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A Message from the Chair

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The Cultural Turn in American Sociology-- A Report from the Field*

In my last chair's letter I raised the question of what the role of the culture section should be at the upcoming "Question of Culture" ASA meeting. I noted that we will all need to be vigilant in combating false stereotypes of cultural analysis. I also suggested that we should seize the opportunity to communicate with our colleagues elsewhere in the discipline about what the new cultural sociology has to offer. In Atlanta we will have plenty of opportunities to do just that. The incoming chair of the section, Robin Wagner Pacifici, has organized a series of intriguing panels under the theme "Culture in Extremis." In addition to these sessions, Bill Bielby and the ASA program committee have invited a couple of dozen others to organize sessions on various cultural topics (including a panel that I put together on meaning and measurement). Beyond this, thanks to the generosity of Tim Dowd (a.k.a. Timithy J. Down) there will be two mini-conferences held at Emory University immediately following culture day at the ASA (see the invitations elsewhere in this newsletter). One, organized by Tim and Pete Peterson, is on the cultural analysis of music. The other, organized by Tim and myself, will focus on meaning and measurement.

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Beyond Moolight And Magnolias: The Myths of the American South

William G. Holt
University of Connecticut

The American South is a region of dramatic cultural contrasts: black/white, gentility/violence, traditions/development—that are in a state of transition. These contrasts provide fertile ground for cultural sociological research. Historically, the black/white racial divisions impacted every facet of southern life. Until the Civil War's end in 1865, slavery was legal in this region. The 1870's Jim Crow laws established after Reconstruction carried legal segregation into the 1960's. The American South juxtaposes a sense of gentility over a backdrop of violence. Drawing on a Francophile tradition from both Charlestonian Huguenots and New Orleanian Catholics, the Old South elites, about 5% of the white population, lived in a world centered on Greek Revival plantations with cotillions

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Radical Essentialism[1]

Albert J. Bergesen
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Sociology was right about the plasticity of humans as a species. We are profoundly flexible and take on identities that are the product of socio-historical forces—my identity, that I am a sociology professor, couldn't exist as an identity in ancient Rome, and so forth. In that sense identities are socially (meaning out of society/economy/history interactions) constructed. Still no argument. Where we were wrong, though, is our theory of where that plasticity came from. We got the cart before the horse. We got it backwards.

We aren't flexible because of multiple selves/identities, but we have multiple identities because we are flexible. To say "flexible" sounds like Essentialism, and that sounds like

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Clifford Geertz and the Interpretive Attitude Survey

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An "interpretive attitude survey" may sound like an oxymoron, at least as Clifford Geertz would use the word "interpretive" to refer to a search for meaning rather than for scientific generalization. But as developed in his article "From the Native's Point of View: On the Nature of Anthropological Understanding" (*Local Knowledge: Further Essays in Interpretive Anthropology*, 1983), what Geertz regards as most distinctive about his research can occur in much the same way, albeit more modestly, when attempting to understand responses to an attitude survey. We can see this by first quoting from the article and then considering an application using survey evidence.

Geertz starts from the posthumous publication of Malinowski's (1967) diary dealing with his years in New Guinea and the Trobriand Islands. The diary revealed that that eminent ethnographer had not been "a walking miracle of empathy, tact, patience, and cosmopolitanism." Geertz then asks:

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In anticipation of this intellectual feast, I want to again take up the question — what should be the role of the culture section (and its members) at these meetings? In thinking about this issue I decided that the best thing would be for each of us to reflect in some concrete way on what it is that we value about cultural sociology. What would we, as scholars, want to explain to our colleagues about the virtues of cultural analysis? What follows is a rather idiosyncratic list of 10 talking points that captures my own sense of how to respond to such a query.

1. Things go better with culture:

First, I would want to make clear that cultural sociology is not a sub-field of the discipline concerned with a particular institutional sphere (such as the arts, the media, or popular culture). Rather I would describe it as, first and foremost, an approach to sociological work that highlights the human side of social phenomena, which is to say, it is an endeavor that emphasizes the ways in which social life is received and produced through the active agency of human beings who, as Geertz (1973) asserts, are suspended in webs of meaning that they themselves have spun. Cultural sociology is that project which seeks to track the way that people make sense of the world, and how, in making sense, so do they make the world. This reflects a fundamentally constructivist orientation. It sees the world as a meaningful place and it endeavors through its activity to discover what the relevant meanings are, to read and report on those meanings and to use them in constructing an explanatory narrative. No doubt, some will quibble with my formulation, but I think we all share some version of this basic idea. Indeed, members of the culture section have led us to this insight by demonstrating that such widely disparate social phenomena as the rise of Italian fascism (Falasca-Zamponi, 1997), the proliferation of gay identities (Armstrong, 2002), the spread of religious nationalism (Friedland, 2002), the use of money (Zelizar, 1997), the rationalization of science (Evans, 2002), the design of census categories (Ventresca, 2002), the experience of love (Swidler, 2001), or of violence (Cerulo, 1998), the decision to build a dam (Espeland, 1998), to organize a railroad (Dobbin, 1994), to recycle waste products (Lounsbury, Ventresca, and Hirsch, 2003), to launch a police assault on a house in Philadelphia (Wagner-Pacifici, 1994), or a raid on the Branch Davidian compound in Waco Texas (Hall, 2002), are all more clearly understood as matters of culture. Each demonstrates that a cultural interpretation, a reading of the shared meanings, adds critical *explanatory* value. Without it, you will misunderstand the phenomena.

2. We've only just begun:

My second observation follows from this. If cultural sociology provides a critical explanatory resource then non-cultural sociologists are missing out. Cultural analysis (understood as a well formed interpretative endeavor) is an indispensable component of all sociological work that seeks to account for social processes, social dynamics or social orders. Without this tool, sociologists will inevitably produce inferior research. This then leads me to my (admittedly partisan and somewhat grand) assertion that culture is more than a sub-field of sociology, it is a frontier, the leading edge of a broad transformation in how sociologists will go about doing their work. Thus, as my colleague Roger Friedland and I have argued at some length in a new essay (Freidland and Mohr, forthcoming), the cultural turn is not, as some would have it, an intellectual trend that we need to get beyond or pull back from (Bonnell and Hunt, 1999), it is, on the contrary, a paradigm shift in sociology and it is has only just begun to manifest itself.

