

The world is not a field – An interview with Michèle Lamont

Interviewed by Anders Hylmö

Abstract

In this interview, Michèle Lamont discusses her intellectual trajectory in relation to other sociologists and research fields. The starting point is her relationship to Bourdieu's field theory in 1980s Paris. She describes how her empirically grounded work developed as a critique of Bourdieu's oeuvre yet draws heavily on it. A common thread in her work is a focus on social and symbolic boundaries and their relation to evaluation and pluralism, which she has extended from her early studies of class boundaries to empirical and theoretical work, ranging from more general cultural processes of inequality and stigmatization, to knowledge production and evaluation in academia. The importance of cumulative work in sociology and the value of a plurality of criteria of worth across different domains emerge as two central themes in the interview. Finally, Lamont reflects on the current status of the sociology discipline and its role in the public sphere. The interview took place in Lund on 7 March 2018, where Lamont was invited keynote speaker at the Sociologidagarna 2018 conference.

Keywords: Michèle Lamont, Bourdieu, symbolic boundaries, morality, STS, evaluation, sociology profession

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Anders Hylmö [AH]: I think of you as a post-Bourdieuian sociologist, if I may use that term. You studied with Pierre Bourdieu in Paris in the early 1980s and in your books, such as *Money, morals and manners* and *The dignity of working men*, you draw upon Bourdieu, but develop and add complexity to the analysis of social and symbolic boundaries.¹ So, perhaps, I could start by asking if you would like to outline your relationship to Bourdieu and how he has influenced your work, and in what ways the latter differs from his approach.

Michèle Lamont [ML]: I came to Paris in 1978 to conduct my graduate work. Before that I trained as a political theorist, primarily in Marxist political theory. I had studied ideology and knowledge and, when I first started discovering Bourdieu's work, I was very intrigued. I remember reading his paper, "Les doxosophes",² which is an analysis and critique of survey research. I found it very powerful, particularly in how it conceptualized the relationship between subject and object, which was the topic of my master's thesis. I started attending his seminars; at the time *Distinction* had just come out. It left a huge impression on me, influenced my thinking in a powerful way, and I spent a great deal of time reading it and trying to understand it.

At the same time, I was critical, maybe because I'm Canadian and have a distance toward French intellectual elite culture. I thought that Bourdieu, a "provincial", had "drunk the Kool-Aid" when it came to thinking about the centrality of familiarity with high culture as a dimension of cultural capital. Although he denounced the symbolic violence of elite culture and its imposition on everyone, he simultaneously exaggerated how important it is in everyday life. In my opinion, there are not many people who care about whether or not you can distinguish between a Rembrandt and a van Gogh – it isn't something that most people think about on a daily basis.

I became interested in analysing which distinctions actually matter in everyday life, at least so far as people draw boundaries when asked to do so. That is how my first project, *Money, morals and manners*, came about. This book came out in 1992. At the time some scholars had been critical of Bourdieu – for instance, Henri Giroux and others emphasized how Bourdieu underplayed resistance and agency. I felt these lines of criticism were rather predictable, given the growing popularity of the Birmingham School and the Gramscian paradigm, so there was much that had not yet been done. In contrast, my book was, I believe, the first that provided an empirically grounded critique of Bourdieu by mobilizing an inductive approach to identify what his framework was missing. At the same time as I was working on *Money, morals and manners*, which argued that distinction did not work as Bourdieu described it, and that Americans embrace a cultural *laissez-faire* that is different from the rigid classification systems

1 Lamont, M. (1992). *Money, morals, and manners: The culture of the French and the American upper-middle class*. Chicago: University of Chicago Press. <https://doi.org/10.7208/chicago/9780226922591.001.0001>; Lamont, M. (2009). *The dignity of working men: Morality and the boundaries of race, class, and immigration*. Cambridge: Harvard University Press.

