Interview

Sociology
1–11

DOI: 10.1177/0038038516646283
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Michèle Lamont: A Portrait of a Capacious Sociologist

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Keywords
biography, disciplinary identity, interview, methodology, Michèle Lamont, *Sociology* 50th anniversary, *Sociology* journal

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(NM): *Perhaps I could start by asking you about your work in recent years on knowledge production – to what extent does this mark a departure from your studies of class and capital?*

(ML): In my mind, there is a lot of continuity, in that *Money, Morals, and Manners* (MMM) (Lamont, 1992) and *The Dignity of Working Men* (DWM) (Lamont, 2000) are both about high status signals and how excellence is defined within certain milieus of the working class and upper working class, respectively. In *How Professors Think* (HPT) (Lamont, 2009), I am studying professional cultures of the humanities and the social sciences, manifested in the deliberation

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of these interdisciplinary panels, to understand how people recognise excellence. Just as in MMM and DWM, I am exploring boundaries and concepts of worth by asking: Who is similar to and different from you? Superior and inferior? Who do you admire? But for HPT I also ask: Who are your best students and what qualities do they incarnate? Who are your intellectual heroes? In all three books, I consider morality as a basis for drawing boundaries, as well as how fluid (or classed or racialised) those boundaries can be. Moral status signals were central in MMM and DWM, and in HPT morality manifests itself through originality (as non-conformism) and through valuing methodological and epistemological pluralism, mostly. It is true that HPT was particularly reflexive about the academic world that I inhabit. For its part, MMM came out of my experiences studying with Bourdieu and living in the French and the American upper middle class, in Paris and Palo Alto, California.

NM: *If there is one sociological theme that narrates your work it is the notion of boundaries, and how you have taken it and advanced it in different directions without necessarily anchoring it in a given approach. I suppose, if you adopt such a degree of methodological pluralism in your enquiry, are you doing sociology or are you doing something that is simultaneously multidisciplinary?*

ML: Well, I think that the word sociology, as a referent, is very much connected to the reality that we experience and perhaps when you are thinking of sociology in the British context, it is quite different from sociology in American contexts. I am President-Elect of the American Sociological Association (ASA) and an overseas friend recently told me how unbelievably unlikely this seemed to her. Her views have to be understood against the background of her perception of American sociology as this kind of intellectual monolith where quantitative research reigns supreme, and where everyone is working based on research designs that involve a two times two table or dependent and independent variables. Living inside American sociology, I am much more in tune with its ebbs and flows and the contested forms of knowledge it encompasses.

Concerning my positioning in relation to disciplines and multidisciplinarity: I am certainly a sociologist first, but, unlike many sociologists, I am very interested in the broad ecology of the social sciences and the humanities as a whole, as is evident in HPT. Having written on interdisciplinarity (Boix Mansilla et al., 2015) and having directed an interdisciplinary team since 2002 (the Successful Societies Program), I am particularly interested in the conditions for successful interdisciplinary collaboration and hope to speak to audiences beyond my own discipline – as was the case in the two collective books that came out of our programme and which I co-edited with Peter A Hall (Hall and Lamont, 2009, 2013).

As for the question of methodological pluralism and the quant/qual divide: I belong to a group within the ASA that took a gamble on transforming American sociology from the inside and I think that the Culture Section of the ASA, which was created in 1986 and is now one of the two largest

sections, with over 1000 members, really speaks to how the discipline has changed internally. There are some people involved in this section who are quantitative, but as an intellectual enterprise, the Culture Section is very *verstehen* oriented and certainly focused on questions of narratives and interpretation.

I say that I am a methodological pluralist to the extent that I regard data gathering techniques simply as tools (Lamont and Swidler, 2014). There are good and bad questions, and good and bad theories and books and articles, but there are no good and bad methods. The method is as good as what we do with it. At the same time, my own research has been primarily interview-based, so I have not been *that* pluralist in my own sociological production, but I have students who have done all kinds of work. I have come to the conclusion that one of the reasons the field I work in has been attractive to many is because it functions as a big umbrella under which various types of scholarships can rest.