3. Can't we all just get along?

My third point will require more exegesis. If the second assertion is true, it does not imply that all sociologists are on their way to becoming ethnographers or qualitative scholars (in the traditional sense of that term). There is, after all, as much value added by scientific inquiry as there is by hermeneutic analysis. Both are valid (though different) ways to advance a field of knowledge. We should throw no one overboard. Besides, it wouldn't really make sense to leave the tools of formal analysis behind. They are extremely powerful tools. So, my third point is that the cultural turn will necessarily involve a kind of rapprochement between the hermeneutic and the scientific. To use C.P. Snow's (1959) metaphor, the two cultures will have to learn how to get along and share meaning. This is less idealistic (and less painful) than it sounds. Indeed the biennial "Cultural Turn" conferences (www.soc.ucsb.edu/ct4) that Roger Friedland and I have hosted at UCSB (since 1997) have demonstrated time and again the profitability of such an exchange.

4. Against Truth and Method:

My fourth point is just a refinement of the third but it takes us to the heart of the matter. There is no inherent contradiction between the hermeneutic and the scientific. There are those like Gadamer (1996) who assert that interpretation is an art form and science is something altogether different. I don't agree with this formulation. I think that humanists and scientists do pretty much the same thing, they generate knowledge. They do so by producing systems of discourse that are essentially forms of talk (and dually, forms of practice) shared by groups of people, following certain norms of collective behavior. The difference between the two cultures concerns their relationship to technology, their style of social organization, and the character of their rhetorical forms (on the latter see Chuck Bazerman's comments on Cultural Turn 4, elsewhere in this newsletter).

5. You Can Count on it:

My fifth point is that science can also be of use in the interpretation of meanings. I say this because I think science is a pretty neutral endeavor at its core (though of course every particular incarnation is necessarily loaded to the gills with assumptions) and thus has the capacity to be applied to all manner of things. The part of science that interests me the most is the aggressive use of technology. Think of astronomers; they use signal detection equipment to measure wave particles that the human senses are incapable of perceiving. Analyses of these data (and of the statistical systems that underlie them) are then used to build knowledge systems. No technology, no knowledge. My suggestion is that cultural sociology should also invite technology in. Like the astronomers, we should use what tools we have available to help sift through streams of data taken from the textual uni-

verse. Doing this will enable us perceive the meaningfulness of the world in ways that our embodied senses are incapable of achieving on their own. Of course to do this is a big job. You need to load all of these assumptions into the machinery and then you need to try it out, time and time again. This works best as a community activity, with lots of like-minded others to bounce ideas off of. This community is beginning to find its feet. Look, for example, at Bernard Harcourt's (2002) fascinating analysis of the meaning of guns in gang culture, Vedres and Csigó's (2002) study of political discourse in post-socialist Hungary, Stanley Lieberson's (2000) trend analysis of first name choices, John Martin's (2000a) mathematical deconstruction of Richard Scarry's children's books, Bearman and Stovel's (2000) use of network analysis to study the autobiographical narratives of members of the German Nazi party, Mische and Pattison's (2000) tripartite lattice analysis of the relationship between social movement discourse and collective action, Breiger 's (2000) duality analysis of power and discourse among Supreme Court justices, my own essays on analyzing welfare discourse (e.g., Mohr, forthcoming-a), or any of a number of other intriguing projects (including a whole sub-genre of work by a new generation of organizational scholars — see Ventresca and Mohr, 2002, for a review).

6. *The Raw and the Cooked*:

My sixth point is that all this stuff is still pretty new. It is true that there have been a number of important antecedents. Think of Lévi-Strauss's (1955, etc.) work on the structural analysis of myth, Charles Osgood and his colleagues' (1971) studies on the semantic differential, Roy D'Andrade (1995) and company's research in cognitive anthropology or Pierre Bourdieu's (e.g., 1984) empirical analyses of cultural fields (see Mohr, 1998, for a review). However, the new quantitative work that I just cited (point 5 above) has all come about within the last decade. And while I think that this work is extremely good, it is admittedly primitive and clumsy, primarily because the science has yet to coalesce. In any case, it surely doesn't stack up well against the wealth of interpretative work by humanists and qualitative sociologists. By this comparison, skepticism about the potential contributions of meaning and measurement is surely justified. But I think that this reflects more about the state of the art than about the state of the world (see point 4 above). My recollection of the first meaning and measurement mini-conference (held at George Mason University back in 1995) was that participants had more enthusiasm than results to share with one another and precious few exemplars to point to. The field has come a long way in 8 years. We now have a rich crop of exemplar studies, a regular stream of ambitious young recruits, and a number of recent defections from the realist ranks by senior scholars such as Mike Allen, Peter Bearman, Ron Breiger, Stanley Lieberson, and Harrison White. It is an exciting time for the measurement of meaning.

7. *The Long and Winding Road:*

It is hard to understand what is new about this work without seeing the context from which it emerged. And so my next point is just to say that this new science of meaning analysis represents a break with what went before, even as it contains continuities from earlier projects. I would tell this story by noting that modern American sociology emerged out of a Faustian bargain that was struck with the natural sciences some 40 years ago. It was around that time that quantitative sociologists began to turn away from the formal analysis of meaning and the broader project of cultural interpretation, splitting subject from object like atoms in the new nuclear age. This happened in part as a reaction to the hollowness that characterized early attempts to model "national values" and the surprising discovery that attitudes did not predict behavior (see Smith, 1998). But, as the early history of network analysis demonstrates, it was also the result of an effort to scale research problems down to a manageable size (Mohr, forthcoming-b). The effort to tie meaning to measurement persisted in sub-fields such as political sociology where public opinion matters have continued to be of interest, but with rare exceptions (e.g., Martin, 2000b), these studies have not found their way back into an explanatory science of social institutions. This turn away from formal efforts of interpretation persisted on into the new age of cultural sociology. Pete Peterson (1976, 1979) can be credited with bringing formal analysis back into cultural studies but he did so by stepping around the problem of meaning. DiMaggio's early work followed a similar strategy. His studies on cultural capital (DiMaggio, 1982) made use of various indicators of cultural participation, knowledge and attitudes to mark off levels of cultural resources which he then showed to have tangible effects (as increments to R-Square) without ever making claims about what his subjects thought, experienced, or believed to be true. The new institutionalists (e.g., Powell and DiMaggio, 1991) followed the same path — after laying out an ambitiously constructivist vision of the organizational world, their research focused on the effects of meanings, not their content. Even Bourdieu, in his own way, failed in his efforts to analyze the content of cultural meanings (see Mohr, forthcoming-c).

