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Afterword to the Special Issue on Knowledge in Practice

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I was very glad to say yes when Claudio Benzecry and Monika Krause asked if I would contribute an afterword to this special issue of *Qualitative Sociology*. I was not one of the discussants at the 2009 Junior Theorists Symposium (JTS) where the papers published here were originally presented. But I was in the audience, and was impressed then, just as I am now, by the range, intellectual vigor, and downright interestingness of the pieces. Young scholars selected to take part in the JTS are typically regarded by their mentors as having the potential to make major contributions to the discipline, and from the papers it is not hard to see why. Given space constraints I won't be able to discuss most of the essays in any depth. Instead, I'll confine myself to making a few observations, based in part on my reading of the papers, about the state of the theory field.

In a piece for the ASA Theory Section newsletter in 2004, Michèle Lamont reported the results of an informal survey of theory teaching in the top ten American graduate departments of sociology. Most of those sociologist who taught theory didn't think of themselves first and foremost as theorists. Rather, "the prime subfield-identity of the theory teachers tends to be that of cultural sociologist, comparative historical sociologist, political sociologist, and gender sociologist" (Lamont 2004, p. 14). The only exceptions were very senior scholars. Lamont found a similar pattern when she examined ASA section memberships. Among the sections, it was Culture, Comparative-Historical, Political Sociology, and Sex and Gender that had the largest number of members who were also members of the Theory Section.

Lamont went on to reflect on the significance of these findings. Are they a function of the fact that there are few jobs these days for self-identified theorists, signaling the declining prestige of the subfield, such that those with theoretical inclinations are forced to reinvent themselves as empirical researchers? Do they mean, more narrowly, that top departments, for whatever reason, are reluctant to hire theorists, and offload their required theory courses onto scholars with other interests? Or do they signal a shift in the nature of the theoretical enterprise, with the most innovative and influential work now being done by

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cultural sociologists, comparative-historicists, political sociologists, and others who draw heavily on theory in the course of mounting their empirical investigations, and who use those investigations, in turn, to develop new concepts and hypotheses that then become part of the discipline's general theoretical toolkit? Lamont argued that the last of these interpretations was correct, that the subfields of cultural sociology, historical sociology, political sociology, and so on, had effectively become "theory satellites," and that this was a good thing for at least two reasons. First, empirically-engaged theorists would be less likely to get caught up in the sterile intellectual debates that preoccupied earlier generations of scholars, such as those over agency versus structure or micro versus macro. The real world of empirical research would push them beyond such dualisms and beckon toward more meaningful theoretical concerns. Second, by subjecting theoretical ideas to empirical scrutiny and reformulating them in light of empirical data, scholars in theory satellite fields could help ensure the operation of a feedback mechanism necessary for sociological knowledge to progress.

The papers by Acord, Fridman, Krause, Marlor, Pagis, Medvetz and Whooley are very much in line with Lamont's observations. They are all deeply theoretical in two senses. On the one hand, they proceed on the basis of explicitly theoreticized views of the social world, as in Whooley's attempt to re-envision processes of professionalization through the lens of Foucault and science studies. On the other hand, though engaged with empirical material and situated in subfields like cultural sociology or economic sociology, the papers aim to give back to theory.

Because they have diverse intellectual goals, it is unfair to evaluate the pieces solely in terms of their theoretical contributions—and it is not my aim here in any case to evaluate their specific arguments. But, since the papers were assembled for the JTS, it does seem reasonable to treat them as representative of new theoretical work coming on line, and consider whether, as a group, they live up to the promise that Lamont saw in contributions coming from the discipline's theory satellites. My sense is that they do, with a significant caveat.

Some theorists continue to maintain that theory should be autonomous from empirical research, but there is little doubt in my mind that the theoretical questions raised in the papers are answered better as a result of the authors' empirical immersion. Consider Fridman's analysis of the "Cashflow" game, or Pagis's ethnography of meditation retreats. In the first case the goal, shared by Callon, is to move beyond worn theoretical discussions about whether human social action is inherently rational, and focus instead on the processes and conditions by and under which actors in the economic realm *learn* to behave like homoeconomicus. In coming to so behave, they help enact an economy that functions more or less in line with the postulates of neoclassical economics. Through a detailed examination of one of the many social sites where this learning occurs, Fridman is able to do more than just illustrate the value of Callon's approach. He can also show that there are a number of distinct subjectivities among those who have learned to perform as rational economic actors, or who aspire to do so (for example, small-scale entrepreneurs and investors who dream of striking it rich versus those who have adopted the perspective encouraged by the game and are oriented toward self-generating income streams). Furthermore, he demonstrates that such learning involves both skill acquisition and a fundamentally transformed sense of self. As he writes, "[d]ay after day, thousands of financial best-seller readers perceive the transformation of their economic performance as a profound transcendental experience. They read books, look for online resources, attend workshops and seminars, and play board games. By playing Cashflow, people change their definitions of mobility and financial success, acquire calculative tools that adjust to those definitions, and attempt to change their internal dispositions through work on the self. Players of Cashflow may joke and have fun during the games, but it is nevertheless serious business." This insight, that economic performativity is a matter of self-identity as much as practice, does not follow automatically from Callon's theoretical premises, however much identity is central to other theoretical takes on performativity. And it raises fascinating questions about the even wider array of economic identities, not reducible to generic social roles like entrepreneur or consumer, that may exist in contemporary capitalist economies, as well as about the relationships among them, the diverse institutional settings in which they are cultivated, and the extent to which they may be entangled with particularistic identities of ethnicity, religion, nation, and so forth. Identities of this sort could provide underrecognized cultural foundations for "varieties of capitalism" (à la Hall and Soskice), national differences in industrial policy (à la Dobbin), regionality as a basis for socioeconomic organization in a globalizing world (à la Piore and Sabel, Fligstein, Whitford, Beckert), and so on. Theories in economic sociology as well as action theory more generally are thus enriched through confrontation with empirical material.

