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Re-embedding pragmatism

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Re-embedding pragmatism

by *Éric Brian*

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Michèle Lamont's *How Professors Think* will probably meet a two-fold audience: the domain of sciences studies and the sociological discipline. In order to limit misunderstandings I should clearly make a distinction between two ideal types. On the one hand, there is a domain defined by its object of study (i. e. sciences) whatever the background references, the concrete methods, the explicit and the implicit interlocutors shared by specialists. On the other hand, there is a discipline (i. e. general sociology) where references, backgrounds and interlocutors are in principle under the scrutiny of the specialists who claim for this specialty whatever their topic. For academic reasons made of claims for institutional legitimacy and access control, domains generally display certain trends towards disciplinary uses and practices. Still, expanding disciplines usually break down in more specialised disciplinary domains. Merton was a general sociologist who studied scientific institutions as case studies. He published *The Sociology of Science* in 1957. Twenty five years later in France, Bourdieu was in a similar position. He published *Homo academicus* in 1984. Another twenty five years and Lamont places herself in a very explicit manner in the continuation of this genealogy.

I will comment her book from this disciplinary standpoint I share. I guess that others could do it from a "sciences studies" perspective. Before doing so I have to add that moving from a "domain" to a "discipline" and back is not so simple. From the standpoint of general sociology the concept an author discusses should work at the level of its syntax, dealing with "sciences" just like with any other domain.

It is at a semantic level that a sociological concept has to fit or not with specific domains. “Anomy,” “autonomy,” “beliefs,” “professions,” “frames,” “interactions,” “domination,” “field,” “habitus,” “action,” etc. as sociological concepts are to be understandable as such, and to be improved with the empirical matter today provided by cultural studies, sciences studies, gender studies, social studies of finance, media studies, etc. like earlier by sociology of religion, of education, of medicine, etc.

There is in addition a noticeable syntactic property in some sociological concepts among those provided in the literature. Their general logic may give account for what is going on inside and for the relationships with the outside, what actors usually express referring to essential specificities. For instance, the concept of “relative autonomy” facilitates the understanding of tensions governing a domain (the internal distinctiveness, *e. g.*: “the best,” “the second best,” and “the others,” or “hard sciences” *vs.* “studies”) and it gives account in a similar manner for the representations shared among actors (their more or less arguable “identity,” *e. g.*: “science” *vs.* “culture,” or “science” *vs.* “politics”). As opposed to overgeneralisation, there are several benefits of such powerful concepts: the understanding of how actors claim for “art for art” or for “pure science” in homologous terms without presupposing that “art” and “science” are more or less “the same”; the analysis of peculiar theoretical effects like confusing “culture” and “science”; a comparative and differentialist attitude in front of underlying assumptions carried by “scientific” or “cultural” fetishisms.¹

Now let’s move to “science studies.” Most of the authors dealing with the domain do not seriously consider strict sociological issues, especially the one I just mentioned. Because they are predominantly topic-oriented, “social studies of sciences” miss the target of innovative sociological research.² Here at the contrary, Lamont does spend the two first chapters arguing efficiently her focus on peer review and panel technology. But doing so, she hardly corrects the ambiguities in the book title.³ If the author pretends that she has exhausted “professor thinking,” I am sorry to say that I have to disagree. Earlier studies have shown that scientific collective decisions (the very topic of the book) do not display the same logic at various scales – in the conduct of a small experiment, in a panel decision (the strata Lamont has studied), and in big

¹ Bourdieu’s tribute to Merton explicitly refers to this property. See: Bourdieu [1990] and Bourdieu [1991].

² David Bloor has met this kind of difficulties in *Knowledge and Social Imagery* [Bloor 1976] and, as a general statement, it may be said that the polysemy of the word “social” is the weakness of the social studies of sciences.

