpreted. Yet people often just analyze the

data. In fact, if you work with companies or their data scientists and engineers, the big discussions are around technique, especially machine learning and algorithms and how to manipulate big data. Very little concern emerges about the actual meaning of any individual piece of data and what it means, and I think that's a huge problem because big data doesn't necessarily mean good data. That's why I approach big data the way I approach little data, which is that research design and theory are essential, and we need to apply systematic standards to them. So, with colleagues, we started reflecting on analytical approaches to big data. I derived this approach on big data from the article on little data published in 1999 in *Sociological Methodology* on systematic social observation, called "ecometrics" (Raudenbush & Sampson, 1999). The analogy was psychometrics, and we used the statistical models for reliability and invalidity in measurement creation. In a 2015 article, also in *Sociological Methodology*, we applied that to big data and made the argument that these data need to be analyzed very carefully and systematically (O'Brien, Sampson, & Winship, 2015). On the opportunity side, they do present ways to view or measure certain social phenomenon in ways that we can't capture with traditional forms of data. My use of big data has been completely substantively driven. For example, I use Twitter data, which can have all kinds of limitations, but they do provide a large-scale signal about urban mobility patterns. The idea is that where you live is not necessarily where you spend time. It's not even just about work and home neighborhoods; as you go to your work neighborhood and you go back and forth to other areas it is not necessarily in a confined geographic space. If you think about individuals in their everyday lives over a span of one or two years, there's a structure to their visitation patterns in the social sense.

NM: Yes. This drives me to another question that I would tag as "borders." So, you spoke about home and work neighborhoods, mobility patterns, I would say also perceived neighborhoods, perceived borders that often do not coincide. So how do you define a neighborhood, a city, and their borders so that they have sociological relevance for urban analysis?

RS: It's an important question, and my analytic position in thinking about these various units of analysis is that there really is no one correct unit. It depends on the theoretical question. It also depends on the phenomenon. Sometimes, a very small neighborhood, maybe even a block, sometimes a housing project, can be a neighborhood. However, even then you may have more potential to interact on your own block or street but there are people at the end of your street and the next street over who interact, and it sort of combines into larger structures. So, I think that small areas, neighborhoods, community areas, and large areas are all important, depending on the phenomena. Cities and urban functional areas are too. In Chapter 10 in *Great American City* I look at spatial models because one of the ways to deal with the modifiable areal unit problem is to think about how a neighborhood is not an island. It shares borders with other communities, and we must model that — taking border areas into account and how adjacent neighborhoods are influencing others. That's still assuming a certain boundary and people's perceptions of those boundaries can differ, but that doesn't mean that there aren't structural effects of those communities. People have shared perceptions that are influencing behavior, and there are also structural influences on behavior — they're both happening. I looked at the findings of "The Importance of Trivial Streets" by Rick Grannis in the *American Journal of Sociology* (1998) where he interviewed people about their perceptions of boundaries, which differed. But actual interaction patterns were very much shaped by the ecology and what he called tertiary communities, which were basically areas where there were no major residential streets or parks (or other physical boundaries) dividing them up. Interactions tend to happen within these spaces; people's behavior was constrained and shaped by ecological patterns.

NM: Speaking about your studies on cities, I found your research focus on health issues really interesting and important also for the future of cities. This pattern will be an essential variable especially related to the evolution of mega cities. How did you relate to these issues? Do you consider health issues a main challenge for cities?

RS: I guess I always had an interest in that, partly because if you look at some of the early Chicago research there has always been the notion that some of the epidemiological and health-related findings were related with crime and other features, which is a hint that the broader social organization of the city is explaining a lot. I think the study of public health and the integration with criminology is important. To me it also suggests a more structural approach that implies common causation. I think we see this clustering in cities around the world, what we often call concentration effects. If we think of the argument in *Great American City*, the concentration of inequality is true in multiple contexts. Cities are always changing, but they are overlaid on a pretty stable structure in most cases. I mean, gentrification is all about change, but still, if you look at a long period of time, the durability of inequality is remarkable.

NM: Your attention to health, big data, demography, and crime all show your interest on a multidisciplinary field. If we look to young researchers, it is quite rare because, even if multi-disciplinary research groups are rising, every component is an expert in a very specific domain. Looking at young Ph.D. students and postdocs, do you think it is possible to conduct research with a multidisciplinary approach as you did, or is it now more difficult?

RS: Things have changed. There's more emphasis on quantity and evaluation. How that affects the nature of work, I'm not sure. I guess in an ideal world, I'd like to believe that in the long run we should be caring about the questions, the quality, the data, and so forth. What I worry about is that it's harder to collect your own data; it's harder to collect original data because of cutbacks in funding. I think that's a pressing concern. In the U.S., we have active defunding of social science, a hostility from the current administration, and cuts to federal agencies. I think for younger scholars it's especially hard to get funding, a structural limitation which may affect the quality of the science. This is also related to what I was saying before about the availability of administrative data and big data. I think that what we are likely to see is more reliance on the kinds of administrative sources that are outside of one's control, a bad thing that requires a certain due diligence. It also may mean that there's a narrowing of the kinds of data and the narrowing of the kinds of questions we can ask with these changes in the nature of social science. So, I do worry about that, for sure.

NM: We spoke about many things, especially challenges for researchers with new items, new tools. You are a reference point for many urban sociologists, not just researchers interested in urban studies. This also means responsibilities towards young researchers. What is the role that you can play related to new challenges for young researchers?

