Response: Inside the Sausage Factory

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Sociologica could not have chosen a better international set of social scientists to discuss my book *How Professors Think: Inside the Curious World of Academic Judgment* (Harvard University Press, 2009). Considered together, they represent the widest range of positions one could have expected from an interdisciplinary audience of experts on evaluation: while Eric Brian writes from the perspective of a theoretically sophisticated and "Bourdieu-informed" historical approach and Flaminio Squazzoni from that of a game theorist, Harry Collins provides a "science studies" take on my book while David Inglis takes the viewpoint of the journal editor that he is to add a thoughtful self-reflective voice to the exchange. Finally, in Johannes Angermuller's text, one finds a self-described post-structuralist reaction.

I thank these colleagues, as well as the editors for participating in or engineering this exchange. Revisiting *How Professors Think* more than a year after its publication is a wonderful pretext to reflect on the reception of the book, as well as on the conditions of its fabrication (therefore the title of this response).

The essay by French historical sociologist of science Eric Brian goes the furthest in locating my contribution in a broad historical and meta-theoretical sweep. First, he observes that I approach peer review as a historically situated form of evaluation, connecting my writings to those of Eighteenth century mathematician Condorcet who proposed that independent scientific societies must be ruled by means of panels and votes and who celebrated the expression of collective opinion, against the background of local assemblées in pre-revolutionary France. Second, Brian's close reading allows him to identify nuances in my theoretical positioning that have gone unnoticed by other critics. More specifically, he contrasts the meta-theoretical model underpinning my analysis with some of the general theoretical frameworks in contemporary social sciences, including the critiques of Bourdieu developed by Boltanski and Thevenot, which lead to a taxonomy of forms of justification. He suggests that my approach examines modes of evaluation in concrete institutional and practical conditions, being more grounded in interaction. He also notes (to quote): "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 and Thevenot), and a rembedded theory of action (Lamont)." He connects my book to longer traditions of discourse on science and skepticism, to argue against a science studies that justifies skepticism toward science by an understanding of its conditions of production. In contrasts, he advocates (as I do) using this understanding to create more informed knowledge production embedded in less naïve epistemological cultures.

I particularly appreciated Brians' remarks on the peculiarities of the peer review system in the United States where the large size of the scientific field may make peer review a particularly appropriate technology for the distribution of rewards than it may be in smaller academic fields, such as France, where anonymity and the independence of evaluators is less easily achieved. He also rightfully points out that consensus should not be privileged over conflicts as an engine for the development of science. However, my depiction of various disciplines (philosophy, English literature, history, anthropology, political science, and economics) does not take the more consensual disciplines (history and economics) as point of reference for the others (as he suggests). If anything, my analysis attempts to make sense of various disciplinary evaluative cultures on their own terms.

On a more critical note, Brian also argues that by studying evaluation through the prism of peer review, I overestimate the importance of institutional regulation in relation to the autonomy of symbolic fields. *Contra* Bourdieu, I do not believe that in a field as large (demographically and otherwise) and institutionalized as American higher education, it makes sense to posit the relative autonomy of symbolic fields. In this universe, independent producers who escape the disciplining powers of peer review are almost non-existent. The principles of structuration of intellectual fields that Bourdieu found in the French context should also be considered as historically contingent.

A final, rather minor, point: Brian rightfully points out that my book does not exhaust "How Professors Think." This title emerged after Harvard University Press told me that my original title, *Cream Rising*, was too "insiderish." Among those who have reviewed the book, several have taken issue with this title, and I share their concern. Never again will I take advice from a marketing department when it comes to choosing a book title. It is a sad fact that the author does not have full control inside the sausage factory!