2 Bourdieu, P. (1972) "Les doxosophes", *Minuit* 1: 26–45.

of French upper-middle class culture, Richard Peterson used survey data to develop the omnivorousness thesis.³ The very first iteration of this omnivorousness thesis is in Peterson's chapter in *Cultivating differences*, which I co-edited with Marcel Fournier.⁴

As a student in Paris, I felt strongly that Bourdieusians believed that the world operated the way Bourdieu described it: like a combative Hobbesian zero-sum field. There were real fights and many people were frankly paranoid because they felt that the world operates like a field, that we are wolves unto one another. I did not want to lead my life based on such premises as I didn't think the world worked that way, at least not always, and not in a consistent manner. My decision to move away from these circles was an ontological decision, based on my understanding of the plural character of human relationships. That is partly what led me to think of morality, because I believed that Bourdieu's understanding of fields was too unidimensional and lacked nuance in capturing how people think about moral issues, and I wanted to do justice to that. This argument is developed in the conclusion of *Money, morals and manners*.

But, at the same time, Bourdieu's work has been enormously stimulating to me and to many others. When cultural sociology exploded in the United States and grew rapidly, starting in the mid-1980s, his growing influence fed the intellectual trend. Because his writings were so stimulating, he helped researchers see many things not previously considered.

AH: Aside from Bourdieu, are there other important intellectual influences that have shaped you as a sociologist?

ML: Just after I completed *Money, morals and manners*, Luc Boltanski spent a year at the Institute for Advanced Studies at Princeton, where I taught. *De la justification* was just coming out then, and I remember talking with him about what he and Laurent Thévenot were trying to pursue in their book – Boltanski said they were going after the blind spots in Bourdieu's work.⁵ They drew on the history of political philosophy to develop a grammar of justification, using the concept of *cit * to consider how people aim to legitimize their views as realizing the common good. My research was also homing in on Bourdieu's blind spot, but from a different direction. I thought it was overly intellectualist to draw on Rousseau in order to understand how people think about the common good. My work was more inductive and empirical because I talked to ordinary people about how they reflect on their understanding of the world and developed what I called moral, socioeconomic, and cultural criteria of evaluation. There was an overlap between what Boltanski and Thévenot aimed to capture with their orders of worth and the criteria I studied. Our projects had real convergence

3 Peterson, R.A. & R.M. Kern (1996) "Changing highbrow taste: From snob to omnivore", *American Sociological Review* 61(5): 900–907. <https://doi.org/10.2307/2096460>

4 Lamont, M. & M. Fournier (1992) *Cultivating differences: Symbolic boundaries and the making of inequality*. Chicago: University of Chicago Press.

5 Boltanski, L. & L. Thévenot (2006) [1991] *On justification: The economies of worth*. Princeton: Princeton University Press.

but were quite different as they did not use the concept of “grammar” and were not engaged in comparative analysis. Also, they were not inductivists, but drew a lot on ethnomethodology – they were explicitly not interested in structure (*contra* Bourdieu), whereas I certainly was.

Rethinking comparative cultural sociology, which I coedited with Thévenot,⁶ is a synthesis of the work that was then being done by their group in Paris, the *Groupe de sociologie politique et morale*, and the work that I was doing with my graduate students at Princeton. This is where I elaborated the concept of “national cultural repertoire” that became quite central to my work. At the same time Thévenot started working on Russia, and he was very interested in thinking about the arrangements that people make for living together in what he called “regimes of proximity” (ways of living together). He was comparing the dormitories at UC Berkeley with communal apartments in the Soviet Union, where families had to share living quarters, and became interested in comparative sociology. We converged, but diverged as well.

Close to them was Bruno Latour, another post-Bourdiesian who, like Boltanski and Thévenot, came to largely define the French approaches that emerged after Bourdieu. *The pasteurization of France* is a truly great book.⁷ When I came to the United States, I went to Stanford where John Meyer was very influential, and he was in the process of developing his world polity theory. He was heavily influenced by Berger and Luckmann. In its constructivism, his work converged with Latour’s, who gave us the tools to understand, in a concrete manner, how a shared definition of reality is institutionalized and black-boxed, which was an important concept to me. At the same time, I also thought that actor-network theory was too power-centred, and a little paranoid, like Bourdieu’s field theory. In France, these were also the years when Michel Foucault was extremely influential.