³ Before blaming the author, one may consider that the title may be due to the publisher. There have been a *How Lawyers Think* [Morris 1937]; more recently a *How Doctors Think* [Groopman 2007] and a *How Mathematicians Think* [Byers 2007].

infrastructures like international observatories or large colliders.⁴ Meanwhile scaling sociological objects has become a relevant issue in general sociology and especially in the sociology of professions, a domain Merton was concerned with [Abbott 2001]. Last but not least, there are many other frames for professors' thinking, first and foremost their conceptual work which is not mirrored at its best in a panel context.⁵ It should be added here that Bourdieu in *Homo academicus* – following some of Max Weber's classical arguments about science – has devoted pages to the analysis of the polarisation of scientific fields between two modes of scientific domination: temporal powers made of bureaucratic business as opposed to more autonomous productions generally conceived as a form of spiritual power. Basing her study on panels, Lamont takes the risk of overestimating the legitimacy of the bureaucratic regulation of science, sharing a functionalist mood inherited from Parsons and Merton. In other words, institutional regulation and the consolidation of relative autonomy are not to be confused.

The very topic of the book is *homo academicus suffragans* (this is not the title of a best-seller, I agree): how professors elaborate in the social frames provided by panels their opinions about their peers and how they arrive to collective decisions for evaluating them. Given the systematic use today of this kind of procedures based on deliberations in a group of experts for scientific ranking, evaluation and regulation, the study is of the greatest interest not only for sociologists but for scientists in general, for scientific policy makers and for their clerks. The book is well written and explicit in many ways. Sometimes the arguments are short and could profit from further discussions [p. 10 for instance or p. 255-256 of the Appendix dealing with data analysis], but all readers will find matter for their own reflexion. The empirical base is the activity of panelists in the peer review system at the US national level. It is an analysis of the panel organisations and mechanisms with special attention given to a set of around eighty interviews of panelists carried along by the author who could have appeared at the same time as an observer and as an insider. The range of sociological arguments runs from structural-functionalism to symbolic interactionism, from American classics (Merton, Whitley, Garfinkel, Goffman) to French social theory (Bourdieu, Latour, Boltanski, Thévenot). Lamont proclaims herself an

⁴ Lamont does comment on Latour and Woolgar's *Laboratory Life* [Latour and Woolgar 1979] but does not deal with decisions in "big science." See for instance Leach [1973]; Darmon and Lemaine [1982]; Pestre [1988].

⁵ Maurice Halbwachs analysing in the 1940s the abstraction of space carried by the collective memory of mathematicians has provided an emblematic example of similar frames involved in "pure" scientific thinking.

interactionist and pragmatist even if – as I have mentioned above – she is sharing functionalist presuppositions.

Lamont is right writing it is very rare for a scholar to “provide systematic bases on which to ground a comparison of disciplines” [p. 19]. Her book offers here a homogenous empirical base (although it deals with the US context where the issue of “English Literature” is at stake for instance). This is precious. Lamont does not underestimate the peculiarity of this national context and explicitly argues that in USA a panel system could regulate the academic world given its size, when in smaller countries (which means almost everywhere else) similar regulation could be irrelevant [p. 5]. I agree observing here that at the smaller scale of European countries – France for instance – a peer review system could become perverse, turning in a social automaton that produces highly predictable bureaucratic decisions leaving no room for a collective reflection on scientific strategy. Another perversion of the peer review system could appear inside small research specialities – for instance in journal management – when a formal satisfaction of peer review evaluation could become an instrument for closing a narrow specialty from the rest of the academic life. Therefore the peer review system is a specific political management tool for the regulation of resources in the scientific world, not a “good practice” warrant of scientific relevance or autonomy. The book will help its readers to understand it from inside.

Among Lamont’s results, let me point out the fact that all disciplines are not equal in front of a panel system, some of them – Lamont mentions History and Economy⁶ – fitting more easily in a system of consensual judgement. Reading Lamont, I get the impression that the trial of a consensus among experts reveals in fact a high degree of normality in a scientific specialty – normality as understood by Thomas Kuhn. I have difficulties when Lamont diagnoses crisis or mutations in unhappier disciplines (such as English Literature or Political science). Indeed I am unable to distinguish in her analysis the representation of the disciplines produced by the panels and an objectivist comparative analysis of the state of the same disciplines obtained by other means. In addition it seems to me difficult to argue that a consensual discipline should be the norm. Science – as opposed to academic scholasticism – is not about consensus. Part of a collapsing specialty transferring its capital to another could be really innovative. Most of scientific innovations come from painful circulations and transfers. One may consider it to be the duty of those involved in science management and regulation to encourage this kind of dynamics.

