

Culture

Section of the American Sociological Association

A Message from the Chair

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Editor's Note

Please let me know your ideas for submissions!

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Mini-conference: "Models in Cultural Sociology" The Wagner School and Sociology Department

New York University

295 Lafayette Street, New York City

August 15, 2007

This is the first of two mini-conferences to celebrate the 20th anniversary of the Culture Section of the ASA. The second will be held in 2008.

How do we use analytic models in cultural sociology? What kind of epistemic tools are they? How are our models related to the theories we espouse? And how do we use models of social life devised by informants? As simplifying representations of what exists in the world, models mediate between our theories and methods. Models stand for and help us see abstract patterns in concrete data, and define what kinds of data we need. In other words, models are conceptual tools we use to guide our research, but unlike theories and methods, we rarely talk about them. That's why I would like to make analytic models the theme of our cultural sociology mini-conference this summer.

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On Justification (and Motivation): A Dual-Process Model of Culture in Action*

Stephen Vaisey

University of North Carolina at Chapel Hill

The sociological meaning of the word "culture" has shifted considerably in the past few decades. DiMaggio (1997) summarizes the trend this way: "The view of culture as values that suffuse other aspects of belief, intention, and collective life [i.e., the "Parsonian" view] has succumbed to one of culture as complex rule-like structures that constitute resources that can be put to strategic use." Though we often debate in general terms "how culture matters," what we usually want to know how culture matters *in the course of human action* (see e.g., Swidler 1986). We might therefore restate DiMaggio's description as follows: the view of culture as a social force that generates

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Art from Start to Finish: Jazz, Painting, Writing, and Other Improvisations

Howard S. Becker, Robert B Faulkner, Barbara Kirshenblatt-Gimblett (eds.)

University of Chicago Press, 2006.

Following are the revised texts of an "author-meets-critics" session held at the inaugural conference of the Research Network on the Sociology of Culture of the European Sociological Association, at Ghent, Belgium last November. By prior arrangement, the contents of the session appear in *Culture* as part of an ongoing project of fostering greater communication among European and U.S. sociologists of culture.

Marie Buscatto

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This book originated, in part, in France, under the impetus of the late Alain Pessin and Olivier Majastre. In 1999, more than fifty French-speaking sociologists of art met in Grenoble to define what a "sociologie des œuvres," a "sociology of artworks," might "be." The participants analyzed several works from various artistic fields – cinema, painting,

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Culture

Diana Crane

University of Pennsylvania

Most sociological studies examine the social context of the arts, either their production or their reception. Studies of the content of artworks are relatively rare because such studies are difficult and controversial (Lévy and Quemin, 2007). They are difficult because they require a highly specialized type of knowledge of a particular form of culture, including its vocabulary, its codes, and its standards

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Mark D. Jacobs

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The topic of this volume—"the art-work itself"—is significant in part because it translates into the idiom of pragmatism a major theoretical issue of critical theory: the autonomy of art. Howie Becker challenges his interlocutors, each studying a very different case, to engage this issue

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Howie Becker Responds...p. 10
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Thinking about models can help us understand what Karin Knorr-Centina would call our epistemic culture. In her study of biologists and physicists, Karin found that each discipline has a distinct epistemic culture that defines what it means to do good work – what there is to know, how to measure things, how to model, how to make arguments, how to design research instruments, and how to define progress in a line of research. The notion of epistemic culture can be equally well applied to our (sub)field. We have our own notions of good work that members of the culture section have been developing over the last twenty years. I'm hoping we can learn more about them by looking closely at the modeling practices we use.

In the period when the section was being founded, Pete Peterson, Paul DiMaggio, Howie Becker, and others outlined an epistemic culture for the subfield. The “production of culture school” was less a theoretical movement than a program for doing and defining good work. When Elizabeth Long contrasted the sociology of culture to cultural studies she was talking less about analytic differences than epistemic ones. The theoretical traditions that scholars employed were more similar than their patterns of scholarship, reflecting epistemic differences that created a divide between the two fields. Successive scholars have raised new questions about the epistemological adequacy of work in cultural sociology. And all these efforts to shape our epistemic culture have affected our modeling strategies.

What difference does modeling make? It is a tool for thinking. We make models to find out or stand for what we think, and then we consider their adequacy. Models are points of reflexivity, and intellectual building blocks, too. They help us define “parts” of the object we are studying, translating theories of power, for example, into a set of behaviors or processes we could actually see.

All scholars who write a book or paper must convince themselves that they know what they are talking about. To do this, they often use models. Doing a study is (at least at one level) a process of developing, refining and redesigning models either to fit data more adequately, or to create a body of data that can address a theory or research problem. A research problem itself contains of a model of we expect to find, and what kind of finding would be conceptually valuable. Finally, the patterns that we “see” in field notes are in fact models that we think might prove analytically fruitful.

Let me give you an example from my own work of how models can drive research. Last summer, I was trying to write a chapter for my book on the Canal du Midi. I wanted to describe the design of the water supply for the canal. I had a model of the culture of construction; it was a meeting place of formal expertise and folk or tacit knowledge. I had the technical history straight; I knew how the water supply was designed. I could see some of the formal measures used for the work, too; they were clear from the archival documents. But I couldn't really say much about local knowledge relevant to work. I assumed that peasants in the area had had intimate knowledge of the local topography relevant to a water system, but how could I find any evidence of it? And how could I recognize its relevance? I developed a model. Locals would learn about the landscape by using it, so I had to go see how it had been used.

I got to the area of France where the canal had been built, and took the old road from Toulouse into the mountains, knowing that people in the region had been using that route well before the canal was built. I found myself riding the ridge of the continental divide – the high point between watersheds. This ridge was integral to my story, and here the winds were strong, so windmills dotted the landscape. No geographer needed to locate the continental divide for local people in the 17th century; the wind and windmills from the period marked it. The divide was also located in a spiritual way with images of the Virgin — shrines and small statues of the Virgin in the small towns along the road. I learned from an old man I met in one of these villages that the Virgin had been venerated on this ridge for centuries because the plague had stopped in these hills — a miracle that was attributed to her mercy. Presumably the plague, carried by rats and notoriously spread in boats, did not easily cross the divide between watersheds.

Plague and miracles as well as windmills, then, helped create an indigenous geography that imbued the divide with both practical and symbolic significance. I also learned that the divide was understood in folklore as the area where the waters parted. This was vernacular knowledge of the sort I had hoped to find, if my model was correct. I came home from this trip to France with pictures of mills and Virgin shrines, and of the an old man who had been my informant. I also came home with words to write, too, because I had found indigenous knowledge of the landscape resulting from local uses and understandings of the topography. I could now talk about an indigenous way of knowing that was clearly pertinent to the design of the water system for the canal. (To remind me of it, I promptly made my desktop image an old windmill I had photographed framed against the deep blue sky at the continental divide.)

I hope that the mini-conference after the ASA meetings this summer in New York will give others a chance to talk about modeling as an analytic process, and reflect on how models serve as intellectual scaffolding in research. I hope we can come away from the conference with a better understanding of the epistemic culture(s) we've been developing over the last twenty years, and what opportunities remain open in our models.

The mini-conference will include two morning plenary sessions on conceptual modeling in cultural sociology, and “found” models that we study and use. In the afternoon, there will be topical workshops on a variety of subjects: scenes, democracy, the arts, education, toolkits/epistemic cultures, political culture, gender/sexuality, and things/materiality.