This is now changing. Today's cultural scientists study the meanings themselves—their shapes, their logical forms, their underlying structures—as well as their effects.

8. *Don't worry, be happy!:*

My eighth point is that these changes will not deprive us of our humanity (or our humanists). The melding of technology and interpretation is nothing new. Language was the first interpretative technology. Writing was another, as was the invention of the library, the printing press, the broadsheet, the fountain pen, the typewriter, the paperback book, the Dewey decimal system, the Xerox machine, the word processor, and the world wide web (to name just a few). One could also speak of literary technologies — the epic poem, the novel, iambic pentameter, the refereed journal, deconstruction, the tenure report. All of these are tools we deploy in the service of interpreting, sharing, and analyzing meaning. My point is that the pursuit of knowledge is never as pure and unmediated as we might imagine and thus the use of formal analysis is less of a radical break in this trajectory than it is another evolutionary step. I expect that within a few decades we'll see literary scholars making regular use of statistical analyses in their readings of Milton and Melville, if only because the technology of reading itself is rapidly moving down this path. In fact, the field of literary computing is already off to a healthy start (e.g., Potter, 1989). But does this mean that

there will no longer be a place for the solitary scholar sitting alone in her study reflecting on the state of the world? I have no doubt that that place will always be honored. I also think that the merger of science and hermeneutics is not going to lead to the de-skilling of humanist scholars. On the contrary, as Paul Attewell (1987) demonstrated, the introduction of new technology into the work place can also lead to an upgrading of skills. Interpretative work may well be facilitated by technology, but it will never be replaced by it.

9. *Reach out and touch someone:*

This brings me back to the start. My ninth point is that the real task before us is to convince mainstream (quantitative) sociologists that their solidly grounded statistical descriptions of the world are partial, inefficient and incomplete. Professing a faith in Berger and Luckmann (1967) is an insufficient response to the stark realities of a socially constructed world. Here is the difference . It doesn't matter that you acknowledge that the world is a meaningful place if you then you go off and either ignore that fact in your measurement process (as organizational ecologists do) or you only go so far as to measure culture's effects (as the new institutionalists have done). If you do this, then you are not using your technology to its full effect. You are running your models with one hand tied behind your back. Is it any wonder that our R-squares have always seemed puny in comparison to our colleagues in the natural sciences? Imagine instead a world where you could never escape the demand to add in that extra explanatory value of cultural analysis, a world where science and hermeneutics are full and equal partners, a world where everything can be and needs to be a matter for interpretation. Tell that to the next positivist you meet. (And if you want to see how to do that with style, see Breiger (2002)).

10. *One step beyond…:*

My tenth and final point would be just to summarize all of this by saying that the cultural turn is coming all right, but it has only just begun to swing on its hinge. The process will not be complete until the scientific side of sociology meets up with the hermeneutic side. It won't be finished until we sociologists have learned all that our colleagues in the humanities have to teach us — about discourse, the ways of metaphor, the power of rhetoric, the logic of narrative. If the world is truly, as Geertz has assured us, a vast web of meanings, then it is hard to see how we can make an effective claim to knowledge about that world until we have mastered both the art and the science of interpretation. We need both skill sets if we are to learn what those webs are made of, how they are spun, how they persist, how they change, how they are used and, and how they structure the institutional lives that we lead. When the pages of the *ASR* are filled with articles whose R-squares are pegged to variations in rhetorical use of social movement activists, the narrative claims of organizational actors, or the metaphorical logics of political agents, when Donna Haraway starts running logistic regressions and Mike Hannan begins to publish interpretative essays in *Signs*, that will be the day that we can start talking about getting beyond the cultural turn. But we have a ways to go before we have to worry about that.

Well, for what its worth, this is the kind of thing I would want to tell my colleagues (both the cultured and the non-cultured) at the upcoming meetings. What about you? What would you want to say?

* Thanks to Ron Breiger, Roger Friedland, Mark Jacobs and Ron Jepperson for helpful comments on this essay.

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Radical Essentialism, continued

the bio-substrate, and that, to the sociological imagination, sounds like fixedness. We cannot conceive how flexibility can be built in, since "built in" rings of biology and that rings fixed responses, which, of course, is the opposite of flexibility. Hence we think it must come from the outside: our multiple social selves/identities, or that we are "socially constructed" to be free, flexible, creative, and so forth. From this view we are empty vessels, or what Essentialism we have is limited to lower order biological functions -- we breathe, eat, and obey low-level stimulus-response processes. But all higher level cognitive and agentic functions derive from socialization/internalization (Durkheim), social interaction (Cooley, Mead), socially constructed realities (Berger and Luckmann), socially constituted actors (J. Meyer) internalized generative mechanisms (Bourdieu) and so forth.

If to say "biology" is to say "automatic response"—what we often mistakenly call "instinct"—then to say "biology" is to say "fixed," and to say "fixed" is to say we don't/can't get flexibility from biology. And that is the epistemological pillar upon which sociology builds its understandings of societal/

individual interactions. But biology, hence Essentialism, isn't only fixed response. Brain is biology too. And brain is mind, and mind is language (Chomsky) and language is the infinite use of finite materials, and that—infinite use—is un-fixedness. Ergo, biology = plasticity, creativity, and open-endedness of human response. From this point of view, the base problem is that our sociological picture of biology is pre-cognitive neuroscience. It is turn-of-the-century behaviorism/pragmatic philosophy, and it's wrong.

Babies don't know the roles they will play or the social formations they will be involved with and so they are prepared to play any of the possible social roles they might occupy, or situations they might find themselves in, or social formations with positions, classes, races, genders they may have to occupy or transform or revolt against. Therefore, we don't come pre-scripted for any particular identity, role, group membership or class position, but we do come constituted (with an innate cognitive endowment of a particular structure and finite sets of mechanisms/operations) to be able to play an unlimited, or virtually infinite number of role, class, status, identity positions. Therefore what is innate, what is essential, is what allows flexibility--agentic creativity and free will--and that is the heart of what is meant by Radical Essentialism.

If human flexibility is indeed rooted in biology (brain/mind mechanisms) then Essentialism is flexibility, choice, indeterminacy, freedom, and free will. And, Essentialism is also agency for the biological is no longer a restraint on human action but in fact its source. It's not that the outcomes of human action are set in the brain/mind, but what is set is indeterminacy itself. That's the key point. Not because our mind is a blank slate, or because we engage in social learning, or internalize "habituses," or mimic, or model (we do some of that, of course, but the point is that isn't the primary way we gain our long noted agentic capacity).