AH: What is your relationship to Foucault’s work?

ML: Well, I have always been kind of dissatisfied with Foucault, empirically. While many of his concepts are very important for American sociology, he has had much more influence in anthropology because his empirical work was not very detailed. However, I was quite influenced by books by Foucauldians like Paul Rabinow. I reviewed his book, *French modern*, which I found powerful because of the quality of the historical argument.⁸ I have borrowed a little from Foucault, but I have also always had a little distance from him.

6 Lamont, M. & L. Thévenot (2000) *Rethinking comparative cultural sociology: Repertoires of evaluation in France and the United States*. Cambridge: Cambridge University Press. <https://doi.org/10.1017/cbo9780511628108>

7 Latour, B. (1993) *The pasteurization of France*. Cambridge: Harvard University Press.

8 Rabinow, P. (1989) *French modern: Norms and forms of the social environment*. Chicago: University of Chicago Press.

AH: Speaking of French and American sociology, I am curious what your own trajectory has meant intellectually. I am thinking about how you grew up and studied in Quebec, then moved to France, and then continued to the United States to pursue postdoctoral research, and recently you have served as the president of the American Sociological Association. What has this experience of moving between countries, languages and from French to US sociology meant to you and your work?

ML: Quebec is a bilingual society, but I grew up in a francophone environment during the peak of the nationalist movement. In my family we never watched TV in English, and we very much defined our identity in opposition to Anglo-American identity. There was also a strong anticolonial dimension, because France remained imbued with colonial attitudes toward not only Quebec but also toward other countries of the Francophonie (e.g. Morocco) and toward some Latin American countries where it continues to exercise a strong cultural influence (Argentina, Brazil, Mexico). This was particularly evident to me when I studied in Paris, as French social scientists were deeply colonial in their worldviews.

In 1982, I happened to learn of a postdoc at Stanford that I could apply for, and I got it. I went to Palo Alto without having ever dreamed of living in the United States. I had a friend who was interested in René Girard, who taught at Stanford, and we talked about the opportunity. But otherwise I had no concept of what American universities were about, so that was serendipitous, to tell you the truth.

In general, people in Quebec go back and forth between English and French quite easily, and in some sense, we are quite critical of both French and American societies. Being in between helped me understand the internal logic of both societies, how conceptions of worth are articulated differently in the two places. So, I think this particular topic of research came to me naturally. Many American sociologists, especially those who study topics such as poverty, inequality, and communities, are very US-centred. There are literatures that are not cosmopolitan at all. In my case, my books were comparative studies of class cultures, and I was working on cosmopolitan topics from the beginning. I liked the idea of going back and forth between the four intellectual cultures (French, American, Canadian, Quebecois). It suited me very well.

AH: I see. This resembles what you say about parts of this experience in the introduction to *How professors think*, where you describe yourself as an insider who is also, in some sense, an outsider.⁹ It made me think about the way that Patricia Hill Collins has talked about “the outsider within” as a concept in feminist epistemology.¹⁰ Would you tell me about your view on feminist standpoint theory as a body of thought?