RS: A lot of what I view myself doing is to try to figure out questions, especially with graduate students. I think we undervalue asking good questions and sometimes that's the hardest thing for graduate students because they tend to learn a kind of cookie-cutter approach and some sort of applied method that is not really chasing good questions. Science is advancing through research discovery as much as it is through questioning our current understanding and posing new questions and generating hypotheses. So, in a sense I consider myself theoretically motivated and that would probably be controversial to some who would consider themselves social theorists. There's this idea of social theorists that produce pure theory, which for me is a foreign concept. I'm always constantly interrogating ideas with how the world works. There

are two kinds of theories: there's theory about theorists or theory itself and there's theory about the empirical world. Sometimes I engage purely theoretical ideas or intellectual history, but I think my primary motivation is theories about the world. In that sense, it's theory about how things work. I don't consider myself, in any way, a statistician, obviously, or a methodologist. Even though I use whatever methods are necessary, I view myself as trying to generate new theory, ideas, and hypotheses.

References

Blau, P.M. (1977). *Inequality and Heterogeneity: A Primitive Theory of Social Structure*. New York: Free Press.

Coleman, J.S. (1988). Social Capital in the Creation of Human Capital. *American Journal of Sociology*, *94*, S95–S120. https://doi.org/10.1086/228943

Fischer, C. (1975). Toward a Subcultural Theory of Urbanism. *American Journal of Sociology*, *80*(6), 1319–1341. https://doi.org/10.1086/225993

Fischer, C. (1981). The Public and Private Worlds of City Life. *American Sociological Review*, *46*(3), 306–316. https://doi.org/10.2307/2095062

Gottfredson, M.R., & Hirschi, T. (1990). *A General Theory of Crime*. Stanford: Stanford University Press.

Grannis, R. (1998). The Importance of Trivial Streets: Residential Streets and Residential Segregation. *American Journal of Sociology*, *103*(6), 1530–1564. https://doi.org/10.1086/231400

Hagan, J., & Peterson, R.D. (Eds.) (1995) *Crime and Inequality*. Stanford: Stanford University Press.

Hawley, A.H. (1973). Ecology and Population. *Science*, *179*(4079), 1196-1201. https://doi.org/10.1126/science.179.4079.1196

Hirschi, T. (1969). *Causes of Delinquency*. Berkeley: University of California Press.

Hirschi, T., & Gottfredson, M.R. (1983). Age and the Explanation of Crime. *American Journal of Sociology*, *89*(3), 552-584. https://doi.org/10.1086/227905

Kornhauser, R.R (1978). *Social Sources of Delinquency. An Appraisal of Analytic Models*. Chicago: University of Chicago Press.

Laub, J.H., & Sampson, R.J. (1988). Unraveling Families and Delinquency: A Reanalysis of the Gluecks' Data. *Criminology*, *26*(3), 355-380. https://doi.org/10.1111/j.1745-9125.1988.tb00846.x

Massey, D.S, & Denton, N.A. (1993). *American Apartheid: Segregation and the Making of the Underclass*. Cambridge: Harvard University Press.

O'Brien, D.T., Sampson, R.J., & Winship, C. (2015). Ecometrics in the Age of Big Data: Measuring and Assessing "Broken Windows" Using Large-Scale Administrative Records. *Sociological Methodology*, *45*(1), 101–147. https://doi.org/10.1177/0081175015576601

Park, R.E., & Burgess, E.W. (2019). *The City* (Enlarged ed.). Chicago: University of Chicago Press. (Original work published 1925).

Quillian, L. (2012). Segregation and Poverty Concentration: The Role of Three Segregations. *American Sociological Review*, *77*(3), 354–379. https://doi.org/10.1177/0003122412447793

Raudenbush, S.W., & Sampson, R.J. (1999). Ecometrics: Toward a Science of Assessing Ecological Settings, with Application to the Systematic Social Observation of Neighbourhoods. *Sociological Methodology*, *29*(1), 1–41. https://doi.org/10.1111/0081-1750.00059

Sampson, R.J. (1985). Neighborhood and Crime: The Structural Determinants of Personal Victimization. *Journal of Research in Crime and Delinquency*, *22*(1), 7–40. https://doi.org/10.1177/0022427885022001002

Sampson, R.J. (1987). Urban Black Violence: The Effect of Male Joblessness and Family Disruption. *American Journal of Sociology*, *93*(2), 348–382. https://doi.org/10.1086/228748

Sampson, R.J. (2012). *Great American City: Chicago and the Enduring Neighbourhood Effect*. Chicago: University of Chicago Press. https://doi.org/10.7208/chicago/9780226733883.001.0001

Sampson, R.J (2019a). The City for the Twenty-First Century. In R.E. Park & E.W. Burgess, *The City* (Enlarged ed.) (pp. vii–xiv). Chicago: University of Chicago Press.

Sampson, R.J. (2019b). Neighbourhood Effects and Beyond: Explaining the Paradoxes of Inequality in the Changing American Metropolis. *Urban Studies*, *56*(1), 3–32. https://doi.org/10.1177/0042098018795363

Sampson, R.J., & Laub, J.H. (1993). *Crime in the Making: Pathways and Turning Points through Life*. Cambridge: Harvard University Press. https://doi.org/10.1177/001112879303900301O

Sampson, R.J., & Raudenbush, S.W. (2004). Seeing Disorder: Neighborhood Stigma and the Social Construction of "Broken Windows." *Social Psychology Quarterly*, *67*(4), 319–342. https://doi.org/10.1177/019027250406700401

Sampson, R.J, & Wilson, W.J. (1995). Toward a Theory of Race, Crime, and Urban Inequality. In J. Hagan & R.D. Peterson (Eds.) *Crime and Inequality* (pp. 37–56). Stanford: Stanford University Press.