The most ambitious of the five sets of comments I have received came from Italian Flaminio Squazzoni, who seized the opportunity to respond to my book to present results from an experiment on the social mechanisms behind peer review, using the tools of game theory. Instead of a short commentary, he sent in a seventeen page paper, accompanied by a three-page list of references, which demonstrates his familiarity with the literature on peer review in the sciences. He rightfully situates peer review within a broader context of collective decision-making and evaluation, which includes the evaluation of start-ups in innovative markets. He suggests that such cases are fruitful puzzles from the perspective of rational choice and collective action. While I am honored by the care he put in preparing his response, I nevertheless recognize that his approach to social norms is incompatible with mine for several reasons. First, he starts with a game-theoretical framework as opposed to an empirical study of the interactions in which evaluation takes place. He focuses on social norms whose relevance are posited, as well as on social sanctions, as core social mechanisms of peer review. Thus he ignores an important and growing literature on social mechanisms that is not rational choice in inspiration, and which creates important bridges toward cultural sociology and pragmatism [see Gross 2009]. Secondly, he dubs his focus on incentives "sociological" and opposes it to my "behind the scene" perspective on peer review. He also argues that my cultural approach makes sociology "marginal" to current debates. This statement not only reveals the distance between the epistemological and intellectual spaces we both occupy, but is also contradicted by the fact that my book is being read and discuss across a wide range of social sciences and humanities disciplines (as reflected in website coverage for instance). Finally, he describes my focus on the social sciences and the humanities as a limitation, as opposed to an intentional choice to illuminate areas of peer review that have been largely neglected.[1] Squazzoni's own expertise lays in the more scientific fields, where he argues "there is a close analogy between the logics of market and the logic of science." In my view, it is more interesting to consider the extent to which peer review operates as a market across clusters of fields, than to posit that it does for the sake of theoretical elegance (mistaking beauty for truth, as Paul Krugman famously put it!)[2] Processes of arbitration and consolidation of scientific value may

[1] On the need for a "social science studies" as a complement to "science studies," see Camic, Gross, and Lamont [forth.]

[2] http://www.nytimes.com/2009/09/06/magazine/06Economic-t.html

be structured differently across types of disciplines, depending on how black-boxing (to borrow a Latourian concept) operates across fields. The mode and degree of cultural embededness of value across various types of scientific markets is an empirical issue that is worth further investigation – just as it would be worthwhile to consider more systematically processes of valuation of research output in parallel with valuation processes in other areas of human activity. Finally, it would also be interesting to parcel out the features of the sausage factory in which Squazzoni operates (where it make sense to ponder about "investment games," "cooperation problems," and "cheating strategies"). I will resist the urge and simply call for more explicit debates around the assumptions central to our respective epistemic and evaluative cultures.

In a very different perspective, in his short essay Harry Collins connects *How Professors Think* with his own writings on interactional expertise [Collins and Evans 2007]. He suggests that the type of expertise that is needed to serve as an evaluator on an interdisciplinary panel is a form of interactional expertise. I am not sure it is, or that this type of knowledge is similar to the forms of interactional expertise Collins and Evans discuss in their book, which are more oriented toward practical knowledge. Social scientists and humanists are all writers and scholars and evaluate proposals based on the display of skills that they themselves master (as writers, researchers, etc.) Collins should ask where does interactional expertise begins and ends. If the concept is to be applied to too many forms of knowledge that is not expert knowledge, it will lose its power altogether.

David Inglis provides a particularly insightful reflexive reaction to *How Professors Think*, using my book as a means to reflect on his own work as co-editor of the journal *Cultural Sociology*. This experience inspires his remark that like the two faces of Janus, the gate-keeping function of peer review often coexists with a desire to do "the fair thing." But his analysis is more complex than that: He compares my argument with Bourdieu's writings on the *illusio* that leads the dominant to construct their judgments as universalist, and the dominated to blame themselves for their failures. Attributing me a position not unlike that of Merton on scientific norms, he writes: "The Lamont reading holds that the players […] have a commitment to the general ideas of fairness, balance, diligent, and so on" which would be "productive of something that resembles fairness." In fact, I advocate for a third, less straightforward position: I state that academics serving on panels distributing resources in nation-wide American competitions often read the fact of being invited to serve as indicative of their high professional standing and of their good reputation as fair-minded scholars. Focusing on their self-concept (a dimension ignored by Bourdieu), I argue that many hope to leave the deliberations with these aspects of their public self enhanced. I do not conclude that the system works, but show that the vast majority of evaluators perceive it as working and that they say they follow customary rules that they perceive as sustaining this fairness: deferring to expertise, demonstrating methodological pluralism, favoring cognitive contextualization (the use of criteria relevant for the discipline of the applicant), etc. As explained in *How Professors Think*, I am agnostic about the fairness of the peer review system as a whole, but concede that we can make judgments concerning the fairness of particular decisions. My explicit intent was less to evaluate this system than to make sense of how it works by focusing on how different proposals are made commensurate, how evaluators frame their idiosyncratic tastes, how they understand the role of horse trading and low-balling in deliberation, etc. I believe their representations of how the system works and what they do to make it work has performativity effects that do make the system fairer. Contrary to what some critics have argued, I do not believe that I let the evaluators off the hook by believing what they tell me about their own practice (I am not so naïve!). Instead, I do take their representations for what they are: social facts worth accounting for. Thus, Inglis is correct when he writes that I am concerned with intersubjectively-held assumptions. Finally, he insightfully comments that any academic reading my book will process through the lenses of his/her own success/failures with peer review and of their own sense of how fairly they have been treated by the academic system.