The participants (so far) include these former chairs of the section: Paul DiMaggio, Cynthia Epstein, Gary Alan Fine, Michèle Lamont, Elizabeth Long, John Mohr, Michael Schudson, Ann Swidler, Robin Wagner-Pacifici, Eviatar Zerubavel, and Vera Zolberg. It seems appropriate to ask the former chairs to present at this anniversary mini-conference, but I will post a tentative schedule so others can propose topics for sessions, too. I will then send out a program for the mini-conference, and provide more details about registration as these arrangements are finalized.

action has become one of culture as a social resource that enables or makes sense of action. Instead of values, norms, and beliefs, we now speak of toolkits and repertoires. In other words (simplifying only slightly), culture has gone from something that *motivates* action to something that *justifies* action.

Like any intellectual trend, this reframing of culture has produced a counter-trend. The most visible is Alexander's "strong program," which aims to preserve a more directly causal role for culture. Unfortunately, empirical work from this perspective has been more focused on asserting cultural coherence and autonomy than on theorizing the action-theoretic properties of culture per se. Culture's motivational force is averred, but little argument has been presented that engages substantively with the action-relevant claims of the toolkit school.

To begin such an engagement, I want to examine the adequacy of the justificatory, repertoire, or toolkit approach as a theory of culture of in action.¹ I briefly review its basic structure and supporting evidence and assess it in light of relevant research. Following DiMaggio's suggestion, I look primarily to cognitive science (broadly defined) for inspiration. This is not because of a desire for psychological or neurological reductionism, but rather because (as shown below) cultural action theories rely in crucial ways on assumptions about human cognitive processes. In contrast to DiMaggio's conclusion that cognitive science supports toolkit theory, however, I argue that the cognition literature actually problematizes its adequacy. Based on this exercise I sketch an alternative theory—a "dual-process model" of culture in action. I argue that this model is not only more consistent than toolkit theory with what we know about cognition but that it is also better able to tackle seemingly contradictory empirical findings.

What is Toolkit Theory?

"Toolkit theory" is a *zeitgeist* as well as a loosely defined school in our subfield. It includes the work of Swidler, Lamont, Boltanski and Thévenot, and others who conceive of culture as something people "use," "deploy," or "mobilize." But this idea is also connected to larger trends in sociological theory that have deemphasized Weber's concern with subjectivity and motivation in favor of emphasizing intersubjectivity and communication (Campbell 1996). Though there are meaningful variations on the basic toolkit or repertoire theme, there are important similarities as well. The main thrust of the perspective is as follows: first, we are all embedded in social networks, situations, and institutions that present us with problems and have power to penalize deviant behavior; second, different social locations provide us with different cultural tools to deal with these situations; third, we use these cultural tools to cope with and make sense of the institutional and network pressures that constrain our actions.²

The intellectual history of this view is too complex to trace here, but Swidler (2001) sums up nicely its empirical logic. The argument goes something like this: people generally pursue consistent lines of action; however, when asked to explain these lines of action, people invariably give contradictory or incoherent accounts of their motives. Being contradictory, the

cultural accounts themselves cannot really be motivating and we must therefore turn outside the person's subjectivity to find the true springs of action. These springs of action are found in institutions, because they have the power to control "departures from the [institutionalized] pattern" via application of "rewards and sanctions" (Jepperson, quoted in Swidler 2001). Therefore institutions both large (e.g., the legal structure of marriage) and small (e.g., my friends) are what drive action, while culture is what helps makes sense of these actions.³

Reasons for Doubt

This argument for toolkit theory is simple, insightful, and elegant. It turns out, however, that it is also based on unrealistic assumptions about the necessary cognitive link between cultural beliefs and motivation. In implicit harmony with the long tradition of Kant-inspired moral philosophy and psychology (e.g., Kohlberg 1981), Swidler assumes that if cultural beliefs *were*, in fact, motivational, they would *have* to be grounded in articulable, rule-like cognitive structures. Moreover, she assumes that if beliefs were motives we would find consistency between the moral beliefs people articulate and their subsequent actions. (That is, if people *really* believed in the romantic model of marriage, they would divorce the instant their marriage no longer promised fulfillment.) Failing to find either pattern, Swidler concludes that the (contradictory) beliefs her informants articulate must be causally unrelated to action itself. DiMaggio (1997, 2002), reviewing the cognitive science literature, makes a similar point. He argues that people indeed know a lot more culture than they use, and that much of this cultural information is contradictory and stored away without reference to its truth value. It follows from this reasoning that the cultural schemas people internalize (being contradictory) cannot themselves be the reasons behind observed behavior. Consistent with the institutional-network focus of toolkit theory, DiMaggio maintains that we should look to the "external environment" for the social cues that activate particular cultural schemas rather than others in different situations.

These arguments are problematic in two ways. First, *contra* the long tradition in moral philosophy, there is increasing evidence that culturally-variable moralities motivate people via intuitions and emotions rather than through conscious reasoning. Psychological anthropologist Richard Shweder, psychologist Jonathan Haidt, and their colleagues have studied cross-cultural differences in moral intuitions for a number of years. Through a combination of cross-cultural fieldwork and ingenious laboratory experiments (see reviews in Haidt 2001, 2005; Shweder 2003), these researchers have found that people make strong moral judgments even when they are unable to provide coherent (or indeed any) reasons for those judgments. These moral intuitions and emotions are not universal in character, but vary according to factors such as country, education, and political affiliation. Rather than merely clinging to their action commitments, respondents manifest distinctly different emotions like contempt, anger, and disgust depending on the implicit cultural code being violated. The upshot of this research is that people can have—and can be motivated by—culturally-

shaped beliefs to which they do not have conscious access. Thus, we need not turn exclusively to institutions or networks to explain the things people do.

Second, DiMaggio's assertion that culture is simply "stored away" without differentiation is only partially supported. There is indeed evidence that people file away bits and pieces of culture and draw on them strategically (say, to win an argument). Yet the primary literature on which DiMaggio relies (cognitive anthropology) has also come to the conclusion that some cultural schemas are more internalized than others. Far from rejecting the notion of internalized beliefs and values, D'Andrade discusses how "the beliefs and values of a culture may be internalized" through "secondary appraisals [i.e., cultural talk] and the cultural shaping of emotion" (p. 227). D'Andrade outlines four levels of internalization, from simple acquisition, to the "cliché stage," to belief, to belief with high salience. He contends that while the lower stages of internalization (on which DiMaggio focuses) concern classification, cultural knowledge, and social reasoning, the final stage becomes truly motivational: "this cultural shaping of emotions gives certain cultural representations emotional *force*, in that individuals experience the truth and rightness of certain ideas as emotions *within* themselves" (p. 229, emphasis in original). He then spends the next fourteen pages discussing how cultural representations can serve as motives for action in some persons and groups. In sum, while arguing *contra* earlier anthropologists that cultural schemas are not perfectly shared or perfectly internalized by *all* members of a given society, cognitive anthropology has certainly not rejected the idea of "culture as values that suffuse other aspects of belief, intention, and collective life."

Toward a Synthetic Theory

With rough sketches of these ideas in place, we can draw meaningful contrasts between toolkit theory, the older "Parsonian" view it displaced, and a more realistic theory of culture in action. First, a realistic model of culture would, like toolkit theory, take cultural pluralism and contradiction seriously. Second, a realistic model of culture in action would also, like the older Parsonian theory, take seriously the possibility that cultural beliefs and values can be subjectively motivating. Adding cultural pluralism and variable internalization to the Parsonian model corrects its functionalist deficiencies and opens up space for analyzing competing and conflicting cultural motivations.

The idea that culture can be both sense-making and motivating—both dependent and independent variable—is appealing, but it raises intellectual problems as well. As John Martin (2003) has pointed out, simply saying "both" to thorny theoretical dichotomies creates more problems than it solves. The particular danger here is that choosing to treat culture as motivational or justificatory will be based on personal preference or intellectual networks rather than on substantive or empirical considerations. This is largely the case already, with sociologists of culture treating moral beliefs (for example) as repertoires provided by particular social locations (e.g., Lamont et al. 1996) and sociologists of religion treating those same beliefs as causes of behavior (e.g., Smith 2005).

