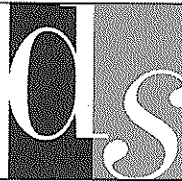


On theories and models



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ABSTRACT Starting from the assumption that the ability to see patterns and thus abstract from actual events and properties of specific objects is universal, the article reviews different conceptualizations of and attitudes toward the terms 'theory' and 'model', identifying two co-existing and opposing tendencies: the love for details (originally praised by Franz Boas) and the attraction to generalizations that can cover a wide range of phenomena. Using as a backdrop seven theses here reproduced in the Appendix, the article also examines the implications of taking the notion of interaction as primary in the analysis of language and culture. Such implications involve the need to identify useful units of analysis and evaluative measures for competing theories and models.

KEY WORDS: *Boas and cosmography, cultural models, interaction, models of and models for, unit of analysis*

Prelude

When my son Marco was five and a half I took him to his first piano lesson. I sat on a couch while the young teacher started to explain the arrangement of the keys. 'You see,' he said, 'there are three black keys here and then two black keys over here and then another three and two, and three and two.' Marco turned away from the keyboard to look up at the teacher and, in a very matter-of-fact tone, said, 'It's a pattern.' The teacher smiled, nodded, and moved right away to the next item on his agenda, 'Mary Had A Little Lamb', a classic first song for beginning piano players in the US.

There are many lessons about things other than piano keyboards that can be drawn from this little story, but one is paramount: humans are able to both produce patterns and recognize them, even at an early age, and language can

articulate our recognition of patterns by describing them and confirming our perceptions and intuitions. We could say that, as social scientists, we go around searching for things that look like piano keyboards. We look for generalizations like the 'three and two' pattern of black keys against the background of white keys. We do find such patterns occasionally but they are not as obvious. We cannot always be as quick as Marco was on that first lesson. For one reason, instead of things like the white and black keys of pianos, we are more likely to find entities with different shades of the same color, half-finished, and often half-hidden. In the world of verbal interaction, human cognition, and cultural interpretation, three-and-two patterns are rare, and social actors as well as the students of their actions must confront the ubiquity of variation, the recurrent violation of expectations, and the incommensurability of alternative interpretations that make human life first, and its scientific interpretation, second, what it is: a conundrum we are condemned to only partly solve. And yet, we do what we do – regardless of whether we call it anthropology, linguistics, sociology or something else – because individually and collectively we feel compelled to go against the odds and make assertions that can be used as generalizations and even predictions. Like piano players, we see patterns on whatever 'keyboard' (metaphorically speaking) we happen to be playing, but differently from piano players, we cannot assume that a B flat (one of the black keys) will remain in the same spot while we move up and down the scale. Furthermore, we soon realize that we are not all looking in the same way, we are not all searching for the same answers, and we do not all start from the same place or stop at the same point in our pattern recognition quest. This is due to the fact that our epistemologies vary, in part, because our ontologies are different.

Historians of science have shown again and again that ambiguity cannot be completely avoided and one can never get to the bottom of all the presuppositions that are behind a given research program. Peter Galison (1999) has argued that ambiguity, in the form of *trading zones*, can be a positive force in allowing the exchange of ideas and the co-existence of different scientific paradigms. Journals, laboratories, and research centers (such as the UCLA Center for Language, Interaction and Culture) do make it possible for scholars to focus on shared interests instead of epistemological differences. There is, however, a price for ambiguity. We risk forgetting what we were trying to accomplish in the first place and even letting others decide what we stand for. By publicly declaring the logic of our enterprise and the differences and similarities with other enterprises, we can be better judges of our own efforts and be in a position to ask ourselves questions like: Do we have a program? What is it like? Can others *adopt* it? *Should* they? What kinds of *skills* does it require? *How* are those skills acquired? *Can* they be taught? What kinds of *results* are our programs likely to produce? To what projects are they *applicable*?

I do not think we can even start answering these questions without establishing some basic common ground. As a first attempt at creating such a common ground, I decided to flesh out a few basic principles that could be used

as points of departure for discussion. I did this in the form of seven theses, which were circulated among the participants of the symposium, *Theories and Models of Language, Interaction, and Culture*,¹ and made available on the Center for Language, Interaction, and Culture (CLIC) web site (they are here reproduced in the Appendix). I want to use this article to review and elaborate on some of the points I made in these theses while examining the meanings of 'theory' and 'model'.

The primacy of interaction

The first thesis I proposed is called 'the primacy of interaction', that is, the idea that interaction is the presupposed ingredient and product of any human affair. In other words, we need interaction to be who we are and, in turn, our ways of being produce further interaction.² The primacy of interaction is closely linked, as a starting assumption, to the centrality of a project I have been involved in for quite some time. If I had to give it a title, I would call it *an interactional theory of authorship* (Duranti, 1986, 1988, 1993, 2003, 2004), which includes intentionality and participation, two related and yet distinct dimensions of human life.