Inglis' reactions to my book are very much in line with my intent: to have academic evaluators reflect on what they do when they evaluate, with the goal of moving us toward a system where clientelism and idiosyncratic taste have less weight (including higher education systems such as that of Italy where an important generational divide overlaps with different types of commitment to universalist evaluation practices). I also hope to help direct the conversation beyond a naïve opposition between "biased" and "objective/rational" judgments that would presume that evaluation can be disembedded – but this position again should not lead us to simple scientific skepticism. In this era of higher education cut-back and governmental doubts about the ability of academics to self-regulate, the importance of self-disciplining and of fine-tuning modes of evaluation and our understanding of their conditions of possibility, is greater than ever. In this context, I am particularly grateful that professor Inglis picked up on the anti-skepticism agenda of *How Professors Think*, as I believe a much broader debate is needed around this question, with a focus on the specific configurations of challenges that peer review meets not only in North America, but also in various countries in Europe.[3]

The last comment is provided by Johannes Angermüller. I have kept it for the end because this is the comment that leaves me most puzzled, particularly because

[3] For an analysis of the French case, see http://www.sauvonsluniversite.com/spip.php?article3212

it states that I share "my interviewees' firm and unshakeable belief in the primacy of excellence." *How Professor Think* is an analysis of the multiple forms that excellence takes across the social sciences and the humanities. It shows that excellence does not exist in and of itself, but is instead a collective accomplishment. More concretely, excellence does not reside in the fellowship proposal being evaluated, but in the interaction between the readable features of this object and their assessment by evaluators who produce this value, or "actualize" or "recognize" it. They do so by voicing a framework of interpretation of these features, which framework aims to convince other panelists of the presence or absence of excellence. I also argue that the actions and arguments of evaluators are constrained by customary rules that are part of the culture of evaluation of the context in which they operate (which varies cross-nationally and otherwise). Thus, the point of the book is precisely that "excellence is never achieved through the intrinsic force of an idea: the construction of excellence is a practical achievement." Yet, this is the perspective that Angermüller contrasts with mine, while emphasizing the role played by "a web of knowledge and power" is in the process.

After reading this commentator's remark, I am left wondering exactly how he would distinguish the post-structuralist analysis of peer review he advocates from my embedded pragmatism. Also, I am puzzled about how a focus on power/knowledge translates into a methodological approach to the topic and into a sociological construction of the object. Finally, I am also unclear whether it makes sense to distinguish between hierarchalizing and tribalizing evaluative practices (or horizontal and vertical differentiation – dimensions that echo the writings of Niklas Luhmann or those of Peter Blau). In the available literature on boundary processes, as in my previous analysis on boundary work across a range of sociological objects, I have found that differentiation often goes hand in hand with the hierarchalization of categories. In light of this observation, I am curious about how Professor Angermuller would situate his contribution to evaluation in the broader sociological tradition and how he argues about his distinctive "added value."

At the risk of infinite regress (aka the "corn flake box" effect), my response to these five authors could have taken the form of a meta-analysis of the evaluative cultures we respectively inhabit. I resisted this more narcissistic approach for the following reason: at the end of the day I feel that peer review is important because it sustains the ability of social scientists to provide conceptual frameworks for, and feed, major societal debates about where we are going and what matters collectively. Whether social problems are framed in terms of individual failures or holes in institutional safety nets matter concretely to the lives of citizens, and to the policies that concern them. And our ability to self-regulate and maintain authority matters for how we are heard by governments, social movements, the media, NGOs, and other significant social actors. Thus, peer review is far more than the product of American imperialism, the perverse brainchild of new public management, or the growing and menacing shadow of neo-liberalism – as it is at times construed by European academics. It is the condition for our social efficacy as experts and actors in social life. And this matters, independently of our own fate as a more or less privileged, somewhat self-serving, and inner-looking professional corporation.

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Abstract: In this response to comments on her book *How Professors Think*, Lamont discusses several points raised by discussants. She also contrasts their respective perspectives and the complementarity of their viewpoints. She identifies the questions they leave open, possible ambiguities in interpretation, as well as topics for future research.

Keywords: Peer review, evaluation, excellence, academics, higher educaton.

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