Nevertheless, I want to argue that a central development in cognitive science—dual-process theory—can help us integrate motivational and justificatory views into a coherent model of culture in action. Dual-process theory holds that humans have two basic cognitive systems, one automatic and largely unconscious, one slow and largely conscious (see e.g., Chaiken and Trope 1999). Based on research in dual-process theory, psychologist Jonathan Haidt (2005) maintains that human cognition is less like a computer (or a toolkit) and more like a rider on the back of an elephant. The rider, which represents our conscious processes, is the part of ourselves we know best—she can talk, reason, and explain things to our heart's content. Yet, for the most part, she is not in charge. The elephant, which stands for our automatic processes, is larger and stronger than the rider, and is totally unencumbered by the need, or the ability, to justify itself. As the metaphor implies, the rider is no match for the elephant in a direct struggle. While the rider usually merely pretends to be in control, she *can* slowly train the elephant over time or perhaps trick it into going a different way. But in any given moment, the elephant is probably in charge.

It is important to note that the rider-elephant metaphor is not only a metaphor but rather an encapsulation of decades of neurological and psychological research (see Gazzaniga 1987; Wilson 2002). It is also imperative to point out that reliance on neuroscience and psychology to develop a more realistic model of human cognition is by no means reductionist. Rather, "the elephant" provides a validated mechanism for understanding how society can "get into" human beings that is eminently compatible with (among others) Giddens's "practical consciousness" and Bourdieu's "habitus."⁴

Taking dual-process theory seriously leads to a simple, but potentially powerful, heuristic model of culture in action. This model maintains that, for the most part, people are motivated by deeply internalized cultural schemas that are backed up by specific emotions. Though of course influenced by external cues (e.g., framing, opportunities) and the possibility of sanctions, people are motivated to pursue lines of action that are consistent with these deeply-held (often unconscious) schemas. (One might call this "elephant mode.") Yet when asked to justify their actions, people can switch into "rider mode" and provide a variety of reasons to justify their conduct, even if the reasons have little to do with the actions themselves. To simplify, we might say that while the rider *uses* culture, the elephant is *shaped* by culture.

This is not simply synthesis for the sake of synthesis. This dual-process model of culture in action has a number of straightforward implications for empirical research. Foremost is that it makes the choice of methods much more important. There is every reason to believe that interview methods are better suited to engage with the "rider" than with the elephant, thus exaggerating cultural contradiction and incoherence. Interview methods are vital for understanding how people make sense of the world, but since they have little purchase on the "elephant," they cannot easily be used to assess the presence or absence of different cultural motivations. Surveys, on the other hand, because they require people to choose the single response that resonates best with them (i.e., with their "elephant"), are well-positioned to distinguish the schemas that motivate action from those that are simply "available" to memory.

Put another way, if talking about our mental processes with an interviewer is like describing a criminal suspect to a sketch artist, then answering survey questions is like picking a suspect out of a line up. This suggests an unorthodox methodological possibility: given the right “line up,” forced-choice surveys may in fact provide more reliable access than interviews to the cognitive processes most relevant to action.

This points the way toward true mixed-method designs. The dual-process model suggests that, rather than supplementing or enriching each other in an optional way, survey and interview methods may be jointly *required* to fully understand how culture matters. My own research on American young adults supports the utility of this approach: despite their discursive inarticulacy about the link between moral beliefs and action in their lives, my respondents’ answers to survey questions about moral judgment are strongly associated with volunteering, various forms of delinquency, and even marked changes in the composition of their social networks some three years later (net of many other factors).⁵ The interviews alone scream “toolkit!” The surveys alone suggest that people act on conscious beliefs. Looking at both, however, yields new insights into basic cultural processes.

Conclusion

I have argued that cultural sociology could benefit from a dual-process model of culture in action that takes both justification and motivation seriously. While I have stressed the limitations of toolkit theory, I should note that many of the points I am making are in fact hinted at by its primary advocates. Swidler, for example, acknowledges that culture can “use us”; the model presented here simply shows how that might work. She also emphasizes habit; I merely extend this to include moral habits as well as behavioral ones. DiMaggio acknowledges the importance of dual cognitive processes; I simply build on those insights and consider their implications for understanding cultural motivation. My goal has not been to tear down, but to build up our collective enterprise. That said, cultural theory is still in its early stages and metaphors matter. I hope I have persuaded some readers that a change from “toolkit” to “rider on an elephant” would in fact constitute theoretical progress.

REFERENCES

- Bourdieu, Pierre. 1984. *Distinction*. Harvard University Press.
- Campbell, Colin. 1996. *The Myth of Social Action*. Cambridge University Press.
- Chaiken, Shelly and Yaacov Trope. 1999. *Dual-Process Theories in Social Psychology*. Guilford.
- D’Andrade, Roy G. 1995. *The Development of Cognitive Anthropology*. Cambridge University Press.
- DiMaggio, Paul. 1997. “Culture and Cognition.” *Annual Review of Sociology* 23:263-87.
- . 2002. “Why Cognitive (and Cultural) Sociology Needs Cognitive Psychology.” Pp. 274-81 in *Culture in Mind*, edited by Karen A. Cerulo. Routledge.
- Gazzaniga, Michael S. 1987. *The Social Brain*. Basic Books.

- Haidt, Jonathan. 2001. “The Emotional Dog and its Rational Tail: A Social Intuitionist Approach to Moral Judgment.” *Psychological Review* 108:814-34.
- . 2005. *The Happiness Hypothesis*. Basic Books.
- Kohlberg, Lawrence. 1981. *Essays on Moral Development*. Harper & Row.
- Lamont, Michèle. 1992. *Money, Morals, and Manners*. University of Chicago Press.
- et al. 1996. “Cultural and Moral Boundaries in the United States: Structural Position, Geographic Location, and Lifestyle Explanations.” *Poetics* 24:31-56.
- Martin, John Levi. 2003. “What is Field Theory?” *American Journal of Sociology* 109:1-49.
- Mills, C. Wright. 1940. “Situated Action and Vocabularies of Motive.” *American Sociological Review* 5:904-913.
- Sayer, Andrew. 2005. *The Moral Significance of Class*. Cambridge University Press.
- Shweder, Richard A. 2003. *Why Do Men Barbecue?: Recipes for Cultural Psychology*. Harvard University Press.
- Smith, Christian. 2005. *Soul Searching: The Religious and Spiritual Lives of American Teenagers*. Oxford University Press.
- Strauss, Claudia and Naomi Quinn. 1997. *A Cognitive Theory of Cultural Meaning*. Cambridge University Press.
- Swidler, Ann. 1986. “Culture in Action: Symbols and Strategies.” *American Sociological Review* 51:273-86.
- . 2001. *Talk of Love: How Culture Matters*. University of California Press.
- Wilson, Timothy D. 2002. *Strangers to Ourselves: Discovering the Adaptive Unconscious*. Harvard University Press.

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ENDNOTES

1 In the interest of brevity, I will refer to this as “toolkit theory.”

2 The “French” variant of this perspective adds to these three a greater emphasis on power relations. It holds that differential access to tools or repertoires is a key mechanism of stratification because groups in power use cultural repertoires to draw boundaries between in-groups and out-groups (e.g., Bourdieu 1984; Lamont 1992).

3 See Mills (1940) for a very early—but remarkably up-to-date—version of this argument.

4 Though Bourdieu (e.g., 1984) focuses on the strategic dimension of habitus, Lamont (1992), Sayer (2005) and Strauss and Quinn (1997) provide reason to consider a moral dimension of habitus that is very consistent with the view presented here.

