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A past chair of the Council for European Studies, Lamont will serve as the 108th President of the American Sociological Association. She was elected as a member of the Royal Society of Canada in 2015. She is also the author of *Money, Morals, and Manners: The Culture of the French and the American Upper-Middle Class* (University of Chicago Press, 1992), *The Dignity of Working Men: Morality and the Boundaries of Race, Class, and Immigration* (Harvard University Press, 2000) and *How Professors Think: Inside the Curious World of Academic Judgment* (Harvard University Press, 2009). Her books have won several awards including the 2002 C. Wright Mills Award from the Society for the Study of Social Problems for *The Dignity of Working Men*.

Lamont has also published over a hundred peer-reviewed articles and book chapters and has led multi-year collaborative projects that have resulted in collective books: *Rethinking Comparative Cultural Sociology: Repertoires of Evaluation in France in the United States* (with Laurent Thévenot, Cambridge University Press, 2000), *Successful Societies: How Institutions and Culture Affect Health* (with Peter A. Hall, Cambridge University Press, 2009), and *Social Resilience in the Neoliberal Era* (with Peter A. Hall, Cambridge University Press, 2013). Other edited publications include *Workshop on Interdisciplinary Standards for Systematic Qualitative Research* (with Patricia White, National Science Foundation, 2008.)

What advice do you have for younger scholars seeking funding for their research?

If you look at the chapter of *How Professors Think* that deals with criteria of evaluation, two of those criteria really stayed with me. The first is that clarity is absolutely essential. Typically, reviewers have to go through proposals back to back, one after another, and by the end of the day they are very tired. They often de-select more than they select. They are looking for reasons to eliminate proposals. For that reason, the proposal really has to be limpid. It has to have focus. You have to improve the proposal and clarify the central claims, often far more than

one would think. It is really important to stick to it and polish the proposal until it is perfect. That can be extremely time consuming. The more you do it, the better you get at it. One of the characteristics that separates winning proposals from others is the degree to which writers are willing to do the painful and not very interesting work of polishing the prose. Often people will state their argument twice. They state the argument, and then write "in other words..." That is because they are not satisfied with their original statement. They should go back and say it clearly the first time.

The second criteria is how well the applicant establishes the connection between theory and data. When we train graduate students it is important to explain that they may be interested in questions that may be impossible to answer because there is no appropriate data. People need to ask, for what is my data best? What can I show with it? The ability to make connections between theory and data is a skill that develops with time. In that also, you get better at it the more you do it. For accomplished researchers, making those connections is like a fish swimming in water. They are in their element. So younger researchers should take the time to attend a lot of talks by top researchers to see people deploying those skills. This will help them become more accurate and interesting in the way they talk about their own research.

Related to that is the importance of having intellectual range. You have to have access to a broad intellectual tool kit in order to imagine new questions. If you don't, you are handicapped. You may not be able to draw connections because you haven't read that much. That goes to theoretical literacy and disciplinary literacy. If you have that, your work is more likely to be original. The most productive people are in conversation with others in the field.

On the question of productivity, collaboration is very important. It gives you deadlines. Deadlines make you more productive. People often make the mistake of working alone. They lose momentum and get lost in their research. That is related to the quality of the proposal itself. If one of the criteria of evaluation is feasibility, which includes whether the applicant is qualified, having been productive in the past increases the likelihood that the project will be completed. It speaks to their preparation. If someone has never done an ambitious project before, they are often not viewed as capable of doing such a project in the future. Their vita gives evaluators an idea of what they are capable.

Another piece of advice I give younger scholars is try, try, try. People who don't apply don't get anything. To exclude yourself is not smart. But look closely at what the reviewers are saying when you get feedback. Be teachable. This is also a crucial asset.

You mentioned connecting theory with data a moment ago. That often trips up younger scholars. What advice do you have for them on that score?