Sampson, R.J., Wilson, W.J., & Katz, H. (2018). Reassessing "Toward A Theory of Race, Crime, and Urban Inequality": Enduring and New Challenges in 21st Century America. *Du Bois Review: Social Science Research on Race*, *15*(1), 13–34. https://doi.org/10.1017/S1742058X18000140

Simmel, G. (2012). The Metropolis and Mental Life (K. Wolff, Trans.). In J. Lin & C. Mele (Eds.), *The Urban Sociology Reader* (pp. 37–45). London: Routledge. (Original work published 1903).

Wang, Q., Phillips, N.E., Small, M.L., & Sampson, R.J. (2018). Urban Mobility and Neighbourhood Isolation in America's 50 Largest Cities. *Proceedings of the National Academy*

of Sciences of the United States of America, *115*(30), 7735–7740. https://doi.org/10.1073/pnas.1802537115

Wilson, W.J. (1987). *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*. Chicago: University of Chicago Press.

Wilson, W.J. (2003). Race, Class and Urban Poverty: A Rejoinder. *Ethnic & Racial Studies*, *26*(6), 1096–1114. https://doi.org/10.1080/0141987032000132522

Wirth, L. (1938). Urbanism as a Way of Life. *American Journal of Sociology*, *44*(1), 1–24. https://doi.org/10.1086/217913

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Value and Values in the Interstices of Journalism and Journalism Studies: An Interview with Candis Callison and Mary Lynn Young

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Abstract

In this interview, Professor Candis Callison and Professor Mary Lynn Young, along with *MEDIA INDIGENA* podcast creator Rick Harp, provide a deep and sometimes personal set of insights as to why the field of journalism studies came to function the way it did and why that field so often falls short in its analysis of issues related to race, indigeneity, gender, and colonialism. Both Callison and Young highlight the arguments they make in their recent book, *Reckoning: Journalism's Limits and Possibilities*, about the role and practice of journalism as it relates to methods, ideals, aspirations, social order, and ethics. They conclude with a discussione of the theoretical and epistemological frameworks that undergird their analyses in the book, and address the tensions between value and values in the news.

Keywords: Indigenous media; journalism; organizational structure; values.

MEDIA INDIGENA is an independent, Indigenous current affairs roundtable podcast, hosted and produced by Rick Harp. This discussion with Candis Callison and Mary Lynn Young took place over episodes 214 and 215.[1]

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1. The following machine-based transcript of the original audio has been edited for brevity and clarity. Please verify against the source audio before quoting.

Rick Harp: These days we hear only a little about the possibilities of journalism but a lot about the limits of journalism. Today, among those who are hopeful, we are joined by the co-authors of *Reckoning: Journalism's Limits and Possibilities*, from Oxford University Press. A book about the media moments we're living through, a time where both crises and opportunity coexist.

We're honored that Candis Callison, and Mary Lynn Young, have agreed to sit down with us to discuss their book. Candis is an associate professor in the Institute for Critical Indigenous Studies and the School of Journalism, Writing and Media at the University of British Columbia. And Mary Lynn Young is a fellow professor of journalism at UBC.

To start out, could I get you two to each reflect on what got you into journalism in the first place? Was it a calling at first? Were you, like me, kind of seduced by its power and how shiny it was? Candis, let's start with you.

Candis Callison: Yeah, I kind of fell into journalism, almost by accident. I had to put out a ton of CVs after I got my undergraduate degree. And then I got a job with one of the very few Indigenous television producers at that time. I mean, this was the mid-90s. There weren't very many Indigenous journalists at the time.

For my undergraduate, I went for a degree in business though I was most interested in history, and I got a minor in psychology and a sort of almost minor in communications. I had this emerging interest in communications that happened over my four-year undergraduate degree. But when I got out, it was really hard to find a job. I was a sort of typical first-gen undergraduate graduate.

I was offered a few jobs. One was working for a bank and one was working for a ski school, and one was working for this documentary series that had, as I said, one of the very few Indigenous television producers who had produced at a high level for the CBC. And I started working on that series. I fell in love with storytelling with audio and video. I took a night school course in film. Then I got a job at the CBC and worked on all kinds of different news and current affairs shows and eventually moved to CTV. I also made a film independently with Canada Council grants on my own First Nation.

What I realized was my kind of core interest was: whose voice matters. The very things that I'm still writing and thinking about, and whether or not there were very many people in the newsroom who looked like me — those kinds of representational issues, and whether our stories were getting told and who they were getting told by and how they were getting told. Those were all the things that were the engine for why I kept going in journalism.

I ended up getting a Ph.D. also by accident because the Internet happened in the late 1990s. I moved to the United States and I worked for one of the very first stations to launch online and offline. And then I worked for a search engine which was one of the top search engines at the time, rivalling Yahoo! It was called Lycos. I always tell my undergrad students because they've never heard of it: at the time that I started at Lycos, people were leaving to work for this *new* startup called Google. It was at the very beginning of the dot com transformation that has really transformed our daily lives as well as what it means to do journalism. And from there, I started down the path towards becoming an academic. I went to M.I.T. and did a Masters in Media Studies first and then a Ph.D. (in History, Anthropology, and Science, Technology, and Society) and really never meant to come back to journalism. But again, my core interests were around whose stories matter, whose voices matter; how do we talk about issues like climate change that are of great concern to us all? And where are we speaking from? Those are the things that still animate what I do in research and what I do in teaching.

RH: Mary Lynn, outline your path into journalism.

Mary Lynn Young: Well, first, that's a great question. And I didn't know that about Candis and ski school. Always good to learn something about your co-author.