5 Similarly, we might wonder whether respondents’ endorsement of either the romantic or prosaic-realist model of marriage might be predictive of differences in divorce rates, infidelity, or other interesting outcomes.

writing, sculpture, dance, cyber-art, circus, tattooing, literature and music (Majestre et Pessin, 2001). Howard Becker and Pierre-Michel Menger were among main speakers, together with other prominent figures of French sociology of arts—Nathalie Heinich, Bruno Péquignot, Alain Pessin and Jean-Pierre Esquenazi. Raymonde Moulin made concluding remarks: “In the last twenty years, the sociology of art has been one of the most convincing interdisciplinary fields. The analysis of the artwork is at the heart of this interdisciplinarity” (Majestre et Pessin, p. 472, my translation)

Howard Becker, Robert Faulkner and Barbara Kirshenblatt-Gimblett have made it a complete new story. They convened a meeting of colleagues, mostly American, who studied different artistic fields – mainly music, literature and painting – and belonged to several academic disciplines – sociology, economics, musicology, ethnomusicology, communication and media studies, art history, performance studies. Interdisciplinarity became key here, multiplying the perspectives and questions relevant to the “work of art itself” (the title of Becker’s chapter).

The authors all shared the idea that art is not an individual product, that “Art is not social because social variables affect it but because it is the product of collective work, the work that all these different people do, which, organized in one way or another, produces the result that is eventually taken to be the artwork itself” (pp. 2-3). This is not news to us; Becker’s concept of an “art world” has become a key concept for sociologists of art, one they start with when they investigate artistic activities.

These experienced scholars asked some tricky and original questions: What is a work of art? When is an artwork finished? Can it ever be said to be finished? What does studying works of art tell us about artistic work? How can we study works of art empirically? Every chapter in the book, whether drawn from archives, observations, interviews or autobiographies, discusses these questions, not looking for definite answers since their first point of agreement is that the work is indeed never finished. Which means that the title is quite misleading, this book is not about *Art from Start to Finish*, it is more about art which is never finished, about endless ends of artworks, about “art from start to a never-reached finish” (p. 20). If people nevertheless regularly say that artworks are complete and finished, they are in fact always working their way through new interpretations, modifications, revivals, partial or complete destructions... and the researcher’s job is then to identify and grasp as completely as possible, even if never fully, the work’s everchanging nature. Researchers then need to study economic, historical, social, personal, technical and even family reasons which may “explain” a specific artwork reality, as demonstrated by economist Richard E. Caves, historian Michael D. Harris, sociologist Robert Faulkner or artists Larry Kagan and Max Gimblett.

Let’s now focus on one very specific topic which is not only of great interest to me as a sociologist of artistic work, but also leads to renewed thinking about artistic activities. Several chapters deal with creation and improvisation. This may at first seem surprising. For years, social scientists have debunked

myths of the genius, of the vocational ideal, of inspirational stereotypes. What can we say about creative processes in the production of artworks without falling in such mythical traps? Is studying artworks and their process of construction a good way to study creativity? What methods do we need then, which principles shall we keep in mind, how can we “go about it”?

Take as an example Robert Faulkner’s detailed analysis of what jazz improvisation consists of. This musician and sociologist tries to solve this apparent contradiction: improvisation is the fruit of “organized imagination” (p.93), it implies both spontaneity and discipline, work and inspiration, indeterminacy and organization. How does that work? He makes careful, detailed analyses of what improvisation is founded on: work the public never sees, hidden in the musician’s “workshop.” He connects those findings to the way improvisation finally occurs on stage. He articulates what people do on stage—their improvisational talent, their ability to find spontaneous paths to great music—and what those same people do at home—practice, rehearse, repeat, learn basics. Improvisation then becomes a permanent “aller retour,” a back-and-forth between “exploitation and exploration.” An artist is both a humble learner and an inspired creator, the two activities here carefully articulated and described through precise and detailed examples. “Improvisation involves working on preplanned and precomposed musical ideas or designs coupled with ‘unanticipated ideas conceived, shaped and transformed under the special conditions of performance’” (p. 93).

Several other chapters follow the same line. Pierre-Michel Menger shows how Rodin’s creativity is partly rooted in “the unfinished and its combinatorial resources: defective creation, hybrid assemblages, and plural creation” (p. 50). Berliner leads us to Zimbabwe to find out that Mbira instrumentalists both need to learn a broad basis of musical knowledge and at the same time be able to (re)invent musical ideas in the moment. Invention is partly rooted in past knowledge. Writer Michael Joyce and sculptor Larry Kagan discuss in a very open and personal way how their own creativity results from subjective choices and are also the result of encounters, discussions and learning processes. Creativity remains a “mysterious” reality, but is also a better understood process, both social and personal, disciplined and chaotic.

How do you investigate such a topic? If the artwork is “fundamentally indeterminate,” if it “changes continuously,” the researcher has to find ways to study those processes at length. If the work’s “finishedness” is both a matter of personal and subjective choices and of collective rules and conventions, how do we follow such personal and collective processes over time? How do we distinguish the artist’s “choices” from collective pressures or chance events?

Thorough observations, accomplished *in situ* and over time, appear here as a methodological “model.” How else may one get access to a detailed description of the interactions, discussions, hesitations, changes or resistances which shape the artistic work and artists’ decision to stop or at least let it go (on stage or outside their workshops)? Observations can be replaced by the use of archives to study past events, as Menger

studied Rodin; by an aesthetic contextual analysis of the artwork, as DeVeaux studied Rollins; or by autobiographical accounts, such as those produced by Max Gimblett (in the article by Barbara Kirshenblatt-Gimblett) or Michael Joyce. The ideal remains the same: describe as precisely as possible how one specific work of art developed, was transformed, left out, destroyed, and/or reinterpreted over time. The detailed description of subjective and objective processes which have affected both the form and the content of a specific work gives us a strong feeling of its indeterminacy, its incompleteness, its impossible end. Studying works of art becomes an endless task which implies multiplying sources, searching for small details, getting obsessed with a diversity of sources.

Can researchers generalize from such detailed, thorough micro studies? This is where the book becomes really enlightening. Generalization is done in such subtle and progressive ways that you may not recognize it when you read it. As the editors discuss in their introduction, generalizing here means finding general ways of thinking about works of art, not as finished products, but as the result of collective and subjective processes which are endlessly constructed over time, which “occur in a series of steps” (p. 4), which are never given and may have been different. Generalization happens here through an implicit comparison of studies in several artistic fields—music, literature, painting—at different times—historical and contemporary—and even in different countries—France, United States or Zimbabwe. The exchanges between the authors during the meeting that produced the book enabled the three editors to come up with a grounded description of works of art as having “careers,” as being indeterminate, as being subjected to many influences.

This leads me to one warning and one request. To make such an analysis hold, one needs to be a knowledgeable virtuoso, an intellectual artist, if such a thing exists . . . and to have plenty of time to go back and forth through many layers of interpretation. You have to collect “data” on all aspects of each studied work—historical, economic, social, familial, personal or technical—so that all pieces can be put together neatly. You also have to be able to both analyze an artwork technically and master sociological concepts scientifically. You need time to gather several layers of knowledge, time here being crucial so that you do not become a victim of the illusion of “finishedness.” Menger, Becker, Faulkner, Berliner and Caves are recognized scientific virtuosos, who need not show the “tricks of their trades” to make their analyses persuasive. But what would happen if this were done by less experienced and gifted academics, not able to articulate “exploitation and exploration” so well? It is always the case that interesting scientific works are created by high quality researchers, but the dangers seem here higher than usual.

