

## Generational Differences in Accounts of the Development of U.S. Cultural Sociology—Let Me Count the Ways: Response to Lizardo’s and Mische’s Comments

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*This essay provides a response to comments offered by Omar Lizardo (2011) and Ann Mische (2011) with reference to my piece “How Has Bourdieu Been Good to Think With? The Case of the United States” (Lamont, 2011). All essays appear in this issue.*

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Omar Lizardo and Ann Mische offer a stimulating reflection on my keynote lecture “How Is Bourdieu Good to Think With?” for which I thank them. I am pleased to find several converging elements in our analyses. I will mostly discuss Omar’s comments, as they came to me first, and as I find myself more at odd with his interpretation of Bourdieu’s impact and implications for U.S. sociology than with Ann’s. This may be because although, generation-wise, Ann is in-between us, the formative debates she was immersed in at the New School resemble those I experienced as a young scholar. Indeed, it seems that Bourdieu was enlightening and refreshing to both of us for largely similar substantive reasons, which had simply lost their relevance by the time Lizardo started his graduate training at University of Arizona at the start of the millennium. Or perhaps it is that Arizona did not have a Marxist legacy, and that his consumption of Bourdieu was filtered by what was salient locally: social psychology, network analysis, and so forth. But my purpose here is not to do a sociology of Omar ... so let us turn to the topic at hand.

My essay sketched five strands in the usage and reception of Bourdieu in the United States, and singled out a few additional lines of work. Lizardo paints the landscape somewhat differently, identifying three phases in the historical appropriation of Bourdieu in the United States. His analysis emphasizes different points than mine, lumping in one category the developments up to

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1992 (date of the publication of the omnivorousness line of work) and making finer distinctions when it comes to more recent developments—some of which are somewhat contemporaneous to his own entrance in the field (he earned his Ph.D. in 2006). His essay provides the occasion for reflecting on generational differences in the representation of sociological fields.

My essay analyzes the reception of Bourdieu in the United States from the early 1980s on. I have lived through the (almost) 30-year period that I describe. I offer a reconstruction of the past, one that is certainly “mediated” (as Latour would say), but less mediated by third-party accounts than is the case for Lizardo’s narrative. My experience is in tension with the story he tells, in several ways. Here are a few corrections and additions.

1. In Omar’s account, the period leading to the publication of the omnivorousness thesis in 1992 is presented as a continuous and somewhat linear progression that would constitute a “Phase 1.” This reconstruction downplays many of the uncertainties and tensions that were central in the framing of Bourdieu before 1992.
2. There were always several approaches to Bourdieu that coexisted at Princeton, represented by the work of Paul DiMaggio, Robert Wuthnow, Viviana Zelizer, and myself (until I left for Harvard in 2003) and reflected in many of the books included in our jointly directed series, the Princeton Series in Cultural Sociology, which began in the late 1990s (for details, see <http://press.princeton.edu/catalogs/series/pscso.html>).
3. The notion of the existence of a clear Princeton-Vanderbilt axis is largely a reinvention of the past. Richard Peterson developed his ideas in conversation with agendas that emerged at Princeton, but the connection was both weaker and more multidirectional than Lizardo intimates, although our former graduate students (Bethany Bryson, Timothy Dowd, and Gabriel Rosman in particular) did collaborate or talked often with Peterson.
4. The development of a “*Reproduction-focused*” research program grew in the 1970s, primarily at Harvard, at a time when the generation of Jerome Karabel, David Swartz, Steven Brint, Paul DiMaggio, Peter Dougherty, and several other graduate students met in a discussion group around Bourdieu’s work after some of them had spent time in France (Karabel and Swartz in particular). Their shared involvement in the *Harvard Educational Review* (a journal still edited by graduate students) was an important site of common activity.
5. The connection with a “*Distinction-focused*” agenda was clearly a different phase, one that brought in a number of other players from a range of institutions, who published their work in the early to mid-1990s (Bonnie Erikson, Cynthia Fuchs Epstein, David Halle, John Hall, Michele Ollivier, and Vera Zolberg, to mention only a few of the key players).
6. Much of what Lizardo describes as “Phase 2”—Bourdieu’s own engineering of the diffusion of his work and the publication of influential commentaries (most importantly the two books edited by Calhoun) is roughly

contemporaneous to the omnivorousness thesis (1992 for the latter, and 1993 and 1994 for the former) and to *Invitation to Reflexive Sociology* (1992). These books do not correspond to different historical phases, but were simultaneous, competing framing events.