Theoretical gain through empirical immersion also characterizes the Pagis study. As Pagis notes, there has been a great deal of research in recent years by philosophers, linguists, and cognitive scientists into the problem of situated cognition, or the idea that "cognition depends not just on the brain but also on the body," that "cognitive activity routinely exploits structure in the natural and social environment," and that "the boundaries of cognition extend beyond the boundaries of individual organisms" (Robbins and Aydede 2009, p. 3). Although one of the distinctive features of this line of inquiry is a blending of philosophical and empirical concerns, reflecting the broader turn toward cognitive science on the part of philosophers that is revolutionizing subfields like philosophy of mind, rarely has it been informed by sociological investigation. Research by "cognitive sociologists" like Cerulo and Zerbuavel has been carried out at arms length from work on situated cognition, despite the thematic overlaps (Oishi et al. 2009; some of Vaisey's work is an exception), while sociological theorists writing about embodied knowledge have often assumed epistemic embodiment rather than exploring its empirical contours (though there are exceptions here too, like Wacquant). To this body of work Pagis brings a healthy dose of sociological empiricism. For real live social actors, she finds, there is no dualism between conceptual and embodied knowledge. Where conceptual knowledge, like Buddhist understanding of the non-self, proves hard to grasp, culturally-inflected embodied experiences such as those meditators undergo may provide a key pathway to learning. Whether this finding will be viewed as significant by scholars of situated cognition remains to be seen, but it certainly contributes to social-theoretical attempts at overcoming the divide between the two forms of knowledge by giving such attempts empirical grounding.

Yet while these, as well as the other essays in this special issue, thus give back to theory through empirical research, I could not help but worry a little while reading them. Although Marion Fourcade is right to say in her afterword that the pieces have some thematic coherence inasmuch as notions of embodied knowledge turn out to be important in many of them, the topical diversity is otherwise enormous. This is true, almost by definition, of the entirety of the work done in theory satellite subfields—it covers a huge substantive range. And this, it seems to me, poses a real problem; not for any particular piece of scholarship, but for the discipline as a whole. It is of course good for sociology to have maximal empirical breadth. But empirically-engaged theoretical work can only give back to theory in the sense of enlarging or enhancing the toolkit most sociologists have at their disposal to solve explanatory puzzles if the theoretical lessons of such work are widely heeded, and their synergies recognized and built upon.

It seems to me there are three possible ways this could occur. First, sociologists could read widely, integrating into the theoretical approaches they deploy relevant ideas they have encountered in far-flung corners of the sociological universe. Yet anecdotal evidence suggests most sociologists do not read extensively outside their primary or secondary subfields. Time constraints prevent them from doing so, and, as the research of Erin Leahey shows, there are strong professional incentives, including monetary ones, to specialize. Will the Fridman piece, despite its importance, ever be read by anyone other than economic sociologists, and ethnographers of economic life in particular? It deserves to be, but sadly the chances are small (larger if and when he publishes a book), and this will necessarily impede its theoretical uptake.

Second, work could be done routinely to summarize what we know from theoretically-oriented empirical scholarship in different domains. At first glance this seems like a perfectly reasonable mechanism for overcoming the problem of topical diversity. There is no shortage of review articles published, whether in *Annual Review of Sociology* or elsewhere, and these *do* seem to be read outside of their specialty areas. While many review articles simply sum up the state of empirical knowledge on this or that substantive topic, like race or inequality, others have wider range and consider a variety of research contributions relevant to cross-cutting theoretical concepts like fields or commensuration processes or emotion work. The problem here is that articles of the latter sort provide insight into a handful of mechanisms and processes, but typically do not take the additional steps of thinking through how mechanisms might combine to yield larger social formations or even social systems, what the nature of such formations might be, or what the conceptual requirements are for their explanation. By and large these articles remain at the level of the middle range. Yet not all valid social-theoretical questions are located at this level.

A third pathway would involve the elaboration of synthetic sociological theories concerned precisely with such combinations—theories that, far from proceeding in an aprioristic fashion, would draw from the richness of work being done in the theory satellites to construct new, overarching theoretical paradigms that bring together conceptual contributions emergent from the welter of empirical research. I do not hold out much hope that one such paradigm will be found that provides the key to answering all sociological questions, but historically it has been in the clash between competing paradigms that the most conceptual clarification and explanatory gain in sociology has occurred. It is surely asking too much to expect that young scholars would undertake such work. But I see precious few efforts in this direction from my senior colleagues either. While new paradigms are occasionally announced, the ambition to do grand theory appears to be in decline, with those who pursue the project usually proceeding in the same old deductivist, empirically disengaged manner as their predecessors.

My hope is that a niche will soon form for scholars committed to inductivist grand theorizing—and not just inductivism in name only, as one finds in Bourdieu. If this occurs, the feedback mechanism posited by Lamont will become fully operative, and theory will in fact benefit from scholarship being done in the theory satellites. If it does not occur, the growth of the satellites will mean that theory, long subject to centrifugal forces anyway, will merely undergo further fragmentation. The latter outcome would be a shame for many reasons, not least that it would mean a failure to profit from the high intellectual quality of theoretically-minded research being done in the satellite fields today, a level of quality to which the papers in this special issue amply attest.

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