

CULTURE/HISTORY TALK, July 31, 2008
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I first want to thank Lyn Spillman for organizing this mini-conference, and for inviting me to speak at it. I suppose I do have a lot of experience boundary-crossing: I do lots of historical work, e.g., some of it targeted more toward historians and political scientists than sociologists. One of my more recent publications, moreover, strays into the complex and highly regimented field of legal history, where I lack both the requisite law and history degrees, and economics, a field in which I am clearly out of my element but determined to wander nonetheless. From the many *revises & resubmits* this paper¹ received, I learned both that it's ok to stray into others' fields of expertise AND that it's not very wise to try and beat experts at their own game. Instead of critiquing fields that I thought needed critiquing, I was beaten back into the defensive posture of 'contributing something new' to otherwise 'flawless' fields. This was frustrating in that I didn't really get to have my say; but it was rewarding in that it at least earned me the right to study something new and different.

I often work at one or more levels removed from sociology. Borrowing Lynn Spillman's terms, I often write in a kind of inter-disciplinary 'pidgen'— though I would modify her formulation in stressing the sense in which our 'pidgen' sociologies, like all pidgen dialects, reflect a *subaltern* position betwixt and between the hierarchy of disciplines. This, I think, is the dirty little secret of

¹ Jason Kaufman, "Corporate Law and the Sovereignty of States," ASR (June, 2008).

interdisciplinarity — potential rejection by both parties. The gains, however, are noteworthy: intellectual freedom, scholarly breadth, and with any luck, creative, original research.

I'm not going to use the rest of my time trying to describe to you literatures you probably know better than I do. I'm just going to talk about what I think, what I've done, and what I think needs doing.

In my purview, as in Prof. Spillman's, the biggest challenge of doing interdisciplinary work is not theory or language but methodology. What constitutes 'convincing' evidence in a given field?

In this respect, I am an avowed relativist; I don't believe strongly in epistemological 'truth,' nor am I over-awed by most scholarly attempts at establishing it. This seems especially true in cultural analysis; nothing could be more ephemeral, less ontologically certain than meanings, ideas, and opinions. And yet, as a sub-discipline, we seem to be moving more and more toward embracing mainstream sociological standards of argumentation and evidence.

My own preference is for well-told stories; narratives that *seem real* but may or may not be falsifiable, replicable, or even strongly verifiable. This is not to say that it's ok to get one's facts wrong but that the way a story is told can be just as important as the evidence used to support it.

This is probably an unpopular position to take even among cultural sociologists — and I want to make the qualification here that, as historically-oriented cultural sociologists, some of the more popular methods — ethnography

and in-depth interviewing — are not readily available to us; I'm not speaking about those in this context.

As cultural sociologists, we create metaphors and narratives that live or die NOT by the facticity of their claims but by their resonance with our peers and their imaginations.

Frankly, evidence without imagination leaves me cold. It pains me to have to morph a topic I love into some semblance of a physics experiment just for the sake of disciplinarity. Epistemological certainty is a valid concern in some fields of sociology; I wonder, however, whether cultural sociologists have tied their wagons to the wrong horse.

This brings me to history, the other discipline that concerns us here. On the one hand, historians, unlike most sociologists, esteem good prose and value scholarship accessible to non-historians. Cultural historians have been particularly successful reaching lay audiences. On the other hand, historians play their own epistemological games, something a teacher of mine once dismissed as “archival fetishism.” If a story cannot be traced through first-hand documentary evidence — preferably via documents that have not already been plumbed by others — then it is essentially worthless from a disciplinary standpoint. Historians, furthermore, are greatly limited in their freedom to conjecture, predict, and make generalizations based on the evidence at hand. This would make it virtually impossible for me to practice as a professional historian.

Nonetheless, as a reader, I generally prefer history to sociology because I can glean from it well-collated details about particular people and events. From sociology, by contrast, one tends to get material that has already been distilled as trends, mechanisms, and fields; generalizations I prefer to make myself.

As a practitioner of historical sociology, moreover, I tend to view the observations and musings of historians as data-points, much the way quantitative sociologists view individual cases in a longitudinal data set. From my readings, broad and wide, I make Bayesian-esque extrapolations about general trends, looking all the while for 'outliers' and omitted variables. If I do my job well, readers will also gain some sense of the 'confidence intervals' surrounding each of my observation. This obviously doesn't meet the objectivity criteria of most methods classes — you could read the same books I do but come to different conclusions. Then again, we all know the same thing can and does happen with analysis of primary documents AND statistical data. Who are we trying to fool?