Basically, I can answer this in two ways. There's what I thought as a younger self. I see it quite differently now why I chose to go into journalism with the benefit of hindsight and a few decades under my belt. What I thought at the time was that, similar to Candis, I was looking for a way to have a job and to do something that I felt was meaningful and committed to in university. And, my mother and stepfather hadn't gone to university. And I didn't know many paths that were open to people with my kind of background growing up in Stoney Creek, Ontario.

When I was at the University of Toronto, they had two newspapers that you could volunteer for as a student. I was completing a history degree with a focus on the medieval and early modern period (I loved it, but not something that you can find a clear path to a career necessarily.) I volunteered at the underdog, more alternative newspaper on campus, called "the newspaper" (all spelled in lower case). And I enjoyed it. I started doing campus news and I started to find it interesting. And I got a job working for the Ministry of Culture and Communications, doing clippings. I'd show up at 7:00 a.m. and go through all the media newspaper clippings and cut out what was relevant to the minister so that it would be on her desk when she got in the morning.

And I thought, OK, this is a fascinating career. It's committed to all these principles of truth and holding power to account. And I bought into all of journalism's ideals when I was a young person and started. I ended up staying on at the ministry, doing some early communications work. I had applied to go to Ryerson. I didn't get accepted the first time that I applied. And, I thought, I've got to try and figure out what I do now to reapply, what I do in between not getting accepted and then trying to apply again the next year. I applied to work at a small weekly newspaper and worked there for a few months, and then I applied to work at the daily newspaper in my hometown, which was Hamilton. The city editor finally gave me a chance to cover a small little area called Flamborough. And I started to cover the council meetings. That's how I got my foot in the door.

My first job after that was as the night weekend crime reporter. I'd work 6 to 2 in the morning at the *Hamilton Spectator* on Friday and Saturday nights. And yeah, I'd cover death, destruction and anything difficult that happened in Hamilton on the weekend. I ended up getting accepted into Ryerson, and I was off to the races. That's what my younger self was probably thinking.

Fundamentally though, I was compelled and I believed the idealized mission that journalism told about itself. When I worked as a journalist, I thought I was finding the truth through fact gathering through my interviews, sources. It was only until I ended up reporting on crime in Houston, Texas, for the *Houston Post* that I started to have a number of moral dilemmas and crises about: was I representing things accurately? Could I, given my level of expertise, my educational background, given the complexity of the structural and sociological landscape that I was covering?

RH: So you were disabused of this crusade that we're supposedly on for The Truth, right? And I'm wondering — was it gradual or was it more an acute thing?

MLY: I had an epiphany about a question about am I causing harm here? It started to come into my consciousness: am I causing harm here?

Because I was covering Houston, the fifth-largest U.S. city; it's a major crime city. There were more than 500 homicides a year. And I was responsible. I worked out of the downtown police station. The city was ringed by petrochemical plants, two major international airports.

What would happen in a day in Houston wouldn't happen in a year sometimes in parts of Canada in terms of just the crime beat. Yes, I started to think I was causing harm, but really, it took decades to re-socialize myself. I have to be honest from both having been a journalist and how I thought I knew what I knew, what I thought I was doing. There was the epiphany about harm. And then I went to graduate school. And it took years there to understand the role of structure, power, gender dynamics. I went in for a master's and Ph.D. in Criminology, and then it took even more time in the university to see not all professional identities or professional groups interact this competitively. Not all professional identities engage in these kinds of relational ways.

RH: You just made me think, how many crime reporters have a criminology degree?

MLY: Not many. No, no. That's why I went back to school. I thought I would do a better job. That was my initial impulse after I started thinking about harm. I thought, okay, I'll go back, I'll get a master's degree... I thought, I'll go back to school and I'll come back as a better-informed crime reporter. I'll be able to do my job better. But then I realized that it was more complicated than that.

RH: But you Candis, how did you come to go, "Jeez, maybe what I'm doing here isn't what I want to be doing."

CC: I relate to both of you. I bought into the voice for the voiceless aspects of journalism, like: 'oh, we just need more of our stories' and not recognizing that there were real solid structural issues. And I don't think I realized that until, like Mary Lynn, I had gone through grad school and come back. I was working on this book that I wrote about climate change, which looked at not only journalists, but scientists, Inuit Circumpolar Council leaders, corporate social responsibility activists, and American evangelicals who were super involved in environmental issues. So, really different groups of people alongside journalists, and it made me start asking a lot of questions. At the same time, I was teaching a media ethics course and really questioning a lot of the foundations of how we thought about ourselves as journalists.

Part of my transformation also comes from the fact that when I started in journalism in Canada, there was no APTN [Aboriginal Peoples Television Network]. I always felt like I was the only Indigenous person in the room and I *was* — in most of my journalism experience. I was hired to work on shows to get the "Indigenous content," to find Indigenous people, because of course, nobody seemed to know an Indigenous person or an Indigenous community organization that they could call. So, that ended up being a very odd part of my career.

When I moved from CBC to CTV, we had the first-ever, national Indigenous current affairs show that was all about the kinds of issues that I was grappling with in my personal life as well. My father and his siblings went to a residential school. The episode that we did on residential schools, I think it was one of the very early, national shows that looked at residential schools. This was back before the Truth and Reconciliation Commission.

People talked about that show in general as being like the first time you really saw this kind of urbane Indigenous community — our first show was talking about the "urban reserve." It was a concept people hadn't even thought about at that time: all of the many Indigenous people who come to a place like Vancouver or Toronto or Winnipeg from different communities and different nations, different languages and cultures. I feel like that was a super-rich time and I didn't have time to process it until I got to grad school.