Ironically, we are agentically flexible because we have very fixed sets of mental mechanisms and computational procedures that have the capacity to generate an infinite number of outcomes from a finite number of more basic elements (the mind as a discrete combinatorial system). The hardest thing for sociologists to understand is that if we were truly open, that is capable of learning everything/anything, we wouldn't be as agentically free as we are with our very specific cognitive hard wiring. But remember: it's not outcomes that are hard wired; it's flexibility through very specific innate cognitive mechanisms. What this suggests is that the postmodern insight about the multiplicity of selves, fractured selves, socially constructed selves, contingent meanings, and an infinitely regressing self as signifier, is one that has to have made some assumptions about the mechanics of the mind. While the Chomskyan mind and its innatist assumptions seem the opposite of such postmodern indeterminacy, they are actually required to complete the post modern theoretical project. What seems the furthest from innatism (Postmodern social constructionism) is, in fact, the most dependent upon internalist mechanisms, which are absolutely necessary to realize the human flexibility claimed by the postmodern approach.

It is the postmodern irony: Social Constructionism is dependent upon Essentialism. Dependent is the weak hypothesis. I would say there can be no Constructionism without Essentialism. That's the strong hypothesis. This is because the postmodern model simply cannot work through the old sociological model of learning, socialization, internalization, habituses, definitions of reality and so forth.

In this regard, the interesting thing, I think, about Chomsky is that while he is a radical on political issues, his theory is also quite radical, as he posits a theory of the mind/through language, generates indeterminacy, creativity, and freedom, and if intedeterminate enough, generates the prime postmodern assumption about the unfixed nature of human action. One could combine Laclau/Mouffe on radical democracy with Chomsky on nativist properties of the mind and there would be no contradictions. Ironically, Chomsky is quite critical of all sorts of postmodern theorizing and seemingly adopts a narrow scientism, but it is his very scientistic theory of mind that provides the underpinning for a realistic cognitive science of things like the postmodern self.

FOOTNOTE

[1] For a further development of this concept, see Albert J. Bergesen, "Culture and Cognition" in M. Jacobs and N. Hanrahan (eds.) *The Blackwell Companion to the Sociology of Culture*, forthcoming.

In short, one couldn't have all the flexibility, fracturedness and multiplicity that is claimed for the post modern self without having a Chomskyan theory of mind/language as the cognitive substrate of the postmodern person (for that matter, for any person at any time in history). Most think Chomsky on language is innatism, hence fixed outcomes; but it just the opposite. It is determined indeterminism. It is fixed cognitive mechanisms, but they provide—by their very fixed operation—creativity and freedom in language use, hence in symbolic interaction, hence in relations between person and self, or self and others, and hence between societal forms. And hence, finally, the whole symbolic interactionist and postmodernist projects.

The tragedy of it all is that even though what we know about the working of the brain/mind is the actual bedrock for the relativism of post-structuralist thought, there is little contact between the cognitive science and postmodern literatures. In some ways this is another version of C.P. Snow's two cultures idea; something like "Two Cultures2". One the one side, with all sorts of assumptions about mind/self/agency/consciousness, and not speaking to the other, is cultural studies and the social constructionist theorizing that mind/language is a cultural construction or outcome of social processes (the Durkheim/Mead heritage). On the other hand there is the cognitive science literature, with assumptions about innate mental architecture, modularity of faculties, and so forth, where mind/language is theorized as an innate en-

> sort some sort of extraordinary sensibility, an almost preternatural capacity to think, feel, and perceive like a native...how is anthropological knowledge of the way natives think, feel, and perceive possible?...What happens to *verstehen* when *einfühlen* disappears? (p. 56)

He answers by making a crucial distinction "...between 'experience-near' and 'experience-distant' concepts:

> An experience-near concept is, roughly, one that someone—a patient, a subject, in our case an informant—might himself naturally and effortlessly use to define what he or his fellows see, feel, think, imagine, and so on, and which he would readily understand when similarly used by others. An experience-distant concept is one that specialists of one sort or another—an analyst, an experimenter, an ethnographer…—employ to forward their scientific, philosophical, or practical aims. 'Love' is an experience-near concept; 'object-cathexis' is an experience-distant one. (p. 57)

The question next becomes how the two kinds of concepts are to be deployed in order:

> to produce an interpretation of the way a people lives which is neither imprisoned within their mental horizons, an ethnography of witchcraft as written by a witch, nor systematically deaf to the distinctive tonalities of their existence, an ethnography of witchcraft as written by a geometer. (57)
>
> People use experience-near concepts spontaneously, unself-consciously...they do not, except fleetingly and on occasion, recognize that there are any 'concepts' involved at all...What else could you call a hippopotamus? Of course the gods are powerful, why else would we fear them? The ethnographer does not, and, in my opinion, largely cannot, perceive what his informants perceive. (p. 58)

Instead, the goal is to:

> grasp concepts that, for another people, are experience-near, and to do so well enough to place them in illuminating connection with experience-distant concepts theorists have fashioned to capture the general features of social life. (p. 58)

Geertz then provides examples from societies where he has worked: Java, Bali, and Morocco. In each case, he has not tried to imagine himself as someone else like a rice peasant or a tribal sheikh, but has tried to gain understanding:

> by searching out and analyzing the symbolic forms—words, images, institutions, behaviors—in terms of which people actually represented themselves to themselves and to one another. (p. 58)

Thus Geertz's method is to listen to the experience-near concepts with which people express themselves, then connect these to theoretical (experience-distant) concepts meaningful to anthropologists working to understand the social world. By moving back and forth between the two types of concepts, with each used to elucidate the other, he hopes to arrive at a kind of insight that neither alone could provide.

My thesis is that what Geertz describes so eloquently is close to what the attitude survey does when used to make sense of how a sample of people—Americans, Pakistanis, or others—construe some aspect of the social world. This is not the survey administered to estimate which candidate or which side of an issue has a majority. Instead, it is the attitude survey employed to illuminate the views of ordinary people by connecting their own words to a more abstract set of concepts they would not themselves employ or perhaps even understand. Open-ended questioning is important for this purpose, since it invites respondents to express themselves in their own words. Such an approach was often used by those who first developed the attitude survey some fifty years ago: Paul Lazarsfeld, Samuel Stouffer, Herbert Hyman. I use here an application from the Vietnam period, though the results when inverted also fit the reactions of the American public to the rapid U.S. military victory over Iraq in 2003.