A request now, for future works. The book never asks this crucial question: is creativity a gendered process? No answer here, maybe due to the fact that only one woman participated in the project. Indeed, what might be the influence of social class, ethnic background, gender, or age on the way artworks develop over time? Those “social factors” were not the authors’ priority. Perhaps they will be more prominent in the next book, which we will await with great expectations.

REFERENCES

Majastre, J. O. and Pessin, A., (eds.) 2001. *Vers une sociologie des oeuvres*. Tomes I et II. Paris: L’Harmattan.

Crane on *Art from Start to Finish*, continued

for style and method. Such knowledge, which most sociologists do not have, can be acquired, but it takes a considerable amount of time. In the process, the sociologist’s orientation toward her subject matter may become less like that of a sociologist and more like that of an artist. The results of such studies may be of little interest to many sociologists because specialized knowledge of the arts is also needed to appreciate such studies.

These types of studies are controversial for several reasons: First, is it permissible to study groups of artworks on the assumption that the works are equivalent? Second, if a single artwork or a single artist’s artistic output is the object of study, what types of generalizations can be made? Some sociological critics argue that artworks are so variable and unique that sociological generalizations are impossible to make.

One solution is to treat artworks as if they were a type of sociological data, a source of information about the society in which the artist lived or about the cultural field in which the artist was operating, as in Bourdieu’s study of Flaubert (Bourdieu, 1992). Alternatively, the content of the artwork can provide evidence for theories of artistic creativity or for theories of artistic change. Still another possibility is to look at art as a type of language: the sociologist studies its codes and interprets its meanings. The problem with all three of these approaches is the high degree of variability of interpretations of

artworks by sociologists as well as by artists and critics (Raynaud, 1999: 139; Lévy and Quemin, 2007). Lack of consensus may inhibit progress in the field and the development of credible results.

Art from Start to Finish presents a promising research program for studying the content of artworks. The goal is to study the creative process which is conceptualized as a series of choices by the artist that lead an artwork in one direction or another. By following the ‘career’ of an artwork and how it changes over time, the sociologist learns a great deal about its content and meaning. This type of analysis raises the question of how and when an artwork is finished. According to the editors of this volume, artworks are indeterminate, changing even after artists have ceased to work on them. The unfinished character of artworks is an outcome of the collective nature of artistic activity. The editors state: “Social science...can deal with the ‘the work itself’...by focusing on the networks of interaction and organization within which the works are made by some people, remade by others as they view them or hear them or read them, taken care of or not by still others, interpreted and reinterpreted by any and all of these people, always keeping in mind that the work has its own characteristics that play a part in this shaping and reshaping....” (p. 6).

The methods that the editors propose for conducting this type of research consist of fieldwork with artists, specifically

interviews and observations, as well as analysis of documents and other vestiges of an artist's career. Becker states in his chapter in the volume that he doubts whether most sociologists would want to conduct this type of research because they are rarely interested in "a single instance of some type of behavior" (p. 29). He suggests that sociological analyses of the content of artworks are most likely to be done by scholars in other disciplines. The majority of the authors in the book are not sociologists.

One problem with this research program is that there are many different art forms but the approach is more suitable for some art forms than for others. In this volume, literature is the poor stepchild, perhaps because it is more difficult to apply this type of analysis to literature than to the fine arts or music. The fact that different art forms embody in different ways the issue of "unfinishedness," to coin a term, is not sufficiently acknowledged in the volume. For example, there is a major difference between arts that are performed and those that are not that has important implications for the relevance of the research program. There is also no discussion of the ways in which artistic choices are programmed by genres and formulas which abound in jazz (one of the major examples of art in the volume), as they do in all forms of art, including literature, film, and other types of music.

Research on the careers of artworks is most feasible when a great deal of documentary material and artifacts are available. In the absence of these types of evidence, the researcher is forced to rely on the artist's retrospective reconstruction of events and influences that culminated in a specific artwork. Using the exceptionally rich body of materials concerning the evolution of Rodin's sculpture, Pierre-Michel Menger is able to advance and refine the research program laid out by Becker and his colleagues. In comparison with Menger's article, some of the other papers are more fragmentary and sometimes heavily autobiographical. One might also say 'relatively unfinished,' but perhaps not in the best sense of that word. Larry Gross refers to the content of his paper as "fragmentary and scattered reflections" (p. 149). Robert Faulkner says his paper is "a preliminary study of improvisation" (p. 93). Two papers by or about artists, Larry Kagan and Max Gimblett, provide interesting accounts of their aesthetic trajectories but a sociological analysis of these accounts is needed to make sense of them. This type of analysis is not provided.

The book itself also seems to be 'unfinished.' Perhaps this is appropriate given its subject matter. The book has four 'introductions' but no conclusion. The first introduction is a foreword by a representative of the organization that funded the project. This is followed by a preface in which the three editors provide an outline of the book. This, in turn, is followed by the editors' introduction in which they make a case for their research program. Finally, chapter 1 by Becker entitled 'The Work Itself' discusses different ways of understanding what constitutes an artwork and expresses some reservations about the research program.

Menger's analysis of Rodin's aesthetic career reconceptualizes the volume's research program in a number of very interesting ways but there is no recognition of this fact in the four introductory chapters. In these chapters, he is described as studying Rodin's use of fragments and Rodin's reluctance to finish his artworks but the implications of Menger's study are not incorporated into the research program. In other words, the book needs at least one conclusion.

How does Menger modify the Becker et al. thesis? First, Menger makes the important point that "the successful work imposes a sense of inevitability; the work cannot be imagined otherwise" (p. 47). This implies in a sense that artworks can acquire an aura of being finished by projecting a sense of inevitability about the form they have finally taken. Second, Menger's study indicates that the concept of incompleteness, the unfinished quality of a work, is less clearcut than is suggested in the introductory chapters of the volume. In discussing the role of 'incompleteness' in the work of Rodin, who left a great many works unfinished, Menger suggests that there are degrees of incompleteness and that it is not always easy to define whether a work is incomplete. Menger says: "making a case for a work being incomplete requires knowing, one way or another, what the complete work should have been" (p. 38). Later, he adds: "the meaning of being unfinished is dialectically dependent on the existence of an outcome, a finished state" (p. 41). Menger implies that some works are indeed finished or complete. He argues that the purpose of looking at an unfinished work is to better understand the artist's finished work (p. 44). By contrast, Becker and his colleagues argue that all works, even those that are defined as complete, are actually incomplete because of changes in them that continue to occur after the artist has finished working on them.

Menger also discusses the origins of the interest in unfinished or incomplete works. He traces this interest to the beginnings of modernism in the 19th century. He quotes the famous art critic, Ernst Gombrich, as saying: "the imperfection of perfection was invented in the 19th century" (p. 59). Modernism was a revolt against the perfectionism of the academic art that preceded it. It prioritized inspiration, feeling and emotion rather than technical perfection.

Finally, Menger emphasizes that the exceptional artist works in a multitude of ways and has a multitude of roles; the sociology of art has to be able to identify the different "procedures of experimentation and negotiation" (p. 58) in an artist's works in all their various states of being finished or unfinished, of being produced or being reproduced.

To conclude, one final remark: the editors make no attempt to situate the book in the existing literature concerning the study of artistic content. Most of this literature is in French by European authors who have debated at considerable length whether or not this type of study is feasible. This field includes authors as various as Pierre Bourdieu, Natalie Heinich, Alain Quemin, Raymonde Moulin, Bruno Pequinot, Jean-Claude Passeron, and Dominique Raynaud. The editors state that the idea for the book resulted from discussions at a conference on this topic (see Majastre and Pessin, 2001), but they do not share their knowledge of this literature with their readers. In this sense also this book could be said to be unfinished.