I want to return to the important topic of the ecology of disciplines in the social sciences: at times, it feels as though American sociologists have been too inward-looking, while economists have had a monopoly on public and political influence, and cognitive psychologists garner attention on the *Op Ed* pages of the *New York Times* with articles on topics such as 'What is the impact of having too many types of ketchup to choose from?'. Conversations internal to cultural sociology on who has the biggest question or the fanciest framing are irrelevant when we think of the great debates of the day (e.g. the rise of populism in Europe and the United States) and the role that the social sciences should be playing in moving us away from the dominance of utilitarian and individualists thinking about social issues, which is so predominant in the American public sphere.

I must confess that I am growing extremely impatient with this and I think that we elude our social and political responsibilities if we are not taking on the challenges of influencing the public sphere. It's not that I take the same position as Michael Burawoy – that sociologists should serve as organic intellectuals for the progressive forces of society. That's not where I stand. I think we play a very important role in maintaining or fostering pluralism in the public sphere. There are a lot of perspectives and types of questions that are not being asked now and we, as well as anthropologists and political philosophers amongst others, have the intellectual equipment needed to bring these questions to the fore. Our political duty is fostering a richer democracy and generating greater social inclusion, if you will, especially given the challenge that the United States has been facing with the ascent of Donald Trump.

I tend to be optimistic about what is going on in sociology. I don't want to be Pollyannaish about the current state of American sociology. Increasingly, more people are coming to sociology because they are interested in addressing questions that are not being studied in other disciplines, such as American political science, as this field is becoming progressively narrower and more

technical. People come to sociology because they want to study power, for instance. Or else they are interested in incarceration or extreme poverty, and I think that is crucial, but at the same time we want to keep in perspective the importance of more theoretically driven work. I am a little concerned that if the discipline becomes too applied or problem focused we may lose the intellectual depth that comes with more conceptual approaches. At the same time, I think that the discipline of sociology in the United States has benefited greatly from focusing on perennial social problems (poverty, racism, immigration) and influencing policy. We just don't want to become slaves to it, or measure our worth solely or primarily in terms of 'impact' or contribution to social or economic innovation.

NM: *You said two things that I felt are possibly in tension. One was about the extent to which it is incumbent on sociologists (you mentioned duties) to be engaged in the public sphere, and the other was to retain a theoretical anchor. Can you be sufficiently theoretically informed when you are doing more applied work or do you have to compromise your theoretical framework?*

ML: I think, if you want to have social impact you have to use a language that can travel and that is often quite incompatible with writing that is more innovative theoretically. So, it falls upon us to provide different versions of the same argument that are suited for different publics. I think for me as a cultural sociologist the concepts of frame, repertoire, boundaries, narratives, or identity are absolutely essential as those are the concepts that allow us to ask questions different than those asked by cognitive psychologists, or to differentiate ourselves from old perspectives on culture influenced by Talcott Parsons with his concepts of values and beliefs. So, concepts matter very much. I was mentioning to a friend yesterday that I remember very well, at the age of 14 or so, reading Germaine Greer's book *The Female Eunuch*, where I discovered the word 'stereotype'. The very existence of concepts was a true revelation to me because it allowed me to name a phenomenon I could intuit but could not capture without the proper vocabulary. Once you understand that social stereotypes exist, you can look at social life through a different lens. Similarly, our discipline provides many crucial analytical tools. Sociology is often put down for using terms that are not commonsensical, but these tools are indispensable for what we do.

Often, the pure and applied sciences have a greater legitimacy as shown by how the general public talks about their findings and research. Disciplines such as chemistry require enormous resources and lab space. These resources help create consensus around which knowledge producers matter and which types of knowledge are fireproof (what Bruno Latour coined as 'the black boxing process'). Resources are crucial in enabling chemists to form consensus around scientific findings and relative academic status. Our own vulnerability as a field is that it is not tied to the distribution of many resources and this works against the black boxing process for sociological knowledge.

In American sociology, economic sociology has had a large influence, very much paralleling the ascent of cultural sociology, and both have had as an agenda to deconstruct assumptions from economics, about the

construction of value for instance. The public influence of these dynamic subfields remains somewhat limited and many sociologists continue to define their identity in relation to one another and in contrast to the public influence of economics or cognitive psychology. Within these subfields, we find a fascination for technical proficiency – the spread of 'big data' as a case in point. People get seduced by the idea of collaborating with others using 'big data' because it is easy to get money to do it, and computer scientists have zero theory and are looking for social scientists who would be willing to play around with it.