What Americans call the Vietnam War (and Vietnamese call the American War) extended through much of the 1960s and early 1970s. Overall support for the war can be traced by drawing on a basic question that the Gallup organization asked repeatedly to national cross-sections of Americans from 1965, the first year of American bombing of North Vietnam, until the war ended:

> In view of the developments since we entered the fighting, do you think the United States made a mistake in sending troops to fight in Vietnam?

In 1965 a substantial majority of Americans responded that the war was not a mistake, but by the end of the 1960s the proportions had reversed. The issue then became *why* the change had occurred and, more specifically, what did the word "mistake" mean to most people. For many of those living in campus towns like Ann Arbor, Berkeley, or Madison, the answer seemed obvious: the public must have come to realize that the war was not so much a struggle against communism, but a civil war between an unpopular government in South Vietnam and a nationalist movement with widespread support. Moreover, as part of U.S. intervention in what was really an internal war, our military power was bringing death and destruction to the Vietnamese people, north and south. This was the main message of the first "teach-in" held in Ann Arbor in 1965, which I attended, and it grew ever stronger on major campuses as the months passed.

Yet there were signs that such a "moral" conception for opposing the war was not in sync with the views of the larger public. For example, country-wide campus protests peaked largely in response to American military offensives, notably the incursion into Cambodia in 1970; however, the polls seemed to show no loss of public support at such points, but rather at times of apparent U.S. defeats, such as the Tet Offensive launched by the North Vietnamese and Vietcong in 1968. To understand possible differences between what was happening on major college campuses and in the larger public, it was necessary to obtain more clearly "experience-near" concepts with which the general population expressed

itself.

Therefore, in a 1971 cross-section survey I repeated the Gallup question quoted above, but then the 1,263 people who agreed that U.S. intervention was a mistake were asked simply: "Why do you think it was a mistake?" The answers these people gave were recorded verbatim and then coded along several lines in order to capture experience-near ways of speaking. It became evident that most of the public did not use the same words—the same shared symbols—as the campus antiwar protestors or indeed as a comparison sample of University of Michigan sociology undergraduates. One of the main public complaints about the war concerned the number of people being killed or injured, but of all such mentions 73 percent referred *only* to American soldiers; for example, "So many of our boys being killed" or phrases to that effect was a frequent answer. Only rarely (10 percent) did a response explicitly mention Vietnamese at all. The rest did not specify nationality.

Similarly, the campus emphasis on Vietnam as primarily an internal conflict in which the U.S. should not intervene appears in the cross-section survey answers, but with a quite different twist: "Let them fight their own war" was the modal assertion, indicating not that the U.S. was intervening where it had no right to intervene, but rather that the U.S. was being pressed by others to become involved in a struggle that was not its responsibility. The answers were isolationist in tone, rather than having the anti-imperialist implication prevalent among the campus protestors. There were also very few public responses that raised any question about the morality of American goals in Vietnam. Instead, a frequent expression was: "Win or get out."

Overall, the interpretive term "pragmatic" appeared to make most sense of the majority of answers of those in the general public who felt the war a mistake. These people seemed not to oppose the war on principle, but for pragmatic reasons having to do with its failure to achieve the original goals and with the American losses that it entailed. "Pragmatic" in this context is an experience-distant concept, one not likely to be used by many ordinary respondents, but one constructed on the basis of listening to their responses. [1] It is what happens in all instances where an investigator "hops back and forth between the whole conceived through the parts that actualize it, and the parts conceived through the whole that motivates them, [as] we seek to turn them, by a sort of intellectual perpetual motion, into explications of one another." (Geertz, p. 69).

In addition, how people referred to the United States having made a "mistake" by involvement in Vietnam varied between those who spoke of the U.S. as "we" (as in "we should never have gotten involved there") and those who spoke of the U.S. as "they" (as in "they should never have gotten involved there"). It was plausible to infer that "we-sayers" felt more closely identified with the government, even as they disagreed with its policies, and "they-sayers" felt more alienated from the government. This led to the prediction that blacks would be more likely to use "they" than whites, an expectation that when tested was strongly supported. Yet respondents were probably unaware of how their unself-conscious use of pronouns could be reconceptualized into experience-distant words like "alienation." [2]

Of course, the use of responses to a single open-ended survey question cannot aspire to the contextual richness of ethnographic interpretation reflected in Geertz's writing, nor does it concern a standardized cultural occasion like a Balinese cockfight. But the methodological aim, when abstracted from other theorizing by Geertz, is not really so different, and the survey exploration has its own advantages—that of allowing study of sub-parts of the population that express somewhat different experience-near concepts, as in the we/they example. Both interpretive anthropology and interpretive surveys must do a lot of hopping back and forth between the concepts used by ordinary people and the concepts developed by social scientists: the goal in both cases is to explicate social expressions that on their surface are enigmatic, and thus understand what "natives" are up to wherever they may live.[3]

FOOTNOTES

[1] Considering the implications of the results for the military victory over Iraq, the term "chauvinistic" is another experience-distant concept that is relevant: "Militant devotion to and glorification of one's own country" (*The American Heritage Dictionary* 1992).

[2] For a different and more extensive use of pronouns in interpretation, see R. Brown and A. Gilman "The Pronouns of Power and Solidarity," in *Style and Language*, edited by T. A. Sebeok, MIT Press, 1960, pp. 253-276.

[3] The full investigation, "Two Sources of Antiwar Sentiment in America," appeared in the *American Journal of Sociology* 78: 513-536, 1972, with a great deal more detail

Moonlight and Magnolias, continued

and chivalry dictating mores. Most the other whites and slaves eked out lives in absolute poverty living in tar-paper shacks with the threat of lynchings. While many southerners pride themselves on traditions like close family ties and religion, the region's haphazard massive development has destroyed most of the existing communities for strip malls and tract housing. Internationally, Atlanta is recognized for these contrasts: as home of *Gone With the Wind* as well as the birthplace of the American Civil Rights movement and President Jimmy Carter's international human rights work. While Tara was a stage set, you may visit the Martin Luther King Historic Site or the Carter Library. This year's ASA convention will be in Atlanta, a region that exemplifies these cultural contradictions in the American South.

Since the 1960's, metropolitan Atlanta' s population has grown at exponential rates while the central city's population declined. Between 1960 and 2000, the metro popula-

tion exploded from 1 million to 4.3 million, a 304% increase. Concurrently, the city's population decreased from 487,455 to 416,476, a 15% change. During this same period the racial composition changed. From 1960 to 2000, the city's black population increased from 38% to 61% while the white population declined from 62% to 33%. During this same period, the metro area's black population grew from 22.8% to 28.9% while the white population dropped from 77.2% to 63%. In the last decade the international population, both legal and illegal immigrants, has exploded both in the metro area and city. Mexican, Korean, Chinese, and Nigerian neighborhoods are revitalizing older inner ring suburbs.