Of course, I really thought the Internet, too, was going to change our lives in very positive ways. Suddenly, those voiceless would have a platform of their own and that actually has been kind of true. We do a chapter in the book that talks about pushback, especially from Indigenous people when it comes to how media are covering them. So, I think some of that stuff has

happened. But, yeah, it's funny to think back to the younger self that was completely bought into thinking that journalists had a very tough job and they do, but not thinking about the broader structural issues that we confront in the book.

RH: Mary Lynn, my read of what you've mapped out in this book is that there are concurrent crises that flow out of this current media rupture and, at the risk of essentializing things, I submit that two broad crises are facing what's known as legacy media. One is a crisis of profitability. The other is a crisis of legitimacy. To me, it's very tempting to connect the two, and we can do that if you feel they very much are. But for now, I'm just curious whether you feel that the first crisis of profitability has sucked up much of the oxygen and effectively starved the other crisis of any attention or any sustained investigation.

MLY: I agree with you and we talk about the myopia that some of the journalism studies literature has focused largely on the business of journalism crisis and the technology crisis, the advent of digitalization or digital media, and its impact on the former legacy players.

CC: The crisis is real, right? The technological, economic crisis. We're seeing daily tweets right now about journalists who are being laid off and losing their jobs. That stuff is really real.

At the same time, what this book is, is a not-hot take on what's happening in journalism. Instead, it builds on this long-term critique of the story that journalism has told itself about what it's doing that often ignores ongoing harms, the way that journalism is very much a part of social ordering, the whiteness of newsrooms. The fact that there are very few people of colour, there are very few Black and Indigenous people in newsrooms in North America. I think that on one hand, we don't want to downplay the crisis. But on the other hand, we also have to look closely at what journalism has been doing and how it might do it better in this time of reorganization that is inevitable as we're watching this downturn economically. The way that journalism has done business as usual isn't working the way that it used to. How do we begin to rethink all aspects of journalism, not just the technology and economic piece of it?

RH: Well, there was this great short quote, and you've alluded to this a bit already in your comments, but I'm thinking here of the late sociologist Herbert J. Gans and his study of mainstream U.S. newscasts back in 1979 led him to conclude, "news reflects the white male social order."

CC: We open our first chapter talking about the history of journalism scholarship by talking specifically about the apology that *National Geographic* issued in the last couple of years. They had a historian look through all of the *National Geographic* coverage. We did a show on this for *MEDIA INDIGENA* and we quote from that show because it's such a powerful moment in terms of media recognizing its participation in ongoing harm, in colonialism and at the same time doing very little to change how newsrooms are composed. Really, the same kind of exoticization of Indigenous people, of people of colour is still going on. That same hope, the same kinds of things that drove certain kinds of publications in response to who they think their audience is and what their audience interests are is still ongoing. We need to think more transformatively about what kind of tool journalism is and about the many kinds of journalism that are already out there operating, and what's good about those many different kinds of journalism.

RH: Still with Gans, though, Mary Lynn, it feels like a sweeping statement, "white male social order." Yet all you have to do is just look at the employment data and it bears it out to this day.

MLY: Yeah, and it is. And I think you are right. It is true and it can be more nuanced in terms of language. It's really about the performance of white masculinity — because we don't want to make it essentialized. Even the studies on gender, about the kinds of women

or the traits that women show who get ahead in newsrooms, end up embodying masculine traits to get ahead in newsrooms. It's not just white men. It's the performance of a certain kind of white masculinity in modern journalism that has become the primary authority and legitimized journalism more than its methods. So the fact that you had certain kinds of people with certain kinds of traits telling the news, you know, the major newscasters in charge of news organizations, those were stabilizing authoritative structures... news itself has been stabilized by the performance of a certain kind of white masculinity, and this repeated performance has grounded it.

RH: Candis, there's an expression I was reminded of by your book: "Beauty is in the eye of the beholder." And I thought, "Well, if that's true of beauty, how true might that be of the stories we tell?" If no single unitary narrative is possible, how many eyes do we need on a given day? If you want to step back further, where do we even choose to look? That's where my mind goes thinking about this.

CC: When we were writing the book, a lot of big issues came up. I had been teaching media ethics for a number of years at the journalism school. I had also been helping students wrestle with a lot of the inconsistencies and paradoxical looseness and rigidity of journalism methods of how to get to what happened, of which narratives matter, of whose voices should be prominent. When the book *Seeing Red* came out, I was grateful for historians who looked closely at how Indigenous people had been represented over time in the Canadian Press. The book mounted this pretty solid argument about how Indigenous people were always found wanting by the press and how it is absolutely part of the colonial social order in Canada.

MLY: There are multiple perspectives that are obviously required. It even goes deeper than that though, to the fact that content only matters so much in terms of developing relationships and engagement and trust. It's that relational piece. All of us have worked as journalists. I'm sure we can all remember that newsrooms didn't want to get calls from audience members complaining. People used to actively avoid trying to answer the phone. There was a whole system set in place that the audience could write short letters to the editor but it didn't filter over to the newsroom part, at least in the news organizations that I worked in.

It's the relational elements that have been undermined as they become more commercial. There are multiple elements to that. But the relationship elements have been undermined to the point that coming to that shared perspective requires navigation and relationship and some degrees of trust. We see this breaking down all over and that's what we try to unearth and get at in the book as well. That's why we moved to harm. All these audiences weren't included and or were harmed all these years and we've got to start there.

RH: We're seeing this turn to audience engagement, right? We need to be more "inclusive." We need to be more "responsive" to the audience. And it's almost like a tacit acknowledgement: yeah, all the things we weren't doing before the digital revolution upset the apple cart, upset our monopoly on — or oligopoly, as the case may be — on the creation and control and distribution of information.