9 Lamont, M. (2009) *How professors think: inside the curious world of academic judgment*. Cambridge: Harvard University Press. <https://doi.org/10.1017/s0003975609990270>

10 Collins, P.H. (1986) “Learning from the outsider within: The sociological significance of Black feminist thought”, *Social Problems* 33(6): 14–32. <https://doi.org/10.2307/800672>

ML: Well, when I read Dorothy Smith's work on standpoint theory for the first time, I was not surprised at all. Because it is a direct extension of the sociology of knowledge, which concerns the relationship between the knowing subject and the world s/he studies. Some of Bourdieu's early work was also about that: we are not holy spirits, we don't see reality in its totality.¹¹ When I trained as a political theorist I spent months reading Georg Lukács' classic, *History and class consciousness*.¹² With this background, I felt strongly that people were making Dorothy Smith's work out to be too significant, that it wasn't actually original. But I understand how it played a very important role in the development of feminist circles and theory, because she was applying standpoint theory to demonstrate how mainstream sociology, which claimed to have a universalist perspective, was in fact very partial. However, if you are trained in the history of epistemology, you know this idea is not new.

AH: Let me continue on that note, with the sociology of knowledge. In the volume you edited with Charles Camic and Neil Gross, *Social knowledge in the making*, you argued that we should adopt a lens borrowed from science and technology studies (STS), but turn that lens from the natural towards the social sciences, and study the ways in which social knowledge is produced and used. I think that is a very interesting agenda, and a case where sociology can learn a great deal from science studies. So, I wonder, in more general terms, what is your relation to science studies, and what do you think sociologists in general could learn from that field?

ML: As an enterprise, STS has been quite successful in that it has attracted a lot of scientists who have decided to study social processes in the sciences. It is an interdisciplinary field, and this is also its weakness: interdisciplinary fields do not always focus enough on knowledge accumulation, or at least on refining and developing previous analyses so that we can understand processes with more depth and specificity. People conduct a lot of case studies, and it is not always that clear what each case study adds to what we know already. So that would be my criticism of STS. But at the same time, it is a creative and stimulating community, and I always enjoy conversing with it. Some authors over-claim the originality of their concepts and contributions. For example, if you have read Weber, Mead, Giddens, and other classics, you know that constructionist arguments and arguments concerning the relationship between micro and macro are not that original – knowledge proceeds by innovation and originality, and I am attuned to this. It's a question of respecting the work that others have done and to try to go beyond it.

But I find that scholars like Steven Shapin, Steve Epstein, Karin Knorr-Cetina, Chandra Mukerji, Wendy Espeland, and Marion Fourcade have been extremely interesting and important. They are sociologists too, and bridge sociology with STS. But parts

11 Bourdieu, P. (1972) "Les doxosophes", *Minuit* 1: 26–45

12 Lukács, G. *History and class consciousness: studies in Marxist dialectics*. London: Merlin, 1971 [1923].

of STS are not connected to sociology, and I don't know them as well. At Harvard you have a strong history of science programme where some are very critical of STS, which they view as overclaiming. There is also Sheila Jasanoff, who is an intellectual force of nature and has a large following. We need to find the right balance. Someone like Steve Epstein does this because he masters the history of sociology and doesn't try to repackage things that are well known. There is sometimes a bit of a faddish element in STS when people are repackaging old ideas that may be new to those trained in the hard sciences.

AH: In *How professors think*, you apply such a perspective to your study of the evaluation of social knowledge. You argue that despite very diverse disciplinary epistemic styles and even personal idiosyncrasies, evaluators are driven by a sense of fairness, consideration for diversity, and what you call “cognitive contextualization”, the principle that proposals should be judged according to the standards of the discipline of the applicants, rather than evaluators promoting their own personal epistemic styles. In a nice formulation, you say that this methodological pluralism produces universalism. I wonder if you could expand on the parallels between your broader work on social and symbolic boundaries, and this work on evaluation and epistemic styles in academia?