Without any major natural resources for its location, Atlanta's leaders rely on civic boosterism loosely based on local cultural history for its development. After James Oglethorpe established the British colony of Georgia in 1733, the state developed along the coastal plain from Savannah to Augusta. The Old South economy was based on agriculture with its plantation and sharecropper systems. Northern Georgia became a buffer zone separating white colonists from the Creeks to the south and Cherokees in the Appalachian foothills. Atlanta traces its origins to a settlement that developed in 1833 at the end of the new Western and Atlantic Railroad. By the 1840's, this rowdy frontier town's new leaders began lobbying to lure the state capital from Milledgeville. General Sherman's "Burning of Atlanta" in 1864 destroyed a relatively small city. After the Civil War, Atlanta's leaders decided to move from an agricultural to industrial economy. Championed by civic leaders like *Atlanta Journal* editor Henry Grady, the New South movement organizers launched regional marketing campaigns and visited major northern cities to attract new investors. Events included the 1895 Cotton States Exposition, a world's fair-type event, most remembered for Booker T. Washington's *Atlanta Compromise* speech.

Atlanta's cozy relationships between government and business elites promoted many studies such as Hunter's Regional City and Stone's urban regime theory. Segregation created two separate systems. Whites managed both the political and business communities while black clergy controlled their community serving as intermediaries. In the early 1960's, young blacks began to question this system. As the city became majority black between 1960 and 1970, Atlanta elected its first black mayor, Maynard Jackson, in 1973. While blacks viewed winning city hall as a major symbol of change, Jackson aligned himself with the business community by ending a janitorial strike impacting the convention industry. This action began the splintering of the black civil rights voting block. Later mayors Andrew Young and Bill Campbell would follow similar patterns. This new black regime based out of the Morehouse/Spellman College axis and Southwest Atlanta, one of the country's most affluent black neighborhoods, does not need the working class black votes to win election. On the surface, Atlanta's Mayor Shirley Franklin, the city's first female black mayor, coupled with a white lesbian chairing the Atlanta City Council provides a symbol of multicultural diversity. In reality, Franklin is the handmaid of the black regime while the white business community pulls the strings.

Atlanta's leadership has a long history of stretching cultural history into boosterism campaigns. While slogans can be hollow, they manage to deliver. In the 1960's at the height of the Civil Rights Movement, Atlanta's boosters declared it was the "City Too Busy to Hate" capitalizing on the fact that the city did not experience the bloody riots of Birmingham and Little Rock. In the early 1970's, the Atlanta Chamber of Commerce proclaimed Atlanta as the "World's Next Great International City" based primarily on Delta's once a week non-stop service to Mexico City. In their drive to be recognized as a national and later international city, leaders developed a "laundry list" of cultural activities from an opera house, symphony hall, museums, pro sports franchises, NCAA Final Fours, Super Bowls, the 1988 Democratic National Convention, and culminating with the 1996 Summer Olympics.

The naming process for public spaces provides insight into the region's cultural conflicts. Civic leaders changed the city's name three times. The Western and Atlantic Railroad called the original settlement Terminus because it was the end of their line. In 1842 residents changed the name to Marthasville for then Governor Lumpkin's daughter. In 1845, the Georgia Railroad's chief engineer suggested the name Atlanta, believed to be the feminine version of Atlantic, because the other name seemed too colloquial.

If you expect to see peachtrees at the ASA conference, you'll need to go to Macon or South Carolina to find an orchard unless it's one on a mug or t-shirt. There are about 70 places with peachtree in the name. The use of this fuzzy fruit as icon dates back to white settlers' mistranslation of the Cherokee settlement, Standing Peachtree, they encountered in 1782. The Cherokee word *pitch* refers to the numerous pinetrees in the region. Actually, Georgia leads the nation in peanut production, but don't expect to see "Goober State" license plates anytime soon.

Street names change constantly in the city. Some were changed because they honored people whose actions are deemed offensive today such as the street named for General Bedford Forrest who reorganized the Ku Klux Klan. The new black leadership has renamed streets in a retelling of city history emphasizing their own families. Mayor Jackson changed Houston Street to John Wesley Dobbs Avenue who was one of twenty-five men responsible for developing the Auburn Avenue business district. No effort was made to rename a street for the former female slave whose cabin once stood at the intersection of Peachtree Street and Auburn Avenue. Others were changed for commercial reasons such as Cain Street to International Boulevard. Another example is Peachtree Center Circle that bisects the two ASA conference hotels. Originally named Ivy Street after a family farm existing prior to the city's formation, the Atlanta City Council renamed it after developer John Portman requested his new Marriott Marquis hotel to have the cache "peachtree" in its address to attract higher land rents.

Life outside the Perimeter, the I-285 beltway, is another world from the city. Only one out of four people in the metro area are natives. Beginning with the 1960's desegregation, old native Dixiecrats moved into the suburbs. By the 1970's, Atlanta became a rustbelt haven for white ethics leaving places like Pittsburgh, Detroit, and Cleveland. The new Republican leadership like Newt Gingrich, from Pennsylvania, and Ralph Reed, a military brat, coalesced in these new edge cities. Here, any development is viewed as good. According to NASA landsat studies, Atlanta lost 30% of its tree canopy

between 1970 and 2000. Paving, sod lawns, and rooftops create a heat island effect so powerful that storm systems divide over the metro area, exacerbating droughts in the summers. Suburban counties like Gwinnett, model themselves after Orange County, California. Ironically, their newest center, Mall of Georgia, forty-five miles from downtown reimagines the state in four themed areas from the mountains, Piedmont, plain, and coast. The outdoor "lifestyle" section of the mall is based on vernacular small town architecture. The food court is a re-creation of Atlanta's historic railroad station that was torn down. It's the perfect postmodern ironic experience for tract house residents whose developments and consumption destroyed the real small towns but crave a sense of community.