CC: That's like the core questions that we try to open up because we think that digital media has opened up these questions. 'How do journalists know what they know?' 'Who gets to decide what good journalism is?'

These kinds of questions, they weren't necessarily asked. They were maybe asked among journalists themselves at the bar or in the newsroom once in a while. But most of the time what Mary Lynn said is right. We sort of had these very small interventions that the public could make or to ask us, as journalists: how did you get that story? How did you arrive at that conclusion? Whereas, with Twitter, if there is real problematic coverage, journalists find out

very quickly and the uproar doesn't stop until it's addressed somehow by the news organization.

Journalism has done some great work, right? We're not saying that it hasn't, but the kind of dominant narratives that journalism has told about what it's doing have not necessarily been problematized. I think they are being more problematized now. I think the newsrooms are beginning to ask questions because they're getting pushback in a digital environment.

So, the arc of the book, the last half of the book looks at how repair and reform and transformation have begun to happen in different kinds of organizations and how hard it is to do at older media organizations, at startups.

MLY: The socialization process in journalism school and in newsrooms is powerful. I'm sure you all remember; I remember being singled out.

My first job when I was starting at the *Hamilton Spectator*, I was sent out to cover the extended bar hours for an hour from 11:00 till midnight on Sunday. I hadn't gone to journalism school yet and I was sent out to interview people... to see if it was busy. And it was really quiet and my lede was "it was quiet on Main Street" (there was a Main Street in Hamilton). So quiet in fact that you could shoot a cannon down the street.

And how did I spell cannon? Not with two *n*'s, unfortunately. Basically, "you could shoot a canon down the street" and that made the blooper board in the newsroom. I remember the censure and the internal practices that newsrooms do to socialize you into not making certain kinds of mistakes and then, as you said, Rick, some things that you can say, some things that you can't say. I mean, it's a powerful organizing force, the newsroom socialization process.

CC: The thing is that one of the real challenges of writing this book and basing it both on sort of a deep study and a deep dive into scholarship and also talking to working journalists right now in various kinds of circumstances — freelancers, newsrooms, etc. — the challenge is really to kind of get to how the interpretive aspect of journalism is also really about socialization, like Mary Lynn was describing.

What is it like to be inside a newsroom? How do we begin to think about that over time, historically, in the present with digital media, how have those things changed? All those kinds of underlying questions are woven into the book as well. I think this is something that journalism scholars have really struggled to also get their heads around.

You mentioned Gans. There have been a couple of other, really famous ethnographies that talk about how it is that stories, frames — the way that we think about what's at issue in a particular news story or what news even is — how much that is determined by the socialization processes in a newsroom. Once you've been working in a newsroom, you do have that sensibility that's hard to describe of what's a news story. What should the lede be? How do we line up what the top stories of the day are? You pick that up in a newsroom, depending on what kind of newsroom you've been in, what kind of platform it's putting the news out on, and who it thinks its audience is.

This is why we ended up doing a chapter specifically on Indigenous journalism because I think that the approach that a lot of Indigenous journalists take explicitly recognizes that they are part of communities, that they are already in an ongoing relationship with lands, waters, non-humans, each other. The question of what are your obligations to lands and waters is already part of how many Indigenous journalists approach their work, which is quite different than the kinds of obligations and responsibilities that a lot of mainstream non-Indigenous journalists might approach their work with.

We all know from *somewhere*. I think that is still a real mind-bender for many who've been kind of socialized and instantiated in a profession that still thinks it can take a view from

nowhere, that still thinks that it's possible to stand outside of your role, of your organization and assess what's going on and come up with a framing and a representation that is in a way omniscient. When in fact, the ways in which we make sense of the world deeply reflects who we are, where we come from, what our experiences have been, and what organization we're part of and who that organization thinks their audience is.

MLY: I wasn't very reflexive as an early journalist. I didn't have a lot of thoughts or hot takes on my backstage thinking and motives and understanding about what the profession was doing and my role in it. It took quite a while, but we devote a whole chapter to journalists' existential crises and the emerging meta genre of journalists' personal stories. They're speculative memoirs, we call them, and they're writing them in real-time and or else they're talking about them in the bar.

RH: Why do journalists, who purport to be deeply skeptical, they don't themselves get to be the subject of skepticism. I always thought it was just like a 'good for the goose, good for the gander' kind of thing. Shouldn't people be skeptical about you, and in turn, you skeptical about the place you work, and its motives and its ability to deliver on what it claims to want to do, serve the public?

CC: Journalists don't like to be critiqued, Mary Lynn said that earlier but I think this is a really big challenge. The challenge around providing good critique for journalists is that journalists are able to take it on board and recognize not just when it's an egregious mistake, but when their framing is not necessarily identifying what the problem is.

One of the things that fascinated me when I started researching climate change was how to make climate change meaningful for diverse publics meant that you had to dive into ethics and morality and reasons why people should change the way they live in order to address climate change. Those sorts of questions also end up falling off the table if you're just focused on a more objective approach to reporting. If you're just reporting the facts then you're leaving off the table what's meaningful.

How do journalists begin to navigate that, especially when it's a pressing issue like climate change? But you could also apply that to Indigenous issues as well. We have a pressing issue when it comes to murdered and missing Indigenous women and girls. We have a pressing issue when it comes to addressing what's happening with Indigenous children who are in care and Indigenous children in the education system. I think these sorts of ongoing issues demand more engaged journalism.

There have been some great examples of that where Indigenous journalists have intervened. Think about the kind of work that Tanya Talaga has done. She's somebody we also interviewed and quote in the book. She says that before the Truth and Reconciliation Commission, before Idle No More, thoughtful, fair, nonracist reporting on Indigenous issues was hard to find.