⁶ I understand the idea of a *de facto* differential treatment of various disciplines, but about Economy I feel perplex. At least, this is based on pre-2008 observations.

One of the most striking elements Lamont brought to her readers is her analysis of the uses among panelists of the keywords “excellence” and “diversity” [chapter 5 and 6]. Up to a quite recent past in the Western world, the public financing of research in social sciences and humanities was basically made of budgets put on the table by more or less public authorities and distributed by means of self regulation among scientists for the best or for the worst. Since the 1980s this system has been under the pressures of budget cuts and of rationalisations of public expenses. But if the 1980s-1990s have been characterised by the redefinition of the relationship between public and private support with a trend of subordination of the public action to the private sector, a recent and rapid transformation of scientific public management is ongoing: its characteristic is a complete revision of the public intervention. During the last few years, these budget pressures became drastic because of the level of public deficits, even if the governments are supposed to be aware of the necessity of maintaining a level of public R&D support, putting more emphasis on the most immediate economic impacts and sometimes on a few commitments for culture and higher education. This is the context of the emergence of benchmarking in the management of science, coming with the key words “ranking,” “diversity,” “excellence.” Public frameworks are quickly evolving, at least in Europe and Japan. Meanwhile public authorities pretend to renounce to scientific policy making, yet keeping an eye on a few economic strategic sanctuaries. The academic world is left to a newly organised competition for resources. Lamont writes that manifestations of this process happened to be conceived as “social Darwinism” [p. 204]. However, policy makers, evaluators and regulators pretend they will only check benchmarks as notations and ranking.⁷

Lamont shows that the words “excellence” and “diversity” are at least polysemic if not ambiguous. There is no consensus on “excellence.” And with “diversity” panelists do not refer to the general political meaning of the word, as for instance something like a positive discrimination at the benefit of “minorities.” In the general political context I have mentioned above, “excellence” and “diversity” are becoming more and more frequent in the discourses held in the scientific instances of regulation. They indeed softly refer to a crude arbitrage. The academic world is one of collectively accepted inequalities, there are two ways to deal with it: entertaining a

⁷ The long running history of scientific evaluation has been already written, see Godin 2005. The historical irony here lays in the fact that the financial business, where notations and ranking flourished for hypothetical reason of “market informational efficiency”, met recently a serious crisis as evaluation criteria. It suggests not only that today the academic world is ruled with cosmetic norms inspired by financial accountancy, but that these norms are under scrutiny (if not obsolete) in the financial world itself. Is this not a relevant illustration of the objective social authority of science and universities today?

Paretian distribution of resources where a few places get the larger amount of supports (this is the policy of “excellence”) or instead compensating some of the well known inequalities (this is the policy of “diversity”). Lamont has clearly shown that the very same words “excellence” and “diversity,” coming from political discourse and carrying strong political assumptions on scientific public founding, are nevertheless redefined in the panelists speech who invest in them deferent meanings intensively attached to the peculiarity of the American academic world. Considering that the panels are the keystone of the national institutions, and that these institutions are the interface between the political world and the academic world, it seems that these two words are carrying at the same time the public financial pressure on sciences and the collective response of the academic milieu trying to define its own autonomous rules of government.

Now let us move to the core theoretical argument of the book: “pragmatic fairness” [chapter 4]. Lamont places herself, as I said, after Merton and Bourdieu. On the one hand she is extending the analytical spectrum opened by Merton with his classical analysis of the “Matthew Effect in Science” (1968) that could be rephrase as a traditional French proverb: “*on ne prête qu’aux riches*” (something like “the more you have the more you get”). On the other hand she proposes after Bourdieu and some of his critics a contribution to the sociology of action. This is noteworthy. But I am upset reading in such a book a so simplistic account of Bourdieu’s conceptual framework: “Bourdieu argues that the judgement of scholars reflects and serves their position in their academic field” [p. 20].⁸ But for the sake of catching Lamont’s actual contribution, let’s move on. She records counter-proposals from Latour, Boltanski, and Thévenot. These authors challenged Bourdieu’s theory of action with arguments based on actantial models (Latour, after Greimas), on forms of justification (Boltanski and Thévenot), on “pragmatic regimes of engagement with the world” (Thévenot).