Atlanta's leadership obsesses over its image. Harkening back to the New South era, leaders view anything local with disdain as they pursue internationalism. Those who question these imagined myths are quickly shunned from Tom Wolfe for *A Man in Full* to academics like Charles Ruthheiser. Being from the American South, I was asked by a fellow academic what I thought of the movie *Deliverance*. Considering that this question came from an acquaintance in another department while we were waiting in the Connecticut College student center canteen line, I was caught off guard. I pondered the situation. I wondered if he would be offended if I asked him what he thought of the *Sopranos*. Raised as a southern gentleman, I replied, "I haven't seen that movie in years." Then, for some reason, I had a craving for a ham sandwich; but I settled for the New London grinder.

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Conference Reports

Culture in Social Movements: Report from the Culture Working Group at the Authority in Contention Workshop (CBSM mini-conference held at Notre Dame, Aug 14-15 2002)

Ann Mische, Rutgers University
David Smilde, University of Georgia

Prior to the ASA in Chicago, the section on Collective Behavior and Social Movements held a mini-conference at Notre Dame entitled "Authority in Contention." The role of culture in political mobilization has received considerable attention from social movement analysts in recent years, with strong overlap between the Culture and CBSM sections. At the CBSM workshop, Lyn Spillman organized a working group to look at the relationship between culture and movements. The working group met for two consecutive days, with a very lively discussion on both days.

Lyn opened the discussion on the first day by raising a historic debate among analysts of culture – should we think about culture as being *constitutive* of social life, or as a *resource* that actors draw upon instrumentally? This question is particularly relevant for the study of social movements, in which activists are often described as "mobilizing" such things as frames, identities, and symbols as they attempt to recruit participants, persuade the public, and confront opponents – thus treating culture as a resource to be manipulated (on par with other resources, such as funding, networks, or organizational infrastructure). David Snow, one of the fathers (with Rob Benford and others) of the "framing" approach in social movement analysis, wanted some clarification of this question: can't culture be both? Several people argued that certainly it can be both, but we should be careful to not go too far in either direction. We should not see culture as completely constitutive, with no room for strategic maneuver; but we should also not see culture as something existing outside of actors, as a "thing" to be deployed. Francesca Polletta, for example, warned us against seeing culture as too unanchored and free-floating; culture imposes constraints on action as well as creating opportunities. Gary Fine reminded us of the importance of groups and interactions in cultural process, and of the need to look at how culture is constitutive of social settings and contexts. Ann Mische talked about how culture constitutes social relationships, but is also often used as a tool in relation-building, so that it can be both constitutive and strategically deployed.

Several people stressed the processual character of culture. Rob Benford reminded us that framing is a process, not just a "thing," sparking some discussion of the multiple levels of culture. On one level, culture can be seen as being constituted by "elements" (such as symbols, identities, frames, narratives, or genres) that can be studied as "objects." On another level, however, we are interested in the ways that such cultural elements are caught up in actions – for example, boundary-work, frame-making (or breaking), story-telling, dialogic interaction. On a third level, we are interested in how these processes contribute to the generation, reproduction, and transformation of larger social formations: for example, social relations, networks, communities, institutions, and fields. But this happens in response to problems, insisted Colin Barker, arguing that culture is driven by dialogue among actors as part of a problem-solving process, rather than being driven by its own abstract logic.

The group then took up the question of the relation between culture and authority, the theme of the larger CBSM workshop. Gary Fine suggested that we look at how authority is generated in social practices that allow some people – but not everyone – to do what they want. We also need to look at how such cultural authority constitutes what we *can't* do – that is, it provides limitations on how we are allowed to solve problems. David Snow and others suggested that this takes us back to ethnomethodology – we need to ask what is unimaginable in a given context. What is the process by which things are ruled out of order? Francesca Polletta pointed out that there are some things that we can't even think, and some things that we can think, but which we actively reject. Studying social movements is important because it shows us the conditions under which such constraints on imaginability change, so that what was previously unthinkable becomes seen as possible. There was a suggestion that we should look at words in the context of status relationships – for example, we can examine whose topics get caught up or repeated, whose words don't get recognized or echoed, which ideas are seen as important while others are not. Several people offered other possible ways of thinking about culture – as a process of meaning-making, as constraining and enabling, as a field of negotiation, debate, and play, as a "feel for the game," as fragmented and inconsistent. At this point Colin Barker said he was tempted to cite Raymond Williams: "I wish the word culture had never been invented!"

This exclamation was followed by a appeal to stop looking at what culture is, but rather to focus on how to study it. We spent the remainder of the session talking about possible research strategies and their tradeoffs. For example, how do we locate frames? How do we know if what people tell us in interviews actually reflects the way they talk about things in other contexts? Is the interview situation a barrier to understanding culture? How do we incorporate the situational aspect of culture? How can we use observation to complement what people verbally tell us? How do we know what the repertoire or frame is before we arrive on the scene? How do we know what can or can't be said? Just as we were embarking on these thorny issues, the first session came to an end.

On the second day, the culture working group discussed different aspects of the relationship between culture as an authority structure and culture as a catalyst for challenges to authority structures. Analysis of the first relationship presupposes that "authority" doesn't have to be a social "entity" but can be a cultural model. Rob Benford suggested that such cultural authority structures can work in subtle ways; for example, movements often end up reproducing the cultural structures that they are trying to change. Activists cannot stand outside of dominant culture; they are bounded by the same linguistic filters. As analysts we have the same problem. Other participants in the discussion had less trouble here. John Krinsky argued that sure there are linguistic and cultural constraints, but activists work and innovate within the boundaries. Lyn Spillman suggested that movements always reproduce some cultural structures but not others. The key is for movements to reproduce the ones they want to further or which they consider irrelevant, and to alter the ones they seek to change.

Inspired by a panel presentation the previous day regarding subversive religion, Jeffrey Broadbent spearheaded a discussion about the moral component of culture. Jeff suggested that the idea of a moral core, a moral rock upon which people found their lives, was necessary for successful activism. He argued that in his work on activism in Japan, only those activists with a strong moral core were able to resist cooptation. Activists with a strong moral core act as the engine that pulls the train. Many participants objected to the notion of a moral core as being impossible to define in a non-circular way. Broadbent suggested that you get at it through a discursive analysis of what shared values are being used. Pressed for defining elements, he suggested belief in an afterlife as one possible primal mover. He added that in his study of eight movements, the only successful ones had a moral core which consisted of a sense of commitment to the group and a deep moral commitment to the issues.