We're still really struggling to get to the kind of fair and engaged reporting that might change the system, that might actually use journalism as a set of tools to make significant changes or to prod the system towards making significant changes.

MLY: That's a great point, Candis — really great point. When I think about when I was doing my Ph.D. and when you look at the nature of event journalism, you could almost argue that it's a cultural working through. We cover the same events over and over again. We cover the same kinds of crime stories. We cover the same kinds of public events in terms of politics and other areas of the public sphere.

RH: Almost ritualistically, eh?

MLY: Definitely. My most generous read is that it is a cultural working through because when I was doing my dissertation, I had a hard time coming up with a research question. I

knew the topic area I wanted to cover. It was crime journalism, media, economics and gender. But I had a hard time coming up with the question and at one point, my advisor said to me, "Mary Lynn, you can't solve the nature of good and evil in a dissertation. You just have to have a research question." That's when the light bulb went off. I thought, oh, I can do this. But I didn't realize that those were some of the larger conceptual themes that I'd been working through from a very young age. I think that as a culture, these are the connections we need to be making as we're doing this cultural working through on this ritualistic event coverage. Ideally, we're also generating moral values. We're generating wisdom, although I'm hesitant to use that word; we're generating other kinds of knowledge to help our communities make sense of these wider questions that these events called into play. But right now, journalism is just that: ritual. Without the other questions, it is just event after event.

Because when you look at the homogeneity of most news coverage globally, the fact that people across the globe who conform to a certain kind of journalism style and believe that that's the method, how the stories look the same, how they come up with similar angles except when there's a nationalist or a national context piece. It's really impressive how powerfully conformist this genre has been in terms of both people's emotional and ways of being in the newsroom, as well as what the content looks like.

I mean, how is it that Canadian Press style and spelling come to matter more than potential harm or harm that's actively ongoing? How is it that style comes to matter more than everyone you're talking to? People misremember. People are conspiracy theorists. People have inner conflicts. They get things wrong. But, because it is so messy, journalists have had to come to these shorthands. It's much more complicated and people's lives, Rick, as you say, are at stake. We try to bring that to the fore in the book that it is life and death as we quote you as saying and that journalists need to care.

RH: Let me ask you, Candis, is racism a fact... or not? And I'll tell you why I ask: because every time I see it, it's slapped inside quotation marks whenever it's reported on, as if it was opinion, not fact.

Is racism a fact or not, and if it's treated as if it isn't, how telling is that on the part of those who do so?

CC: It's such an interesting question, point, observation about journalism — that racism ends up being a claim that people make as opposed to an observable experience, documented with much precision increasingly over the last century.

We talked a little bit already about the *National Geographic* apology, right? They needed a documented, analytical opinion from somebody who had gone through all of their reporting to say: actually, it was racist. When, in fact, one of the interesting points they make in this story that they do on themselves is that at a time when they were reporting on the crowning of a king in Ethiopia, Black men and women in the United States could not get into a lecture at a *National Geographic* place because everything was segregated still at that time.

So, it's the same thing with objectivity. If you think about the way that the history of journalism has been told, we talk about the rise of objectivity and how it emerged in the 1920s. Well, in the 1920s, who had the vote? Who was able to fully participate in democracy?

When you start asking those kinds of questions, it becomes sort of untenable that the way that journalism has perceived itself as recording a story that gets better over time when you know the experience of many minority populations. Marginalized populations, Indigenous populations have definitely had an uneven experience with democracy, with colonialism, and have often suffered and not had their stories told in anything like mainstream news. In fact, in most cases, at least when it comes to Indigenous people, we've had to start our own media. If

you look at the way that media has evolved, you see the way that Indigenous people have taken it up at every single turn, including when it came to digital media.

RH: Is genocide a fact? Is settler colonialism? What's a fact anymore? I don't understand!

MLY: It's a really good question. And I think, obviously, genocide's a fact as is settler colonialism. Facts, as Candis talks about in her first book, are often settled upon in the social. And when you look at James Carey's definition of journalism, it's a special kind of public conversation and it performs a cultural function. And one of those functions is to, as he talks about and Candis mentioned earlier, create enemies and allies.

But I would also suggest it's this kind of, as Candis talks about, "the communality of facts." It's to help people work through the communality — the coming together — of what makes a fact. And that's really what journalism's function is to both recognize that it's part of a much larger set of systems and to try and help people work through what's more likely to be true than not.

CC: I think it also goes to: if Indigenous journalists or other journalists from communities of colour come into a situation and sort of pronounce on it, they're immediately called out as having some form of bias. And of course, in the book, we argue that coming from a community actually gives you a form of expertise; the knowledge that you have from your community and from your communal experiences is a form of expertise. And it should be treated as such.

It's interesting because the example you're talking about — you could right away say: well, are you biased or are you supporting one particular social order or view of the world or, one hoped for kind of future and present?

I think these are the challenges that we try to open up by recognizing that there are multiple perspectives and that journalism is often making choices about enemies and allies, about the systems that they want to support and reinforce. At the end of the book, we call for a kind of *systems journalism* — so that journalists situate themselves as adding to prior journalisms, as coming from somewhere, and as providing a window through their reporting on events of the way that systems interact and intersect and the ways a particular moment can shed light on broader trends, on broader problems, and potentially also, on broader solutions.

MLY: Candis and I co-teach a course that comes out of the book. One of the early lectures is on what is journalism, and we take many different approaches.

The first approach that journalists would likely all agree to — they would all say, yes, journalism is a fourth estate function. It holds power to account. It is fundamental to democracy.