NM: *So you're not persuaded that 'big data' is the new horizon for sociology?*

ML: It is in terms of ease of access to funds, but perhaps not in terms of theoretical contribution. By definition, given its inductive character, big data research involves zero theory and there is a bandwagon effect: people think that those who do it are hot and with it. There is a very good article by a former student of mine, Chris Bail (published in *Theory and Society*), which is kind of a plea to use theory when engaging with big data. Using big data to identify patterns may be interesting, but not necessarily significant: it all depends on the theoretical framing. There are many ways of framing questions and to think that data will speak by itself is simply naïve empiricism. This can be a danger and we have to proceed cautiously and not let the tail wag the dog.

NM: *The question I have then is – to what extent is sociology a problem driven activity, or is it something that is anchored in methodological practice, be it in data analysis or be it something else?*

ML: One of the many strengths of sociology is its multi-perspectival nature. We can focus on the micro, meso, and macro. The fact that the ASA has 50 some sections is evidence that the discipline is a multi-headed hydra that has many shapes and forms, in part because we take on a range of questions and deal with many levels of reality. Each of them may be best approached through different analytical tools (e.g. world system concepts don't work too well for understanding face-to-face interactions). Research that is conducted in the business schools is typically problem driven, but this is not true of most sociological research – we are often theory driven, but some sociologists do problem driven research. As long as the boundaries of the discipline remain quite flexible this diversity remains a strength.

I should add that I am not one of these sociologists who suffer from physics envy, who thinks our disciplines should have clearly defined concepts (a sociological periodic table of sorts) and a shared vision of all our intellectual gains so that we can all engage similarly in building cumulative knowledge in a linear way. There is quite a bit of cumulative knowledge being constructed by sociologists, but often progress operates more through elimination or obsolescence (i.e. through literatures falling by the wayside). Our canon is not clearly delineated. Our introductory books are often ridiculously outdated – for instance, they continue to describe the discipline as organised around functionalist, conflict, and symbolic interactionist theory. Yet, the

discipline appears to me to be quite vibrant and generative. So we do move forward without having books that would be the equivalent of Samuelson's textbooks on micro and macroeconomics.

Earlier sociologists of science had this notion that knowledge accumulation would follow a linear pattern, based on a fantasy of how physics would have been practised in the 1950s. As Knorr Cetina (1999) shows in her book *Epistemic Cultures*, even high-energy physics does not function like this. It's more like they debate – What are the next questions to be addressed? What are the regions of knowledge production that are most likely to be fruitful? Other questions fall by the wayside. When I came to the United States to Stanford in 1983, the Department of Sociology was led by Joe Berger who developed 'status expectation theory'. There, each member of the school were supposed to add, one by one, bricks to a wall of theory building, and the theory had to be internally coherent. These researchers operated on a Popperian model that was directly borrowed from the hard sciences, and their ideal was to produce a sociology that was parallel to this. That tradition has produced some really fantastic sociologists, like Cecilia Ridgeway, but there was an awful lot of work that came out of that tradition that turned out not to be very significant and it was developed at times based on this fantasy of how knowledge accumulation works.

These days, social scientists are often arguing about limits to progress in economics, psychology and political science by pointing to failures in replication and falsification. Luckily, but for a few exceptions, our discipline is not as concerned with replication. Again, we operate by obsolescence, just like most scientific disciplines. But myths are performative … and they do influence how sociologists think their discipline is doing in comparison to others. This partly explains why we find a certain amount of self-hatred among sociologists.

This being said, I am very opposed to social scientists trying to reinvent the wheel without giving due recognition to the work that others are doing. So, for instance, there is an enormous amount of research being done in anthropology on immigration in the United States and some do not read the large literature that American sociologists have produced on this topic for decades. And, vice versa, I think there is a lot of work that anthropologists have produced on race over the years that American sociologists have been ignoring. So it is partly about competing disciplinary projects, boundary work, and building distinct sandboxes where one can claim originality. I think we need to try to be respectful of the time and effort people put into knowledge production and make sure that we pay due respect. It is a basic question of honesty and professionalism.