REFERENCES

- Bourdieu, P. 1992. *Les règles de l'art: Genèse et structure du champ littéraire*. Paris : Seuil.
- Lévy, C. and Quemin, A., 2007. La sociologie des oeuvres sous conditions. *L'année sociologique*, forthcoming.
- Majastre, J. O. and Pessin, A., (eds.) 2001. *Vers une sociologie des oeuvres*. Tmes I et II. Paris: L'Harmattan.
- Raynaud, D., 1999. L'émergence d'une sociologie des oeuvres: une évaluation critique. *Cahiers internationaux de sociologie*, 106: 119-143.

by posing the research problem: “when, if ever, is the work of art finished?” The research problem is puzzling, specific, and concrete enough to focus attention on the social organization and process of art as work, as readers would expect from the author of *Art Worlds*. Becker clarifies the problematic by postulating “the Principle of the Fundamental Indeterminacy of the Artwork”: “it is impossible, in principle, for sociologists or anyone else to speak of the ‘work itself’ because there is no such thing. There are only the many occasions on which a work appears or is performed or read or viewed, each of which can be different from all the others. . . . the ‘work itself’ is isolated only by virtue of a collective act of definition. . . . what the work is, while by no way arbitrary, is subject to great variation and can never be settled definitively in some way that is dictated by its physical nature” (23-4). Nonetheless, consistent with these premises of symbolic interactionism, “any work of art can thus be profitably seen as a series of choices. . . . some combination of routine and unusual choices among available opportunities” (26). The methodological implication of this theoretical stance is to “encourage us to work like artists, to improvise, to think against the grain, and to embrace the unpredictable” (18).

Following Becker’s own practice, it is perhaps best to start the analysis of this volume by employing a genetic method to recount the organization and process of the work. This book had its start at a conference in Grenoble, France, devoted to the question, “what about the artwork itself?” (Majastre J.-O. and A. Pessin, 2001). This led Becker, one of the participants in that conference, to organize another conference two years later (under the auspices of the Social Science Research Council and with support from the Rockefeller Foundation) on “the work itself.” Becker circulated his Grenoble paper, along with a paper by Pierre Michel-Menger (also from Grenoble) to a new set of artist-scholars and scholar-artists, as “provocations” to consider the question “how do we know when an artwork is finished?” These provocations led not only to the new conference, but to a serial process of mutual engagement by the participants, in which they successively re-worked their own papers to take explicit account of each other’s. The result is a model for artistic and intellectual dialogue across multiple sets of boundaries. Participants represented the U.S and France, artists and scholars from a range of disciplines, humanists and social scientists, modernists and postmodernists. There is a reflexive logic to the fact that *Art from Start to Finish* has no conclusion. It is a work—indeed an artwork—that explores the unfinished character of art. There is also a pragmatic reason for the lack of a conclusion. The book opens a dialogue so broad and deep that it cannot be closed.

What makes the quality of dialogue even more exceptional is that the participants embrace “architectonically” different semantic presuppositions (see Watson 1993), which often challenge common conceptions of core terms. Menger, for example, disrupts the ordinary conception of “artwork,” by adopting a game-theoretic approach to the analysis of artistic careers; the hypertext novelist and scholar Michael Joyce disrupts the ordinary conception of “artist,” by the principled way

he treats scholarship as a game. Menger offers many explanations for Rodin’s practice of preserving as “art” all his sketches, studies, experiments, fragments, unfinished pieces and reproductions: it was for Rodin a statement about the phenomenology of perception, the dynamics of creation, the idea of art continually surpassing its physical execution. Above all, however, Menger finds in this practice the outcome of a game of artistic choice: he advocates “analysis of a completely deployed space of games, in the precise sense offered by game theory, in which the production, definition, evaluation and commercialization of works, in their various possible states of uniqueness and multiplicity, of being finished or unfinished, of being ‘produced’ versus being ‘reproduced’ . . . are the subject of a remarkably open ensemble of procedures of experimentation and negotiation” (57-8). For Menger, the “artwork” is not an object but a flux. Similarly, for the deconstructionist Joyce, whose Derridean playfulness—in the form of word games, associations, inversions, and permutations—is a perfectly serious analytic strategy, the “artist” is not a central person or even an identifiable group, but rather an infinite network of hypertextual others.

In these and many other ways, the fundamental differences of intellectual orientation embraced by the contributors to this volume extend to their choices of method, voice, intention, and view of reality (again, I am loosely adopting terms suggested by Watson 1993, drawing on the work of his mentor Richard McKeon). Becker invites into dialogue not just fellow pragmatists, but also utilitarians, deconstructionists, and mystics among others. While the economist Richard Caves employs a “logistic” method and Becker a “problem-focused” one, for example, Joyce employs an “agonistic” one. Joyce writes in a “personal” voice, while Menger adopts a “disciplinary” one, and the painter Max Gimblett (interviewed by the performance scholar Barbara Kirshenblatt-Gimblett) breaks into a “revelatory” one. Oddly missing from the contemporary discourse about art is mention of “beauty”; implicitly at least, the community of contributors convey fundamentally different meanings of that commonplace. Gimblett, who practices Rinzai Zen, koan study, and calligraphy, conceives beauty as “substrative,” beneath the surface of appearances. Michael Harris, who studies Yoruba art, is sensitive to the “transcendent” nature of beauty. For Caves, beauty is “essential” at least to the extent that it can be evaluated impersonally by the market.

For that matter, there are profound differences of perspective about the meaning of “indeterminacy.” Indeterminacy enters into these chapters on a number of different levels. Menger recognizes it on the level of the phenomenology of production—Rodin developed a technique involving different stages of perception and correction based (in effect) on a Husserlian conception of the perceptive act. Larry Kagan focuses instead on the indeterminacy of reception; he conceives the shadows cast by mounting his sculptural objects on the walls of exhibition spaces as an integral part of the artworks. Others talk about indeterminacy on the level of social organization, deriving from the plurality of reception. To the deconstructionists, the only thing that is stable is absence.

The architectonic dissonance created by the clash of these various semantic orientations energizes the dialogue and expands its range of expressiveness. Because it is not explicitly recognized, however, this dissonance can also induce interlocutors to talk past each other. For Becker and many others, for example, to “finish” an artwork means (to borrow a formulation from one of Becker’s earlier works) to “get it out the door.” Gimblett seems to agree that a painting is completed when it is sold—except that he attempts to buy back his own compositions that later seem to him uninspired, which implies that being finished is a property of the artwork itself. Joyce characteristically pushes understanding to its deconstructive limit by claiming that closure is only a transition to recurrent openings, so that there is no such thing as a finish. Although the volume’s concluding essay, by the culture critic Bruce Jackson, hints at the ambiguities created by the multiplicity of frames, the quality of the dialogue captured by the volume could have been sharpened by one more layer of reflexive commentary about that architectonic dissonance.

As Diana Crane observes, the reader of this volume is left desiring a clearer conclusion. Menger and the communications scholar Larry Gross extol the artistic value of fragments, and Becker exhorts contributors to create a collective improvisation. The presentation of the volume follows that style; that is part of the volume’s originality. But to better appreciate dissonance, some degree of resolution is necessary. Members of a jazz quartet, after turns improvising their solos, ordinarily join together in unison to reprise their common theme. There can be no such reprise in this volume, for in effect the contributors are playing in different keys. A bit of reciprocal transposition might be in order, to emphasize commonalities and sharpen contrasts.