ML: *How professors think* is based on what evaluators say they do. The argument is also that the people who agree to serve on research grant committees are invested in the notion that these committees are worthwhile, and they think the committees' work is worthwhile in part because they try to behave in a universalistic manner, and they believe that most people who are involved also try to behave in a universalistic way. Or so they told me when I interviewed them. I am not saying that it is a pure universe in which everything works wonderfully, but in the context of the interviews, they described what they do to formulate what they believe is a fair judgement. For instance, they talked about “cognitive contextualism”, or giving more weight to the evaluative criteria of the discipline of the applicant, than to their own disciplinary criteria. Note, however, that this is a context where they distribute funds that are not going to benefit themselves directly – it is a national competition. If I had done interviews about, for instance, the evaluation of candidates for graduate admission in their own departments, where it is a zero-sum game, and where people may have vested interests in getting more people like themselves, more students for them to work with, it is likely that I would have heard descriptions of different practices.

I return to the work on symbolic and social boundaries: one of the basic ideas in *How professors think*, which parallels some of what I have done in other areas, is the notion that mobilizing a multiplicity of criteria of evaluation is what is most enabling, because it allows for various kinds of excellence to coexist. So, in sociology for instance, we should favour methodological pluralism over methodological tribalism, as I have argued in my paper on interviewing, co-authored with Ann Swidler.¹³ When it comes

13 Lamont, M. & A. Swidler (2014) “Methodological pluralism and the possibilities and limits of interviewing”, *Qualitative Sociology* 37(2): 153–71. <https://doi.org/10.1007/s11133-014-9274-z>.

to evaluating the worth of people under neoliberalism, I have also argued that we should avoid using a single set of criteria and instead value a plurality of criteria.¹⁴ Those are parallel elements, the idea that boundaries should be somewhat flexible, permeable, and multiple, that it is better to have that than the kind of doxa that is constructed around a single set of criteria.

AH: It is now almost a decade since *How professors think* was published. I am wondering, in the light of the rapidly increasing use of bibliometric indicators as evaluative criteria, a development that has been called a “metric tide”, what are your thoughts on the current and future status of peer review?¹⁵

ML: *How professors think* promotes connoisseurship in evaluation, and I believe that quantification can't replace this. There is a push among proponents of open access to spread the idea that anyone can review. In my view, the idea that any reader can be a reviewer is nonsensical and will lead to the de-professionalization of academic work and a decline in the value and respect of knowledge. Reviewing presupposes a degree of expertise, and the reason why we ask some people rather than others to evaluate is because they have a familiarity with the field and can recognize what is novel. At the same time, we know that the system is in crisis, that it is more and more difficult to get people to do the work of reviewing, that many journals have to ask a great many people before they find reviewers, and that we are all exhausted with reviewing. This calls for a much broader conversation and the solution may be a turn toward “slow scholarship” and a move away from “publish or perish” which favours quantity of papers over quality.

Often, *less is more*. If we could all publish only two papers a year, and it was understood that this number is what is reasonable, we would have many more consequential works published. There is room for a much broader discussion about how to promote the quality of work, not the quantity. But although this is quite clear at a place like Harvard, many universities fear making the judgement of quality themselves. There are departments where it is just about counting the number of papers published; this route is the end of scholarship. Relying on the ranking of journals doesn't make sense either, because many good papers are published in journals that are not highly ranked. People really must engage in deliberation about the quality of the work, this cannot be avoided.

14 Lamont, M. (2018) “From ‘having’ to ‘being’: self worth and the current crisis of American society”, Lecture given at the LSE on 25 October, 2018. <http://www.lse.ac.uk/lse-player?id=4537>

15 Wilsdon, J., et al. (2016). *The metric tide: Report of the independent review of the role of metrics in research assessment and management*. London: SAGE. <https://doi.org/10.4135/9781473978782>

AH: This theme of quality judgement and evaluation spans a lot of your work, and it is the focus of your review piece on the sociology of valuation and evaluation, where you talk of this in terms of a very general social process.¹⁶ This nascent area of study spans at least cultural sociology, economic sociology, and science studies. What is your view of the field of studies of valuation and evaluation today, and where do you see it heading?