In *How Professors Think*, I described this as an evanescent criterion. Reviewers often don't receive any direction in applying this standard and what it means varies enormously across the disciplines. There is a very good paper by Camic and Gross in the *Annual Review of Sociology* on eight different understandings of theory. Some are most relevant for the humanities, like critical theory à la Frankfurt School, which is very different from Mertonian middle range theory which is more often used in the social sciences. Many of the panelists in literary studies I talked with said there is too much theory used in literary criticism. But theory in that context means something very different from what it means in sociology. I think the question should be addressed differently for each field. For sociology, I like to say that researchers should draw on two sets of literature. One is the literature that gives them analytical tools and concepts — a conceptual frame such as cultural capital, boundaries, narrative, or social structure. The other literature forms the conversations to which you want to contribute, the questions others have addressed which you also want to consider. What causes inequality? This is an old question that many researchers are tackling. There are many competing hypotheses. I would say, for the kind of work I do, you want to have a theoretical focus that is very clear and very well informed. It cannot just be descriptive. That does not count as interesting. If you are looking at social processes, how are certain outcomes enabled by the environment? In my forthcoming work *Getting Respect: Dealing with Stigma and Discrimination in the United States, Brazil, and Israel*, our explanatory approach is configurational, i.e. about macro and meso level factors that enable and constrain patterns of responses. It is very different from variable based analysis that privileges replicability. A lot of literary critics are critical of too much theory. Theory is often mentioned just to position oneself and to signal where one belongs. Are you Foucauldian or Judith Butlerian? People say there is too much symbolic use of theory. But different disciplines require different approaches to theory.

Since readers of grant proposals are often not from the applicant's field, does that mean they have to translate theories from their discipline for outsiders?

One way to answer that is that historians win grants in larger proportion because they write much better. The work they do appeals more easily to interdisciplinary evaluators. They speak convincingly about the significance of the work. It is normally the job of the evaluator not to dismiss the work because they don't get it. People who serve on these committees have to have the intellectual breadth to understand proposals from multiple disciplinary perspectives. But not all competitions are of equal quality. When panelists use idiosyncratic criteria to evaluate proposals, you need a program officer or another panelist to stand up to them and say, "No, that is not a valid objection." But this does not always happen, of course.

I was very interested in your analysis of literary studies, and the difficulty literary scholars have in agreeing on criteria of evaluation. Given that, what do advise applicants from literary fields to do?

I think if you ask a literary scholar they would say that there are criteria. They would say those criteria would have to do with clarity and the quality of the writing. They would say those criteria have to do with the extent to which applicants are steeped in the relevant knowledge and have command of the crucial debates in the discipline, and the whole range of positions people are taking. Saying something original is crucial. They have at least four or five criteria. Now, how does that manifest itself concretely? There are variations. Two scholars would react to a given proposal very differently. My answer is that many practitioners who are very involved in the evaluation of literary criticism would believe there are criteria.

Do you think there are criteria?

Yes, I do. If you study a competition and several people make the same comments on a given proposal, there are criteria for the evaluation of literary criticism. If there is eighty percent convergence among the evaluators, there are criteria that they share. Other fields may have a convergence of only forty percent. Evaluative cultures vary a lot across field, as I argue in Chapter 2 of *How Professor Think*.

It sounds like convergence among reviewers is key to determining whether they are using valid criteria.

My position would be that there are no intrinsically valid criteria. Instead, we find subjective agreement on the importance of some criteria. The way it works is that evaluators typically rank the candidates before the meeting and the grant-making institution compiles the rankings and distributes them to the reviewers at the start of the face-to-face deliberations. It often happens that more than one reviewer will give candidates the same ranking. That convinces program officers and panelists that there is convergence. Once convergence happens, it convinces people that excellence actually exists. Then there is a tendency to downplay differences.

I was struck by the consensus-driven quality of these decisions. That is not the case with other decisions in academia, like those over tenure and promotion, for instance.