Please don't misunderstand me: I have nothing against empirical rigor or experimental design — I sometimes work in this vein myself. I simply think the means — i.e. the methodology — should suit the ends — the topics and questions at hand. This is something historical sociology doesn't do particularly well, I don't think. All too often, our publications seem motivated by the availability of data. These kinds of methodological straight-jackets betray the very things that are so fascinating about culture — its ephemerality, subjectivity, and multi-dimensionality.

If our case studies must be limited to those with the most epistemological purity, then we need consider *what* exactly we can know and *why* we want to know it. Of the two, I put far more weight on the latter than the former: Why we might want to know something, let alone dedicate hour upon hour reading about it, should outweigh how certain we might be about the results. This runs the risk, of course, of turning cultural sociology into mere journalism, though I for one have an awful lot of respect for journalists. There are two things we might learn from them, in fact: one, an appreciation of the ultimate unknowability of events, trends, and people — everything is ultimately hearsay; and two, a healthy respect for our readers, who we bore or brow-beat at our own expense.

With all that said, I do have epistemological standards, and I write about them from time to time. In a 2005 Annual Review piece, called “Endogenous Explanation in the Sociology of Culture,” I tried to differentiate two kinds of cultural argument: those that seek a-cultural explanations of cultural phenomena (i.e. exogenous) and those that seek cultural explanations of cultural phenomena (i.e. endogenous). Can culture explain culture, in other words?

Most endogenous arguments, I concluded, are more descriptive than they are explanatory. They tend to flesh out the similarities and differences of cultural phenomena rather well — though they tend to give short shrift to similarities — but they also tend to leave explanation of said differences aside, or merely conjecture about possible explanations after the fact. My objection to descriptive analysis stems, perhaps, from the fact that I am a cultural sociologist with an

historical bent. Everything changes, or at least moves through time, as any historian would argue, and things that move through time demand ‘explanation’ of what is happening and why. Describing change over time, in other words, is short-hand for explaining them. Analyses that don’t describe change over time leave out a big part of the picture.

At the same time, I’ve noticed of late the reverse trend in many explanation-based papers in historical sociology, particularly those of an institutionalist bent: rather than looking to distill trends into parsimonious accounts, they settle for the historians’ kitchen-sink approach, naming every factor, actor, and mechanism possible and labeling them all ‘institutional’ features of the *explanans*. When I read these, I wonder if historical sociology isn’t simply becoming an institutionalist branch of history. This is an over-correction equally damaging to the future of our sub-discipline.

At the opposite extreme, where generalization remains prevalent, I find another marked tendency to complain about: tautological reasoning prevails in a lot of what I read in culture and history. Given my recent research on the comparative politics of the US and Canada², one that particularly concerns me is Lipset’s: As I read him, he explains current ideological differences with reference to ideological differences at the time of the American Revolution. Though he inserts selective migration as a mechanism that brings these differences to bear over time, I don’t find anywhere in his work an explanation of the *origins* of those differences. In my own work, I reengage this search with my own bias to

² I have a book on this topic forthcoming at Harvard University Press, title TBA.

exogenous explanation: — i.e. independent variables that do not revolve around symbols, memes, beliefs, or tastes.

I think I've found an answer in the colonial legal systems of both countries. The common law evolved in notably different ways in the proto-American and proto-Canadian colonies. But, I realize, this game can get convoluted fast, for can't law be characterized as a distillation of cultural ideals?

Well, no, or so I would have it (though I can't 'prove' it to be so). My approach to law focuses on individuals and organizations making highly contingent decisions, exerting power over one another, and creating the institutional scripts and frames within which hundreds of thousands of cognitively, temporally, and ideologically discrete individuals inhabit their respect spaces in northern North America. We don't necessarily get the law our cultural preferences would lead us to expect.

But, again, is my argument any realer, any more convincing than Lipset's? Should it trump his because it musters better facts or more up to date methods? I think not. Lipset's work became famous not because it was so convincing — it's not very convincing at all — it's famous because it resonates so clearly with the thoughts and concerns of readers; it is *useful* to them. Nor is Lipset regarded as any less of a scholar for lack of archival or statistical research. To me, this says a lot about what makes for meaningful inter-disciplinary work in culture and history. For the most part, cultural sociologists are not physicists of the social world, nor should we aspire to be.

Evidence and generalizability are far from the greatest strengths of cultural analysis. The question I would like to put on the table for discussion is this: What are our greatest assets? What should the 'culture & history' agenda be for the 21st century?

Personally, I think we should just research topics not on their 'researchability' but for the windows they open into pressing, real-world concerns. In my own work, that has meant research on cultural globalization; cultural exclusion; taste and social networks; and comparative political development. I prefer work that avoids tautological reasoning and offers creative, new explanations for relevant, counter-intuitive outcomes. Surely, you have pressing concerns of your own. I nonetheless assert that interdisciplinarity is no protection from the banality of positivism.