ML: In fall 2017, I was at the 4S meeting in Boston, and there was a day-long mini-conference on academic evaluation organized by Steve Woolgar and others at which I was a discussant.¹⁷ There was an abundance of good papers. Moreover, in some countries, such as France and Germany, sociology of valuation and evaluation is hot, and in Switzerland there is plenty of public funding available to study evaluation and peer review. As a reaction to the Shanghai rankings, some countries, departments and universities have become worried about their position in international rankings, and this has generated a lot of interest in the sociology of (e)valuation.

Then again, a point I make in that paper is that we need to think about how to develop a more cumulative agenda in the sociology of evaluation so that we don't have the same arguments being made 20 times over. We need to encourage scholars to bring together case studies, compare findings and to think more systematically about these processes. I have another recent paper with Stefan Beljean and Matthew Clair that systematically compares evaluation and other kinds of cultural processes (commensuration, stigmatization), asking "how does it work?"¹⁸ We should learn across cases, and connect literatures between economic sociology, cultural sociology and science studies.

AH: You have said that sociology as a profession seems to suffer from self-hatred, and that, in contrast to economists, "many sociologists behave like kindergarteners when it comes to increasing our impact". You have also argued that we must get better at thinking strategically about how to increase our public impact.¹⁹ Would you care to expand on these ideas today? How should we act as a profession?

ML: I said that in the context of the United States, where economists have the Council of Economic Advisers to the president. They have devised institutions that allow them to influence what is happening in Washington in a very systematic manner. They created a "fake" Nobel prize – they are ingenious when it comes to creating such tools.

16 Lamont, M. (2012) "Toward a comparative sociology of valuation and evaluation", *Annual Review of Sociology* 38(1) 201–21. <https://doi.org/10.1146/annurev-soc-070308-120022>

17 The panel on *Academic evaluation in an age of "post truth"* at the 2017 conference of the Society for the Social Study of Science was convened by Mario Biagioli, Claes-Fredrik Helgesson and Steve Woolgar.

18 Lamont, M., S. Beljean & M. Clair (2014) "What is missing? Cultural processes and causal pathways to inequality", *Socio-Economic Review* 12(3): 1–36. <https://doi.org/10.1093/ser/mwu011>

19 Meer, N. & Lamont, M. (2016) "Michèle Lamont: A portrait of a capacious sociologist", *Sociology* 50(5): 1012–1022. <https://doi.org/10.1177/0038038516646283>

There are also many people who are ABDs²⁰ and who write about economics on social media and in the press. The same holds true for psychology, which has for instance a radio programme called *Hidden brain* on National Public Radio (NPR). These outlets are important for knowledge diffusion, and sociology is not well served in this respect. Just yesterday morning I was listening to the radio while getting ready, and there was a piece about how economists had discovered that birth rate varies with economic prosperity. That is a finding from demography that goes way back, but the journalist was stating that an economist had just discovered this fact!

We need to be far more strategic. More sociologists should be hired by the media to promote sociological knowledge. There is much more we could do. There was a gigantic fight amongst French sociologists last year in the pages of *Le monde*; they were going at each other without any notion that it makes sociology look bad. We should remember that there is a contest around who shapes the public sphere. For the future of our societies it matters if only economists have influence because what we have to say is important in terms of where the world is going.

AH: That is agreed. But thinking about this in connection to your recurrent emphasis on interdisciplinarity and intellectual pluralism, I wonder about your views on pluralism within sociology. Do you see any tension between pluralism within the discipline on the one hand and, on the other hand, a strengthening of our professional identity as sociologists and the impact of sociology? How do the disciplinary boundaries of sociology relate to our ability to constitute a distinct sociological theoretical approach?

ML: This is an excellent point, because many people who think that sociology is not prestigious or taken seriously enough would like us to be far more like economics, more consensual and less pluralistic. People react to different challenges in different ways. Look at what has happened to political science in the United States, where it tries to look more like economics. I don't think this has served them very well. Many students come to political science to study politics, but can't because so much of what they end up studying is methods. The fact that sociology has not gone in that direction actually serves us well. We are multi-paradigmatic, and that is a strength, not a weakness. We need to embrace our breadth.