NM: *There are several points that you touch on there that are really fascinating. Despite the tension between economists, economics as a disciplinary badge or disciplinary concept in people's minds is quite resilient.*

ML: Economists are quite extraordinary in their ability to behave in conformity with their perceived self-interest. They are really good at self-promotion and they are competitive with each other in terms of putting their hands on

resources, influencing policy-makers and making sure that their work comes out in outlets where it will have an impact. In contrast, many sociologists behave like kindergarteners when it comes to increasing our impact. There are a number of younger sociologists who are getting impressive contracts from publishers, and there are several books on inequality that are likely to have a big impact on the public discourse. This is really important. As a profession, we need to be much more attuned to the hegemonic discourse that economists produce on themselves, and their corporate spirit when it comes to promoting their discipline as a whole. Again, what we do matters a lot for the diffusion of less individualistic frameworks for interpreting societies. We have to be more present in the public sphere because of what our work can empower politically or otherwise.

Of course, economics is also full of factions. You can be sure that labour economists and behavioural economists have very little in common and they certainly think that their respective intellectual programmes are not working in sync, so we don't want to underplay the internal diversity of that discipline. Yet territories that were sociology's not that long ago now have to be shared with economics. It is the case for culture, for instance – even if for the most part they operate with an obsolete concept of culture that is inspired by Margaret Mead.

There is a risk that at the end of the day sociology will be left with the study of networks, as they are often conceptualised as operating at the meso level, which is where sociologists are best positioned to claim superior knowledge. My view is that we need to play our cards well and on a terrain where we are strongest. Unlike political science, we have escaped the tragic temptation of defining ourselves as emulating economics. Nor do we aspire to be the second best at cognitive science. We should think about the types of questions that we are uniquely equipped to answer and go after those. The fact remains that we are a discipline that can work at a micro level and a macro level with different intellectual tools, and working with connecting these levels at the meso level (through institutions, neighbourhoods, organisations, networks, and cultural repertoires) is one of our unique strengths; again, we should play on these strengths!

NM: *Your example of sharing terrain goes to the heart of what we are trying to explore in this special issue. I wonder whether you see any benefits coming back to sociology?*

ML: Well, it is well known that economists rarely cite non-economists. The only realistic response to this is to try and produce research that will have such an impact on the public debate that they will have to address it. I don't think whining is ever a good solution! There are foundations that have decided to allocate a huge amount of resources to defusing the work of economists and to ignore cultural sociology. So maybe we need to be a bit more strategic and ask: What is it that we want to accomplish as a discipline? Where are the foundations and organisations that are able to help us set an intellectual

agenda while combining resources? What influential media can we have access to which can allow us to bypass gatekeeping from other disciplines?

In *Homo Academicus*, Bourdieu (1984) was right in pointing to competing disciplinary agendas. Take the example of the influential book *Scarcity*, which analyses how the poor think (Mullainathan and Shafir, 2013). This book does not take into consideration how focusing on structural contexts and material resources helps us understand the ways the poor make sense of their situation. The authors' approach to poverty could not be more different than sociological approaches, as decision making is key in their framework. Yet *Scarcity* is a book that received a great deal of coverage, and I would say that sociologists should have an agenda to match it in influence and in defining what kinds of programmes need to be in place to address it. The book *Evicted*, recently published by my colleague Matt Desmond (2016), does this beautifully.

NM: *You mention Bourdieu, I suppose he is a sociologist who has not only dominated sociology but also the social sciences more broadly; in many ways he has given a sociological language to non-sociologists.*

ML: Of course his work has been enormously generative and in the American context there are a number of people advocating using it in a canonical way. A few sociologists may not be happy that people like me are using the concepts of field, capital and habitus independently of each other. Some people wanted American sociology to become 'orthodox' Bourdieusian, which frankly did not work at all. Yet, the discipline has been enormously enriched both by Bourdieu's work and by innovations that came from people pointing to his blind spots. That is what I have done in MMM, and frankly I think this is why my first book was influential. It is great if Bourdieu's work serves as a source of inspiration, if it helps put new ideas on the table. For instance, among scholars who are studying knowledge communities, the sociology of evaluation (Lamont, 2012) is a booming field right now. I could go on and on pointing to developments that can be traced back to Bourdieu's work, but that have gone beyond his highly predictable field analysis.