Yet because all the contributors are so immersed in their subject-areas, they do combine to reach certain common insights and striking contrasts. Among the commonalities, as Marie Buscatto observes, is the dialectical relation between discipline and creativity. Robert Faulkner’s “shedding culture” can serve as a covering metaphor for this dialectic. Musicians “go to the shed” to practice their scales and “develop their chops,” but also to imagine and rehearse the new moves they

might make to expand their groups’ renditions of standards; at showtime, the musicians must shed their “shed culture” to be in the moment of performance. This is reminiscent of Gimblett’s discipline of “all mind/no mind,” the engineering training that Kagan brings to bear on the development of the sculptural technique to create “object/shadow,” and the way that the apprentice Shona musicians studied by the musicologist Paul Berliner regard performance as the medium of ongoing knowledge, and rigorous practice as the foundation of performative flexibility. Among the contrasts that emerge from comparing chapters are interpretations of the sources of artistic power, sacred or profane. While the Yoruba, in Harris’s account, believe that art is created out of cosmic, metaphysical material, Gross claims that de Kooning’s painted toilet seat, as offered at auction, is “venerated not only because it was touched by the artist’s hand but possibly because it was touched by another part of the author’s sacred anatomy” (156).

Speaking metaphorically—how can one resist the analogy between this volume and a jazz performance?—the contributors have produced something akin to “free jazz.” They have collaborated on a profound meditation about art that suggests topics even more profound. One of the elegant features of this volume is its clever selection of titles—for the chapters as well as for the book as a whole. Becker’s career of studying art as work draws obvious inspiration from John Dewey, who insisted not only on seeing “art as experience,” but also on seeing all experience as—potentially—a form of art, capable of effecting self-realization. “Art from start to finish” (encompassing the Gimblett’s chapter, entitled “This is a stone from the endless beach”) connotes not just the finitude of artistic practice, but human mortality in general. Beyond opening collective reflection on “the artwork itself,” this volume opens a line of inquiry into the nature of beauty and the meaning of life.

REFERENCES

- Majastre, J. O. and Pessin, A., (eds.) 2001. *Vers une sociologie des oeuvres*. Tomes I et II. Paris: L’Harmattan.
- Watson, Walter. 1993 [1985]. *The Architectonics of Meaning*. Chicago: University of Chicago Press.

Howie Becker Responds

It was wonderful to hear three gifted analysts take up the *mélange* that my colleagues and I put together before, during, and after a wonderfully exciting day and a half of discussion. It’s only historical accident that makes me the one to speak on behalf of the entire crew who participated in the making of the book. I was the one who was in Europe at the time of the ESA Culture Network’s meeting. But, of course, I don’t speak for the others involved; they might well have more and different things to say.

We meant the resulting book to be provocative and the discussion shows that it was at least that. We wanted to provoke in the sense of opening up areas for further research. According to Kuhn, science makes real progress when workers in a discipline share a common point of view, limited though it may be, and apply themselves to the same questions, even though those questions don’t cover the field of possible topics.

Buscatto takes up our provocation by noting that the title misleads: it promises to discuss “art from start to finish,” but then insists that there is no finish, that artworks change continuously and never come to rest in a permanent form.

She notes, correctly, that you have to know a lot—not just about sociology, but also about the art you are studying—to do the kind of work exemplified in the book. Most of the authors (and that includes almost all the social scientists) were in fact practitioners of one or another art, and had the kind of detailed knowledge—of procedures and steps in the careers of artworks—she rightly identifies as necessary. And the artists involved in this work were pretty good social scientists. Whether they had degrees to attest to that competence or not, they had the habits of thought and analytic skills necessary for the understanding of social processes. We engaged (it was a notable feature of our meeting) in none of the fussing about defini-

tions of art that arise whenever theoreticians of art, whether they are professional philosophers who make that their business or sociologists who think such decisions are necessary preliminaries to serious research, confront sociological interpretations. We all understood that art is a practical activity, which unfolds just as other practical activities sociologists study do. The artists present understood that especially well since, after all, it is the substance of their daily work.

Buscatto thinks that this is an especially high hurdle to ask researchers to jump, and wonders whether we can expect "ordinary" people who are not "virtuosi" to clear it. Crane raises the same question, noting that doing research on art "require[s] a highly specialized type of knowledge of a particular form of culture, including its vocabulary, its codes, and its standards for style and method. Such knowledge, which most sociologists do not have, can be acquired, but it takes a considerable amount of time." Buscatto doesn't raise the point because she wants to be exempted from the requirement. In fact, she is herself a jazz singer and that's what she studies, writing out of a real knowledge of what she's talking about (Buscatto 2003).

I think, on the contrary, that asking researchers to have some serious knowledge about the kind of art they study represents exactly the kind of minimum knowledge any researcher in any field ought to have. We expect someone who does work in a country not their own to know, more than superficially, the history and politics and culture of that place. And to know, usually, the language as well. We would be suspicious of a study of Italian politics or French cooking or Japanese manga done by someone who had never visited those places and whose command of the language stopped with "Hello," "Thank you," and "Goodbye."

These are not onerous extra requirements peculiar to the study of the arts. Researchers who lack that kind of linguistic and cultural knowledge about whatever they are studying are bound to make interpretive mistakes, to imagine that a particular artistic act "means" this or that when someone familiar with the art knows, or can easily imagine, all the technical and situational exigencies that led to the work happening just that way. Researchers need that kind of detailed technical knowledge to understand, as well, how the work continues to change from day to day, performance to performance, venue to venue. And not just performed works being performed differently, but also physical objects looking different in different settings with different lighting (Larry Kagan's sculptures, displayed in the book, incorporate their own shadows and so have to be displayed in a way that is usually site specific) and literary works varying depending on the way audiences participate in them (the form and content of the hypertexts Michael Joyce, another of the authors in the book, both writes and writes about depend on the choices readers make, so that no two readers read just the same work).

But every collective human activity produces its own local language and culture, and not to know it leaves research open to errors. Donald Roy's deep understanding of the language and activity of workers in the machine shop he studied allowed him to avoid misunderstanding what they were doing, and why they thought what they did was the right thing to do then and there (see, for example, Roy 2006). Many people have written about factories, before and since, lacking that knowledge, and have mistakenly attributed ideas and feelings to workers that

those folks never had. In the same way, critics and aestheticians and social scientists unfamiliar with the day-to-day realities of making art misunderstand what artists have in mind, and attribute ideas and feelings to them that they never had.

So I don't think it is too much to ask of sociologists who study the arts to be deeply at home in what they study. Quite the opposite. It's what we ought to require of any sociologist who studies anything and wants to be taken seriously.

Buscatto remarks on the relative absence of women in the lineup. I want to alibi a little here, pleading that women in this field get disproportionately besieged by requests to participate in symposia and meetings, because there are still relatively few in most fields, which probably accounted for several people we invited regretfully declining. That's not a very good answer, because her own work shows how much we would have profited from discussion of the inevitably gendered nature of work of any kind, especially artwork. But it's true.

Crane worries about whether it's possible to study the careers of works in the way the book's participants propose and suggests many reasons why the answer might be "No, it can't be done." I guess it depends, in the cliché I learned as an undergraduate at the University of Chicago, on how you define your terms. If you only accept as the objects of study specific stand-alone works (like paintings or poems) and define anything else as irrelevant, you leave out performing arts and especially improvisational works in music, theater, and elsewhere. Not to say that improvisation is the touchstone of what's to be studied either, but art of that kind represents a limiting case that talk about the nature of artworks should address if it wants to stretch and get out of the middle of the road. Consider Sonny Rollins' view of what he's doing when he plays (as reported in the article by Scott DeVeaux) or the work of the Shona musicians described by Paul Berliner.

Crane notes that "the majority of the authors in the book are not sociologists." That's right, they aren't, and that was on purpose, so that we could achieve a breadth of cases and approaches and a wealth of detailed knowledge that a meeting of sociologists alone could never achieve.

Nor is it as impossible as Crane suggests to study what's unfinished. Archives do exist, historians are always finding caches of previously unknown documents, and you can create the documents yourself by doing work in the field with artists as they produce whatever they produce. (Scott DeVeaux's 1997 book on the origins of bebop relies on just such a mixture of data.) Artists, often quite self-conscious about the processes and problems of their work, give firsthand testimony.