AH: LET ME turn to your recent book, *Getting respect*, where you and your co-authors picked up the political-philosophical writings of Axel Honneth and Nancy Fraser on recognition and redistribution, and turned them into an empirical sociological study.²¹

20 "All But Dissertation", American term designating graduate students who have completed their doctoral coursework, but not their dissertation.

21 Lamont, M, G. M. Silva, J. Welburn, J. Guetzkow, N. Mizrachi, H. Herzog & E. Reis (2016) *Getting respect: Responding to stigma and discrimination in the United States, Brazil, and Israel*. Princeton: Princeton University Press. <https://doi.org/10.23943/princeton/9780691183404.001.0001>; Fraser, N. & A. Honneth (2003) *Redistribution or recognition?: A political-philosophical exchange*. London & New York: Verso.

I think of that as a nice example of the way you have brought concepts and ideas from other fields and made them sociological. Would you elaborate on that?

ML: Well, I don't think we did so much with their work actually. I thought that their debate was very helpful in articulating the need to move the sociology of inequality, which had been focused on distribution, toward a greater consideration of recognition. Our mobilizing their work was more a framing device than a true extension of their work. Their debate was mostly about identity politics. The issue of privileging identity over class is now totally moot, as most people acknowledge the value of studying both topics.

AH: It seems to me that the rise of the new right-wing populism, in North America as well as in Europe and here in Sweden, has meant that issues of cultural inclusion and exclusion, which have been so central to your work, have become much more salient in recent years. For example, you examined very recently how Donald Trump both appealed to and strengthened the moral boundaries of the white working class in the United States.²² How do you understand this shift, and how do you think sociology can make a difference?

ML: In *How professors think* I argue that different disciplines can shine different lights on these developments, and that disciplines produce knowledge that is complementary. The kind of light we shine on the phenomenon focuses on group boundaries and resonance between electoral discourse and the groups that Trump targets (as audiences or scapegoats). Political scientists ask the question: “is it culture or the economy?”²³ To me it is about both – it is a question of status positioning.

Some economists²⁴ are concerned with the question of whether populism is stronger in places where more jobs are being lost to China, so they have a direct economic determinism. My colleagues at Harvard, political scientist Steven Levitsky and Daniel Ziblatt, just published a book, *How democracies die*,²⁵ which was on the top of the *New York times* bestseller list for several weeks. As experts on populism in Latin America and Europe, they look at cases where democracies died in the past. They describe different conditions that lead to the destruction of democracy, and these conditions are very

22 Lamont, M., B.Y. Park & E. Ayala-Hurtado (2017) “Trump’s electoral speeches and his appeal to the American white working class”, *The British Journal of Sociology* 68(S1): 153–80. <https://doi.org/10.1111/1468-4446.12315>

23 Mutz, D. (2018) “Status threat, not economic hardship, explains the 2016 presidential vote”, *Proceedings of the National Academy of Sciences* 115(19): E4330-E4339. <https://doi.org/10.1073/pnas.1718155115>; Gidron, N., & Hall, P. A. (2017) “The politics of social status: Economic and cultural roots of the populist right”, *The British Journal of Sociology* 68, S57–S84. <https://doi.org/10.1111/1468-4446.12319>

24 Autor, D., D. Dorn, G. Hanson & K. Majlesi (2016) “Importing Political Polarization: The Electoral Consequences of Rising Trade Exposure”, NBER Working Papers No. 22637.

25 Levitsky, S. & D. Ziblatt (2018) *How democracies die*. New York: Crown.

much tied to the rule of the law and other elements that are essential to political theory. This is important as well, and understanding the puzzle requires a multidimensional enterprise, with each discipline shedding light on different parts of what is happening. We sociologists can contribute a better understanding of group boundaries and struggles over group positioning.²⁶ Cultural sociology can help us understand cultural polarization, how social media creates political “silos”, etc. These are all sociological questions that have to do with how the public sphere functions concretely, and what and who feeds it.