Bourdieu has built his sociological theorization trying to catch the action at the very point where forms of the social history meet: one carried by the actor himself (the “habitus”), the other displayed by the rest of the world (the “field”). Doing so

⁸ In Lamont’s defense, I have to mention that I have encountered before and abroad similar fables, like “at the origin was a crypto marxist called Bourdieu, than came sophisticated French pragmatists...” Just like Durkheim in his times, Bourdieu has spent many pages to ridicule this kind of narrow minded understanding. The problem with this kind of short circuits is not only that they are unfounded but that they hide Bourdieu’s place in a sociological and philosophical trajectory which run from Durkheim and his immediate followers to Lévy-Strauss, Merleau-Ponty or Sartre; the French sociological scene during the 1970s and 1980s with people like Touraine or Boudon; and a variety of developments – pro, around and against Bourdieu’s works – in the French sociological scene and its connections to other social sciences during the 1990s and the 2000s. It is a pity but I’m afraid this is the counterpart of the academic star system under recent globalization. But books are circulating and the scientific work goes on.

he proposed a theory of “habitus” which could be criticised as a little simple, and a theory of social structures which could appear a little over-dimensioned.⁹ Boltanski and Thévenot have rejected elements used in Bourdieu’s reasoning. They deal with the qualifications of the actor and of the conditions of the action. Doing so, they have proposed to refer the justification of action to a set of ideal narrative regimes.¹⁰ For me, for instance, the problem was not so much the attention given to action, but the fact that building these regimes, Boltanski and Thévenot were abandoning any serious attention to the historicity of action and to the formation of its conditions of possibility.

Lamont scrutinized her eighty-one interviews. She suspended the idealistic philosophy of action presupposed in the architecture of Boltanski and Thévenot’s various sets of regimes and focussed on the panelists’ discourse. Doing so, she has been able to reembed – to reimplant – these speeches in their concrete institutional and practical conditions. Who are the panelists? Where and how do they speak? How do they deal with the various resources they mention? That fits with usual business in ethnography and interactionist sociology, isn’t it? This is a real result, we have now in the sociological toolbox a structural theory of action (Bourdieu), a reticular theory of action (Latour), a taxonomic theory of action (Boltanski or Thévenot), and a reembedded theory of action (Lamont). It accounts for what Lamont has called “pragmatic fairness” in order to characterise the form of collective decision drawn in the scientific panels she studied. She places her realistic contribution under the auspices of Goffman (I take it) and Garfinkel (I am more sceptical). I understand her points, but I do not feel necessary to push to much the conclusion in the direction of ethnomethodology, because it may encourage an exclusive subjectivist interpretation which is not so evident.¹¹ However, this is an actual achievement, and the general sociologist dealing with action will have to explore here a new frontline.

Lamont is thus contributing to the sociology of action. She is referring to Rational Choice observing through together with her panelists its ascent in political sciences and “the concomitant hegemony of formal theory and methodology” [p. 95 ff.]. But RCT was obviously not among her theoretical options. Indeed translating actions in equations has been under Bourdieu’s attacks from his early works on Algeria to the latest. Lamont – just like Latour, Boltanski or Thévenot – clearly owes a debt to Bourdieu she underestimates. And ungratefully forgetting the epistemological dimension

⁹ This is not the place to explain why and how he did that, and how his arguments have evolved from one publication to the other.

¹⁰ Here again, it is an outline: there have been various formulations of this argument.