The second one that we go through is journalism is a professional ideology — basically, it is what journalists say it is. And this is from many scholars, not us. It's how journalists practise journalism, their norms and practices, their ideals. That's what journalism is at any point.

The third one that we go through is that journalism conforms to dominant ideology. Like Noam Chomsky, that it supports state interests; Anderson and Robertson, that it supports settler colonialism; McChesney and other scholars are political economists, that it supports capitalism.

And so arguably, a challenge is that, again, if you go back to Carey, it's a special kind of conversation. Journalism is all of those things. It's such a broad umbrella term. Trying to parse it is so hard when it is doing all of those functions in different ways.

RH: In some ways, I find political/legislative reporters to be in the worst position to try and talk about power because power operates outside the halls of parliament. And if we want to get into these bigger structural pictures of how power works and reproduces itself. Looking at Bill X or Y or whether Party Z is up or down in the polls and all this horse racing stuff and

the scandals are more soap opera and drama. It has nothing to do with helping its audience understand how power works.

It's funny you mentioned Chomsky, though, because never far from my mind in these types of discussions is a scene in a documentary called *Manufacturing Consent*, which is a doc about Noam Chomsky in the media. And there's a *New York Times* editor who was interviewed and he had this to say:

"There's a saying about legislation that legislation is like making sausage, that the less you know about how it's done, the better for your appetite. The same is true of this business. If you were in a conference in which decisions are being made and what to put on page one or what not, you would get, I think, the impression that important decisions were being made in a flippant and frivolous way. But, given the pressures of time to try and get things out, you resort to a kind of shorthand and you have to fill that paper up every day."

So basically, newsrooms are like abattoirs. But, it's funny, as white male dominated space, I guess they are indeed sausage factories.

CC: But this is partly why we argue that, in fact, the crisis facing journalism is maybe more profound. That it isn't just about economic changes and technological changes and new platforms coming along, but it's really about questioning what kind of tool journalism is, what kinds of work can you do with it, what kinds of interventions and transformations can be made. And we can point to many instances in which journalism has done better and we can point to a lot of instances in which journalism hasn't.

We actually teach with that film, *Manufacturing Consent*. We're probably really some of the few journalism professors who do teach with it. It's considered a bit, I don't know, what's the word? Passé?

RH: It's a pre-Internet film so it's dated.

MLY: Rick, you just said it so brilliantly about power. Journalists tend to focus on the horse race and the discrete events versus the fascia or the links between them. There's another scholar who writes about the strategic role of emotionality as a strategic ritual of emotionality versus objectivity. Part of the horse race and part of some of these events are also emotions run amok. You get a certain set of emotions in journalism: outrage, anger, fear, anxiety. You only have to look to some of the COVID-19 coverage, and this is not to suggest that the pandemic is in any way not fearful or anxious. But, journalism adds its own emotional cocktail, ritual of emotion that is, again, under-addressed in some of our conversations to Candis' point.

RH: Well, I'm glad you spoke about this concept of "the news event" because that's something you flag as a key element of current media practices. Let me quote you.

"Journalism's focus and forte of elevating what anthropologists would call the particular through its focus on news events has made it difficult to reflect and name structures and contexts in the reporting."

So, reporters can't see the forest for the trees. They privilege the micro at the expense of the macro. Having said that, changing that — and I guess we're starting to move a little bit into fixing things — changing that sounds like more than just a tweak around the edges, does it not?

MLY You talked about it, your capacity for reflection, earlier. When I look at my trajectory, the discrete events worked for me because my backstage I was still trying to work through the universal. I think it takes a tool kit. It takes a personal tool kit at that level of the self, structural, sociological, cultural tool kit. It takes a lot of tools to be able to start to bridge the particular to the universal and create understanding and be able to digest it.

CC: I think you're right, though, Rick. I think it is more than just a tweak.

And that is the challenge, right? As people who work in journalism education and actively participate in conversations like I do on this podcast; Mary Lynn has co-founded *The Conversation* [Canada]. The kinds of work that we do both in and out of the university are really thinking about how journalism considers its role and thinking about its potential and its possibility to shine light on these persistent episodes of injustice. Because of the event orientation of news — like what's new here, what happened here, that prevents us from really taking into account the broader trends and the broader structural considerations when it comes to gender, race, colonialism, etc.

Turning these questions to the broader problems to which events could direct our attention, that's something that journalism necessarily hasn't always reached for. Certainly in news, that's true; but, maybe more of that has been done in investigative journalism. So, maybe more so in some genres of journalism rather than others. This systems perspective really matters when it comes to looking at certain events where marginalized individuals, communities, and populations have continually encountered a press who have really not taken into account the history. We interviewed Jenni Monet, who was arrested at Standing Rock. She's an Indigenous journalist [Laguna Pueblo], and she talked about how it's important not only just to know that history matters, but *when* it matters. That's an important aspect of journalism and journalism education that I think has been neglected and particularly so in North America, where the history of Canada and the United States when it comes to Indigenous people has largely been erased.

RH: I think it's pretty clear we could talk for many hours more. I mean this is — it's an existential crisis as you've described it. So I want to thank you both for being so generous with your time and for being so generous with your book, your work.

CC: Thanks, Rick, and thanks for having us on. One of the great things that I think is woven through the book is my participation in this podcast. So, thank you for inviting me to be part of many conversations.

RH: Well, I expect some royalties any day now. [laughter]

CC: You and me both. [laughter]

MLY: Thanks, Rick, for your leadership in this space, really. You've done an excellent job on this podcast; it's impressive. Thank you.

RH: Happy to do so. Take care, you two.

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