AH: Apart from that, if we look at international sociology today, can you point to any specific research areas that you either find theoretically thrilling, in need of development, or that cover really pressing areas, where you think that sociological research can make a difference for society?

ML: There has been a lot of important work on globalization, but there is a need for more empirical specificity. I really like the work of Giseline Kuipers at the University of Leuven, among others. She has studied areas such as dubbing in films and concepts of beauty and fashion. Her work provides a detailed understanding of globalization and the processes of exchanges. There is also important work being done by Margaret Frye and Ann Mische on how people define their future selves.²⁷ The black-boxing that is involved in projecting oneself into the future is important, and it is attracting attention.

I am also interested in the comparative study of middle-class formation to better understand how it varies with state capacity, the extent of inequality within a country, how exclusive is the status culture, etc. What does distinction look like in an African country, such as Uganda, where there is a weak labour market for professionals and managers? Where people can go to university, gain skills, engage in high-status cultural consumption, and yet not be employed as professionals? I think that there are a lot of interesting questions around these questions and, indeed, we organized a conference around these themes at Harvard in fall 2018.²⁸

26 See also Mickey, R. & L. Way (2019) “Can American Democracy Survive the Loss of ‘White’ Dominance?”, presented at 2019 Successful Societies Program Meeting on European and American Populism and Group Boundaries, Banff, Alberta, CA.

27 Frye, M. (2012) “Bright futures in Malawi’s new dawn: Educational aspirations as assertions of identity”, *American Journal of Sociology* 117(6): 1565–1624. <https://doi.org/10.1086/664542>; Mische, A. (2009) “Projects and Possibilities: Researching Futures in Action”, *Sociological Forum* 24(3): 694–704. <https://doi.org/10.1111/j.1573-7861.2009.01127.x>; Beckert, J. (2016) *Imagined futures: Fictional expectations and capitalist dynamics*. Cambridge: Harvard University Press.

28 The conference, Changing Middle Classes: Comparative and Global Perspectives, was held at Harvard University on September 21–22, 2018. <https://wcfia.harvard.edu/conferences/18-changing-middle-classes-comparative-and-global-perspectives>

AH: You are known as not only a prominent researcher, but also a very engaged teacher and institution-builder. I wonder, is there any general advice that you would give to young PhD students at the beginning of their careers and, on the other hand, to senior sociologists on creating the institutional conditions for younger researchers to thrive in?

ML: If you are committed to the prosperity of the social sciences, making sure that it is attractive to young people is crucial. As a current example, the #MeToo movement is doing a lot to create an environment in which youth want to lead their lives, where they find their values reflected. We have to think about what kind of work culture we create. I am the director of the Weatherhead Centre for International Affairs at Harvard, and I took on this role in part to help change the university in a direction that better expresses my feminist values.

AH: I think this takes us nicely back to where you started with your moral orientation and the critique of the implications of Bourdieu's field theory. Is there any advice that you would like to give to young sociologists?

ML: My main piece of advice is to do things you believe in, because otherwise it is hard to continue. People sometimes become cynical. Intellectual passion is a fire that needs to be fed, and if you only do things that you feel you have to do, work becomes so instrumental that it isn't fun anymore.

At the same time, there is a period in one's life when you have to buckle down. You have to maximize how meaningful the work is, but you also have to be dedicated enough to push through for long periods of time. One of my mottos is "long-term pain for long-term gain". If you do research that is easy, focusing mostly on low-hanging fruit, the work is unlikely to have an impact. Thinking back, *Getting respect* was a really complicated and daring book. It is an example of a project that was truly challenging, but I am happy my co-authors and I wrote it, even if we all nearly died doing it. I don't know, frankly, if it is going to have an impact, but it gives me great satisfaction, as an important book and a worthwhile endeavour.

AH: Thank you very much for your answers!