¹¹ Here we are at the border of social phenomenology and it’s a perilous zone given the effective genealogies that could be traced between the various authors concerned in France, Germany and USA since the 1930s.

of Bourdieu's sociology, they are now helpless. It seems it is time to reevaluate this epistemological background and its relationship with historicisation. The irony is for instance that the current specialists of "Rational Choice Theory" consider that their ancient founder is the Eighteenth century mathematician Condorcet (1743-1794), as their modern founder at the Twentieth century is Kenneth J. Arrow. I have no special information on Arrow and panels, but I must mention that Condorcet's well known and rarely read *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (1785) was a study carrying mathematical innovations together with systematic concrete considerations on the conditions for the expression of collective opinion. The same issues emerged more than two centuries ago... Who are the panelists? What kind of relationship do they entertain? How do they confront their opinions? Are there better forms of deliberation and votes? How to deal with heterogeneous panels? And so on. An interesting point here, is that the backstage of this book is, on the one hand, the formation of local assemblies in pre-Revolutionary France in order to regulate and manage at various levels the resources in a general context of growing financial crisis and, on the other hand, the experience of collective decisions in various academic institutions in the Eighteenth century. In addition, Condorcet, at least in the French speaking scientific tradition, has brought these results to a wider scientific audience and coined this idea that a independent scientific society in a post-Revolutionary era must be ruled by means of panels and votes.¹² So there is a history of the theorisation and of the institutionalisation of "pragmatic fairness." Seriously considering it seems to me the next response to amnesic and abusive uses of formalisation in social sciences. Such a line of reasoning may be pushed up to current social sciences. This means attention paid for epistemology, for history of "social mathematics," for the sociology of collective memory, but it is not the place here to develop this line of conduct.¹³

Let's come back to the predictable receptions of *How Professors think*. Here, the sociologist faces a similar dilemma to the one a Nineteenth century biologist could have experienced after having shown that the human body is full of bacteria to an audience of doctors and profanes who had some other understanding of these things. He (or she) indicates a newly observed necessity challenging thoughts shaped by earlier scientific forms of reasoning. He (or she) is therefore at the crossroads. Should he (or she) exploit profane's credulity and of less informed therapists' weaknesses? In other words, should he (or she) exploit sociological disenchantment about scientific

¹² For more on this topic see Brian [1994] or Christin [2001], two works by authors related to Bourdieu and informed about the sociological debates on the theory of action.

¹³ To go further see Brian [2009]; Brian and Jaisson [2007]; Halbwachs [2008].

disenchantment, and turn himself (or herself) into a disillusionment priest – even flirting with charlatanry? Or, on the other hand, should he (or she) give up easy victories, acknowledging that science is done like this for the worst... as for the best? Biology at the end of the Nineteenth century choose the second way. But science studies, since the 1990s, hardly struggle to break away from the first. They dwell so close to the sciences – pushing them away in one move, and reaching out to them in the next – in a few words, cultivating a lack of sociological objectivisation.

The merit in *How Professors think* is to anchor the survey in the very concrete and effective conditions of the evaluation work and in the stakes of general sociology. Doing so, Lamont surprisingly joins matters and reflections discussed more than two centuries ago. Therefore, her book should not drive its reader towards scientific skepticism, but towards the critical discussion of formal modelisations of “rational choice,” providing empirical documentation together with relevant analytical proposals. In addition, through this historical parallel, the book suggests that if one must consider the necessary illusion of the normativity in peer review for instance, it is in order to conclude that science is a human activity just like any other, characterised by its orientations towards specific goals (*i. e.* searching elements of truth by empirical and reasoned means), its specific practices, uses and regulations accordingly shaped and revised all along its history – a statement that Merton and Bourdieu could have shared. Is this not a line of reasoning that a philosopher of the Enlightenment could have also shared? At least, would he have identified in the simplistic cynicism that today usually comes with the reception of sociological works dealing with science the marks of superstition, sophism and obscurantism... The relevance of Lamont’s book may allow its readers – those aware of the long running formation of scientific institutions – to share such a diagnose.

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Re-embedding pragmatism

Abstract: The article discusses Michèle Lamont's *How Professors Think* (2009) from the standpoint of general sociology and its potential reception. Her contribution to pragmatic sociology is compared with earlier authors. The confrontation with social choice theories is sketched.

Keywords: Pragmatism, peer review, scientific regulation, benchmarks.

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