7. The influence of new institutionalism was already strong from the early 1980s, long before its convergence with a Bourdieu-inflected literature, represented to a limited extent in the more recent work of Neil Fligstein. This agenda shaped a full generation of Stanford-trained scholars who received very little exposure to the work of Bourdieu at the time (John Meyer has never been a theorist of domination).
8. Concerning "Phase 3," Bourdieu would have cringed at the idea of a cognitive sociology that flirts with neuropsychology while downplaying the more phenomenological heritage of the late Durkheim (remember that Durkheim viewed psychology as the nemesis of sociology). If we currently have a convergence between cognitive sociology and Bourdieu-inspired agendas, we need a circumspect and qualified description of the areas of similarities and differences.
9. Most of the third generation in historical sociology is not Bourdieusian in character, as demonstrated in Adams et al. (2004).
10. But is "Phase 3" a phase? How many books do we need before having a trend? Three? Four? Ten? As much as I am sympathetic to the recent work on embodiment, I prefer to reserve judgment for a few years at least.

U.S. sociology has a long tradition of neglecting its collective memory (witness the constant celebration of the Chicago School that we find in France, in contrast with its much more low-key lingering presence in the United States). Against this background, I am left wondering what I have learned from this simple exercise of comparison between modes of lumping and splitting a particular literature. Is it just yet one more case of competing versions of the past that are put to the service of programs for future? More than that, we see how contrasted accounts find support in different experiences of temporality and social trajectories. Thus the importance of focusing on knowledge-making practices to study knowledge production, diffusion, evaluation, and application (Camic et al., 2011). While intergenerational exchanges are essential for cross-pollination, they are also essential for cultivating a collective ability to judge innovation accurately. This is the condition for our continued professional autonomy.

With this in mind, I return briefly to the comments made by Ann Mische, which describe a moment from 1990s sociology that I remember very well. This is a time in recent sociological history when the New School stood out as a particularly vibrant incubator of new thinking, thanks to Tilly and to the group of brilliant students (including Ann) who surrounded him. Ann alludes to the intellectual generativity that resulted from the encounter between Bourdieu and Tilly, a Tilly who was still (for a while) holding on to a view of cultural processes that were marked by its Marxist (and resource-dependency)

origin. What came out of this incubator was theoretically informed empirical work as well as seminal theoretical papers that have had a strong impact on not only cultural sociology, but also the fields of social movement, historical sociology, and beyond. One thinks here in particular of the emotional turn in the study of social movement, of the growing concern for issues of agency and subject formation (present in Ann's work in particular), and of new work about sources of social change at the level of the micro politics of everyday life. Bourdieusian thinking was in the background of many of these new developments, in part because Bourdieu taught us how to think about intersubjectivity with more complexity (by 1970s standards). I am left wondering how this legacy will influence our research as cultural sociologists engage more purposefully in fields such as behavioral economic, cognitive psychology, and evolutionary psychology. I agree with Omar Lizardo and many others that we have to tackle these fields, which have generated such great interest (thanks in large part to the promotional work of literary agent John Brockman; for details, see [edge.org](http://edge.org), one of the most ingenious Internet-based tools for intellectual hegemony I know of). I remain convinced that we should not give away the store and that cultural sociologists should play on their strengths, by reorienting the debates toward the study of intersubjective processes of definitions of reality, a domain for which our most widely used analytical tools (frame, repertoires, narratives, boundaries, institutions, etc.) are uniquely suited.

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