Tenure is different. There are considerations that do not pertain to the quality of the work. Tenure can alter the balance of power and the relative salience of areas of specialization within the department. If you give tenure to a Europeanist, what does that mean for the Latin American program? There are competitions for hegemony within each department and each tenure decision influences that. If it is an ecology where people make many decisions together, each decision is not independent from the others. Some people may feel they are always on the losing side, and that affects their ability to make arguments that can persuade their colleagues. People always say that with tenure, the good cases are easy and the bad cases are easy, and the cases in the middle are hard. Cases in which competing criteria are used are the most difficult. You get different evaluations made of the same product depending on the criteria that are used.

I was also struck by your critique of Bourdieu, and your argument that decisions about grants are made not to further an agenda or consolidate power, but out of civic-mindedness.

The kinds of organizations I study are very prestigious – the American Council for Learned Societies, for instance. Of course, people are happy to serve as panelists on their competition because it gives them a chance to influence the distribution of resources. But they also agree to serve because they are honored to be asked or because they have received funding and want to give back to funders that have supported their work in the past. That is very different from serving on a promotion committee, which is more of a service to your department.

Have the priorities of grant making institutions changed over time, or has the pursuit of excellence remained something of a fixed star for them?

Disciplines diffuse different visions of excellence. In disciplines where Foucault has been influential, scholars think about excellence as something that is constructed and institutionalized, an epistemic discourse of sort. In contrast, in economics, though they are familiar with the Kuhnian notion of the paradigm shift, they tend to essentialize excellence. People at the top of their fields tend to think there is something objective about excellence. Part of that is age and generational culture. In more scientific fields they are more likely to think there are objective standards of excellence. In fields where postmodernism is influential, that is not the case.

There are also differences between the way public and private institutions operate. At The University of Texas at Austin where I taught at the start of my career, the university was much more proactive in overseeing everything than Harvard University is. At the University of California, Los Angeles, the university is very much in the service of California. That drives many of the choices the administration makes. That is less of a concern at private institutions.

Across the academy, diversity is more salient now than it was twenty years ago. Also, all data show that academics are working longer hours than they ever did before. They are all overburdened. Our lives are very different from that of the gentleman academic of long ago. Work conditions are different. Even the social origins of faculty members are different. There are more dual career families now. I remember that when I arrived at Princeton University in 1987, I was told that social life among the faculty had been transformed drastically after faculty spouses started working. There were far fewer dinner parties, because both husbands and wives were working. It was a huge change in what it meant to be a faculty member. A world of genteel cocktail parties is less likely when you were raising toddlers and you have two young professionals working fifty hours a week.

One person I spoke with said he thought the consensus-driven nature of the process made it difficult for truly original work to get funded. He argued that,

in political science, for instance, the triumph of rational choice has been so thorough that it has become hegemonic and excludes alternative approaches.

I am not sure I agree. Rational choice is much less hegemonic now than it was fifteen years ago. Many would say that paradigm is losing speed, with the cognitive revolution. Behavioral economics is destroying many of the assumptions on which economics was based. That landscape is very much changed. As the economy itself has changed, macroeconomic theory and labor economics have changed with it. Political science is also going through a phase where everyone is concerned with counterfactual and value controlled experiments. But new books, like *Advances in Comparative-Historical Analysis* by James Mahoney and Kathleen Thelen have come out that are critical of that and argue that this trend toward methodological purity is leading to a narrowing and impoverishment of the field. So, from where I sit, the state of these fields seems more dynamic. True, there are committees where you find different trends, perhaps at the National Science Foundation (NSF). I led a committee for the NSF on evaluating the quality of research in the social sciences (Lamont and White 2008). Consistent with the influence of King, Keohane, and Verba's *Designing Social Inquiry*, some political scientists argued that qualitative research in their field should match the standards to which quantitative research is held. They measured qualitative research by standards applied to quantitative research. I don't think this is wise. If replicability is given more weight as a criteria, you are setting up qualitative research for a losing battle. You have to play on an even field and consider under what light qualitative research shines or for what it is uniquely good. This subordination of qualitative to quantitative research in political science has led some political scientists to use sociology as a new point of reference. I always put my money on the fact that people get tired of closing off options. Young people will always want to make their reputation by trying out some new approach or set of questions.

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