Crane makes much of the problems of analyzing works that are "incomplete." This is a crucial misunderstanding. The book is not about works that were never completed, it's mostly about works that *were* completed (which is to say that their authors might have, for a moment at least, decided they were) and then kept on changing, so that an analyst could never specify a point at which they were "finished" and now you can talk about them as stable objects. It's not quibbling to insist on the difference between "incomplete" and "never finished." "Incomplete" suggests that there is a plan for the work, which immediately creates the problem that it's often unclear who makes, or has the authority to make, that plan. Cinema is a

notoriously difficult field in which to place the praise or blame, but not the only one. (Sutherland's classic book (1976) describes the deep involvement of Victorian English publishers in the novels of Dickens, Thackeray, Trollope, and others.) The answer to that question has to be discovered empirically in every case, never taken as given. "Never finished" suggests that the state and fate of the object or idea continue to be decided—long after the artist (on whom Crane concentrates) has seen the last of it—by the activities of all the others who deal with it. Latour's (1987) book-length discussion of the fate of scientific findings is a key reference here.

Crane complains (the others as well, to varying degrees) that the book strays from the conventional format for academic books: no unified point of view, no reviews of the relevant literature and, especially, no conclusion. Guilty as charged. Scholarly books take many forms, a time-honored one being the report of a meeting which brings together a variety of people to discuss the present state of work in an area. Our book never intended to be as complete or original or even unified production as, say, the well-known Clifford-Marcus *Writing Culture* (1986), which so jump-started the study of academic writing as activity rather than object, which likewise has no conclusion beyond a short "Afterword" by one of the editors, not at all the kind of theoretical conclusion Crane would like to see. Ours is that kind of book, a report on new ways of thinking about things, not a self-contained, conventionally "finished" work, reporting the conclusions of one thinker.

In the nature of the case, a finished theoretically conventional treatise was never possible. Rather, we intended—in organizing the meeting and producing the book—to suggest a new point of view on an old and notoriously obstinate problem, work on which has not yielded any striking results. (Crane's citations of the French literature are misleading here, since not all the works she cites embody the point of view attributed to them, most do not attempt analyses of particular works, and in any event most of them are not, practically speaking, available to American readers to inspect for themselves). Our book does not debate the possibility of doing such analyses. It just does them, though not always in conventional academic style.

Mark Jacobs best grasps the nature of the enterprise, seeing it as a conversation among people engaged in widely differing activities who found they had something to say to one another, a conversation that would not produce a unified point of view which could be simply stated, but one that pushed around ideas and themes that could furnish the problems social scientists need to organize their work. He is sensitive as well to the variety of modes of expression the book contains, recognizing that the plain style of scientific discourse may be less suited to some topics than more allusive styles common in the arts. It's a long way from the precise analytic discourse of Richard Caves' economic probing of the meaning of "finished" to the mysticism of the painter Max Gimblett or what Jacobs describes as author Michael Joyce's "word games, associations, inversions, and permutations." But, as Jacobs understands, these are all perfectly serious analytic strategies.

Jacobs says, probably correctly, that readers of this book will wish that the ambiguities left by our only partly disciplined discussion were better resolved. Speaking for myself, I don't think that was possible and, in any case, not desirable. The best outcome I could have hoped for was that our unfinished work would be taken up by people in all the fields who contributed to the discussion, who would make its unresolved problems theirs, finding them promising enough to work on.

REFERENCES

Buscatto, Marie. 2003. "Chanteuse de jazz n'est point métier d'homme: L'accord imparfait entre voix et instrument en France." *Revue française de sociologie* 44 (1).

Clifford, James, and George E. Marcus (Eds.). 1986. *Writing Culture*. Berkeley: University of California Press.

DeVeaux, Scott. 1997. *The Birth of Bebop: A Social and Musical History*. Berkeley: University of California Press.

Latour, Bruno. 1987. *Science in Action*. Cambridge: Harvard University Press.

Roy, Donald. 2006. "Cooperation and Conflict in the Factory: Some Observations and Questions Regarding Conceptualization of Intergroup Relations Within Bureaucratic Social Structures." *Qualitative Sociology* 29 (1):59-85.

Sutherland, J.H. 1976. *Victorian Novelists and Publishers*. Chicago: University of Chicago Press.

Promising Culture: Announcing the TASA Cultural Sociology Group www.culturalsociology.org

A Cultural Sociology Group has recently been established within The Australian Sociological Association (TASA). The Group has a general mission to advance cultural sociological analysis and debate. This occurs in sessions at the annual TASA conference, through the scheduling of special workshops and via the domain www.culturalsociology.org. The website provides a focus for the Group's members, graduate students, international fellows as well as other cultural sociologists. The pages profile recent journal articles and books by members and fellows, news of upcoming international conferences, research images and a list of relevant Internet links.

The Group's formation can be understood as part of a broader resurgence of academic interest in culture and the recent spate of its institutionalisation, as evident in the launch of the journal *Cultural Sociology* and founding of the ESA Research Network for the Sociology of Culture. However, in other ways the origins and aims of the TASA Cultural Sociology Group are distinctive to the intellectual environment of Australia. Australian sociology has long been constituted by a number of prominent scholars with a cultural leaning. Many of these are still active within Australian institutions, including Peter Beilharz (La Trobe), John Carroll (La Trobe), Bob Connell (Sydney) and Anthony Elliott (Flinders). Other notable scholars of a cultural bent have gone on to advance their careers abroad after making valuable contributions to Australian academe. Many of these are international fellows of the Group, such as Tony Bennett (Open University), Jim McKay (Durham), Toby Miller (UC, Riverside), Philip Smith (Yale) and John Tulloch (Brunel).

Despite the individual efforts of these pioneers there is not a well established tradition of cultural sociology in Australia. Sociologists with an interest in culture have traditionally either been more strongly tied to international programs and colleagues or domestically aligned themselves with the highly popular interdisciplinary project of cultural studies. As a consequence, there has been little sense of what constitutes or characterises a distinctive Australian cultural sociology.

It is in the face of this history that the Cultural Sociology Group has formed, calling for a debate about the general boundaries and character of cultural sociology in Australia. The Group aims to play an important role in this by publicising the work being undertaken by cultural sociologists within Australia and establishing forums for the sharing and discussion of knowledge. The 'promise' is to develop a distinctive Australian cultural sociology that informs cultural sociology internationally. With cultural sociology's strong suit of theoretical and methodological rigour, this paradigm will provide an intellectual alternative to the dominance of cultural studies in Australia. This is not to argue that there are not productive convergences to be found between sociology and cultural studies. Cultural sociology has always been a meeting place for the ideas of social sciences and humanities. However, we do not consider such interdisciplinary thought to be inconsistent with the aim of establishing an autonomous and strong Australian cultural sociological perspective that demands its due intellectual and policy relevance.

Brad West (Flinders University) and Eduardo de la Fuente (Macquarie University) are co-convenors of the Cultural Sociology

May 4, 2007. Third Annual UCSD Culture Conference. Department of Sociology, University of California, San Diego.

Please join us for this one-day conference that will bring together sociologists who are interested in the study of culture. Keynote speakers **Michele Lamont** and **Katherine Newman** will present full-length papers, while our "State of the Discipline" panelists will discuss the role of culture in their subfields: **Marion Fourcade-Gourinchas** (Economic Sociology), **Richard Madsen** (Religion), **Dawne Moon** (Sexuality), and **John Skrentny** (Race and Ethnicity). **Contact: Stephanie Chan, email: stchan@ucsd.edu.**

For more information, visit: <http://sociology.ucsd.edu/curren/cultureconf07.htm>.