NM: *The other orientation is also French social theorists …*

ML: I think very highly of the work of my collaborators Laurent Thévenot and Luc Boltanski, as well as the work of the next generation of scholars they trained – Nicolas Dodier, Cyril Lemieux and others. I still exchange with some of them. Their work is diffusing internationally and is feeding the growing interest in pragmatism in the United States – even if many American sociologists do not really understand what their work is about and what it was responding to, as a critique of Bourdieu's obsession with power. But there is more. There is a huge fad for Bruno Latour in the United States in disciplines that are not social scientific. In ethnomusicology and in schools of architecture people are reading Latour and that makes sense given their concern with materiality. Yet, as you know, Latour is borrowing an enormous

amount from Howard Becker and symbolic interactionism, and at the same time he is openly hostile to sociology and he often presents his ideas as independent from their sociological sources. I have no issue with those French theories becoming influential, but my problem is that sometimes people who read them have little background in the history of theory, and are simply wowed by the theoretical firework. Those trained in pure sciences who move to science and technology studies may not know that phenomenology exists, for instance; they may think the notion of social construction has been invented by Latour. This is not what the enterprise of the University is supposed to be about, so that is why I am more than a little bit concerned.

NM: *You mentioned verstehen right at the beginning and when I think about methodological approaches I think about their relationship to disciplines. Yet presently verstehen is something that is seen as instinctive to anthropologists but a labour to sociologists.*

ML: I wouldn't say that because if you look at what is happening in historical sociology, much of the literature on social or culture processes is about configurations and about context. How can you analyse configurations and context without doing comprehensive sociology à la Max Weber? Quantitativists and qualitativists are doing interpretation when it comes to the production of social science knowledge and the key question for me can be: 'How much do they engage in data reduction?' In my work, I am often drawing on a large number of interviews, and I will have frequency counts. This requires data reduction; it is interpretive work but at the same time, frequencies are useful to display patterns that emerge from the data considered as a whole. That's quite different than illustrating a theoretical point with a few chosen examples, without knowing how significant this example is when considering the data in its entirety. But of course there are multiple approaches to doing qualitative analysis, and each has their pluses and minuses.

If by *verstehen* you have in mind interpretive work, such as is practised in film studies for instance, I would draw a different conclusion: those scholars analyse a film for instance, and propose an interpretation more exclusively based on their reading a particular text. This is not the same kind of knowledge practice as engaging in systematic data analysis, where data itself puts limitations on what can and cannot be said. Along the same lines, there is a big difference between cultural studies and cultural sociology as well, to the extent that cultural sociology is an empirical project: it requires analysing systematically a body of evidence and drawing conclusions from it, and this is not the same as reading an author and giving your take on him/her. Both are interpretive activities, but of a very different kind, and they operate within very different types of constraints.

NM: *That is an interesting distinction between cultural studies and cultural sociology because historically in the British tradition, cultural studies were inherently empirical too.*

ML: Exactly, but in the United States, it became more of a humanities project, a way for English departments and French and German departments to broaden their audience, to talk to one another and to other theory minded scholars, and, indirectly, to improve their enrolment. In the 1990s, Romance language departments started saying, 'We are not only going to do French literature, we are going to do French cultural studies and talk about North African immigrants', so some of these fields ended up producing soft sociology. In sociology per se, while the work of Paul Willis, EP Thompson and others certainly had an impact, they were tied to Marxism, and to a somewhat repetitive focus on resistance as a response to a Gramscian concern with hegemony. The Birmingham school was not aiming to develop a *cumulative research agenda*. That is why I think the Birmingham school fell a little by the wayside in American sociology, though it became a classic point of reference.

To the extent that I understand it (and I am not that familiar with it), I believe that British sociology has more people who do exclusively theory work. For its part German sociology is somewhat polarised between those who do theory and those who do empirical (by which some mean quantitative) work. There seems to be an assumption that those who do quantitative analysis do not need theory. The configuration of how theory and empirical are articulated across the various national sociological traditions and disciplines appears to vary widely. In the French case, you often have a lot of theory and some French sociologists think a lot about the '*construction d'objet*' but they may not as concerned by research design and data analysis as American sociologists are. The relationship between the various tools across our trade is very different, not only across disciplinary traditions but also across national contexts.

Funding

This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.

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Date submitted March 2016
Date accepted April 2016