Others argued that even such moral core is constructed through culture. Lyn suggested that the idea of a moral core is problematic inasmuch as research generally shows that values are the most changeable aspect of culture and often are not engaged at all. Marc Steinberg objected to the idea of a moral core, arguing that culture is not "out there" but rather is a relation between people. Values are situationally and relationally produced and "culture" should be considered a verb. This prompted others, in agreement, to say that we have to move beyond the idea of a unitary self. Others countered that there are vocabularies of motive and that if you gave ten different reasons for something, you would lose legitimacy for being inconsistent. This is one reason that social movements frequently try to have some internal control regarding defining statements: they have a clear sense of right and wrong vocabularies of motive. Others suggested that it would only be delegitimizing if the different claims were mutually contradictory. David Smilde suggested that the idea of a moral core and relationality might not be mutually incompatible, since the core activists are most likely to be at the confluence of networks of other activists with diverse reasons for participating and therefore will make more abstract moral claims and use less restricted codes.

The session ended with a discussion of the oft-heard complaint that the term "culture" is ambiguous and frequently ends up encompassing almost everything. Is there anything that is not cultural? John Krinsky suggested that Jeff Broadbent's idea of a moral core suggested something that was pre-cultural and unconstructed. Death, the prospect of which Broadbent had seen as a main source of morality, is indeed beyond culture. Lyn Spillman pointed out that indeed "culture" is frequently opposed for definitional purposes to biology. Others proposed that death may be biological, but its impact is always understood through culture. Several participants pointed out that a lot of the definitional bogs surrounding the term culture can be avoided by simply focusing on sub terms like "discourse," "frames," or "narratives," or by focusing on symbolic acts. Others argued that cultural analysts would do well not to get too wrapped up in the issue, as the same can be said about any sub-field of sociology. Almost every social phenomenon has an organization, is characterized by inequality and has gender dimensions, yet this doesn't mean that the

sociological study of organizations, gender and stratification are somehow hopelessly ambiguous.

I May Be Strange, But I am No Stranger-- Cultural Turn 4 at UCSB

Charles Bazerman, Education Dept., UCSB

I have been asked to comment on what it was like to be a disciplinary outsider, a humanist, at the Cultural Turn 4 conference of sociologists. But I hardly felt the outsider—I felt more like someone approaching a homeland he had only imagined.

Writing is a social act—that has become a slogan in my field, the teaching of writing, over the last two decades, and I flatter myself to think that my 1983 review essay "Scientific Writing as a Social Act" helped establish that slogan. I do know it was the first attempt to systematically apply sociological literature—in this case, from the sociology of science—to understanding what writing does socially and how social forces help shape the forms writers adopt. Since then a number of us interested in writing (including my good friend Joanne Yates who was a plenary speaker at CT4) have been looking into social theory and sociological research to help us understand writing. But as we have looked to situate writing within society, we have also been seeing how writing is a key site of social action and how the typified forms writing gravitates toward (genres) are important social-structurating elements, particularly in modern knowledge-saturated, documentary, bureaucratic, credentialed, educated societies.

Thirty-two years ago I received a degree in English Literature specializing in poetry of the British Renaissance and I have spent most of my career in English Departments, until my recent move to a school of education; nonetheless, for a long time I have imagined I have been doing a kind of applied sociology. Even in graduate school, I was already asking sociological questions, like why did people write and publish books at a particular time and place, why did people read them, and what were the social consequences of these strange practices. And the answers were sociological as I investigated poems at a time of political transition—the death of a queen and installation of new king. The poems were motivated by the class interests of the writers; the styles were stratified by social position and profession; and the consequences had to do with systems of alliance and patronage and the rearticulation of national identity and loyalties under changing conditions. When I soon turned my commitment from literature to literacy studies, particularly the academic literacies my students struggled with, I began to understand writing as situated and meaningful within institutions of schooling and academic disciplines.

My connection with sociology became explicit when I started to study scientific writing in the late seventies and I participated over several years in the seminar run by Robert Merton and Harriet Zuckerman and I became a member of the Society for the Social Studies of Science. At that time sociologists of science were exploring how scientific writing was part of the sociality of science, and that publications were not pure statements of knowledge, but contentious social actions in a field of conflict. Bruno Latour, Steve Woolgar, Karin Knorr-Cetina, David Edge, Michael Mulkay, Michael Lynch, Harry Collins and a mere slip of a lad Trevor Pinch (also a plenary speaker at CT4)—from our different perspectives we all began opening this Pandora's box of epistemological specters, as Mulkay and Gilbert framed the issue. But as a specialist in writing with a practical commitment to understanding how language worked, I found different issues in the sociality of writing. I began to see how writers addressed perceived social situations; how forms of writing organized actions and relations; how historical developments of communicative forms and forums went along with historical developments of community, authority and social practice (see *Shaping Written Knowledge*). My studies into the formation of the scientific communities around text production, textual forms, systems of distribution, and systems of evaluation then led me to look at the textual organization of other disciplines and professions. And eventually I began looking into the relation between academic fields and other areas of social practice, such as finances, government, and politics (see *The Language of Edison's Light*).

As a writing teacher, I was ever aware that each act of writing involved orientations to definitions of situations, complex choice making, and the formation of mind, identity, and commitment in interaction with emerging text. I found phenomenological and micro- interactional sociology with their view of the evanescence of social experience spoke eloquently to the complex interactions mediated by texts between writer and reader. But writing changes the equation of social communicative relations typically studied in face to face interaction. Texts travel through distance and time, they endure in physical artifacts, and they are frequently reproduced for multiple readers. The need to make texts intelligible across multiple and distant occasions also increases the pressure for conventionality and typification. All this suggests that texts may provide some mechanisms by which societies gain regularity and order over times and places. One way to consider genres, for example, is to consider them as social institutions. And one way to consider the effect and evaluate the success of texts is to consider them as speech acts that constitute social facts—an uncontested bureaucratic file constitutes the social facts of a person's relation to an agency; any text that does its work successfully serves as a landmark to be taken into account by all who follow in its penumbra.

I was getting ever more convinced that not only was sociology useful for understanding writing, and that writing studies might be of use to sociologists—but that in fact writing and sociology were deeply tied together. Consequently, I hung around sociologists ever more, particularly once I arrived at UCSB a decade ago. I attended several of the previous CT conferences, made friends with micro-, meso-, and macro-sociologists, and took part in many interdisciplinary activities.

But while I was gaining a faith in sociology, only the sociology of science folk among sociologists, as far as I was aware, had shown much interest in writing—and that only from a somewhat limited perspective and for most attention had shortly turned elsewhere. However, in the past decade I have seen a new sociological interest in texts. Dorothy Smith and her cadre of institutional ethnographers follow the texts by which institutional control of daily life is made operational.