Unfortunately, there are several misunderstandings regarding what interaction really means and, consequently, there are misunderstandings of the extent to which it plays a role in human affairs and how it should be documented. It is not uncommon to hear claims that in some social situations there is no interaction between speaker and audience-addressee or that the interaction is so minimal that it can be ignored for the purposes of analysis. Despite the popularity of Bakhtin's idea of 'dialogic imagination' in the past two decades (Bakhtin, 1981; Holquist, 1983; Todorov, 1984), the view of communication as the public 'airing' of ideas that started out as privately conceived and individually owned is too powerful to be ignored. I redundantly call this view *monologic solipsism*. It is a set of beliefs and related interpretive practices that are deeply embedded in, and reinforced by, myriad social institutions, including state bureaucracies and the educational and legal systems, with their associated ideologies and practices of literacy. But past and present work by any of the speakers today could be used to convincingly argue that interaction precedes, accompanies, and follows any communicative act and therefore the formation of our sense of being. Speakers are constantly evaluating their about-to-be-uttered words vis-à-vis their about-to-be-addressed audience. The audience is always part of the message even before it *does* anything (and it always does do something). The notion of *recipient design* introduced by conversation analysts in the 1960s gave us a powerful analytical tool to examine some of these phenomena. Taking advantage of video-taped interactions, the range of phenomena to be investigated under such a notion was expanded by Charles Goodwin, who meticulously demonstrated the continuous adjustment that is done, even within the same utterance, as speakers and addressees aurally and visually negotiate their partnership in meaning-making (Goodwin, 1979, 1981).

Although Goodwin's data and analysis make it more difficult to believe that it's all happening in the speaker's mind, someone might still argue that since the individual speaker is, after all, the one who is doing the thinking, rethinking, and the speaking and repairing, whatever interaction takes place is reabsorbed, filtered, and ultimately reduced to monologic discourse, i.e. the speech produced by the individual speaker, who must take responsibility for his or her own individually owned and expressed intentions. I am tempted to use an oxymoron for this position: *dialogic solipsism*. The existence of dialogue is in this case recognized, but authorship is still managed, controlled and claimed by the speaker as an autonomous and free agent. Individuals edit their own original ideas into new ideas that take into consideration a variety of contextual factors, including the reaction of co-conversationalists, but they (and those who analyze their talk) continue to believe that authorship of inter-actors can be individually assigned.

I could further expand on the misunderstood nature of interaction, but I hope that this provides a glimpse of the sweeping impact that a rethinking of communication from the point of view of a truly interactional perspective would have. In fact, an important question to address in this context is whether we can distinguish between interaction and communication at all, regardless of how they have been defined in the past.

Now I want to move on to some critical tropes sometimes invoked in the discussion of any approach that privileges interaction.

The 'affective impulse' toward details

One of the implications of the focus on interaction is a relentless interest in details. All the authors in this special issue spend a great deal of time detecting and describing details of what is said or done, details of the material-technological environment, details of human relationships, details of cultural assumptions and even personal history (well, we do not all do this all the time, but we do it often enough to identify with such practices as part of our methodology).

In this interest, or rather passion for details, we are no pioneers. A love for details of human culture(s) is, of course, part of the very history of science, the humanities, and the social sciences included. But the stance with respect to details varies within different theories and models and the interest in details is often placed in opposition to the interest in sweeping generalizations. Franz Boas, one of the founders of US anthropology, was not a fan of grand theorizing and, not by accident, early on in his career came to the defense of details in a paper published in 1887, where he borrowed Wilhelm von Humboldt's concept of 'cosmography' to set up a dichotomy between two types of researchers, the 'naturalist' and the 'cosmographer'. He wrote:

Losing sight of the single facts, he [i.e. the naturalist] sees only the beautiful order of the world. The cosmographer, on the other hand, holds to the phenomenon which is

the object of his study, may it occupy a high or a low rank in the system of physical sciences, and lovingly tries to penetrate into its secrets until every feature is plain and clear. This occupation with the object of his affection affords him a delight not inferior to that which the physicist enjoys in his systematical arrangement of the world. (Boas, [1887] 1940: 645)

For Boas, the cosmographer's (read 'ethnographer's') interest in details was due to an affective impulse, to be contrasted with the naturalist's 'aesthetic impulse'.³ Here Boas uses 'aesthetic' to mean elegant, orderly, and with a logic of its own (a meaning that was also adopted by his student, Edward Sapir, in writing about the logic of grammar).⁴

It would be easy to argue that such a distinction is no longer recognized and that Boas's dichotomies have been reframed in different terms, such as, for example, the micro- vs. macro-contrast in sociolinguistics or in sociology (e.g. Alexander et al., 1987). We also know that the love for details is not necessarily divorced from the search for 'laws' if we rename them 'rules' or just 'generalizations'.

But Boas's dichotomy seems surprisingly close to a contrast that continues to surface in contemporary scholarship. It is the opposition between those researchers who claim that they know what they are searching for (e.g. cooperation, conflict, identity-formation, motivation, repression, power differential) and those who supposedly dive into their data looking for a phenomenon they might have had no previous interest in or knowledge of (see Schegloff, 1996) or a phenomenon that they thought they knew but, on further investigation through particular types of data (e.g. audio-visual recordings of spontaneous interaction), turns out to be different from what is remembered or imagined by participants, experts, and observers (e.g. in my work on Samoan greetings, see Duranti, 1992, 1997a, 1997b; and more examples can be found in the work of any of the contributors to this special issue).

The usual criticism of members of the first group (the theory-savvy researchers) is that by setting up an agenda (or simply formulating a question) before looking at their data: (1) they are unable to see what else is going on, besides what they are looking for, i.e. cooperation, conflict, identity-formation, motivation, repression, gender or class or ethnic differences, etc. (Schegloff, 1997); and (2) they will always manage to find what they want to find, i.e. examples of cooperation, conflict, identity-formation, motivation, repression, gender or class or ethnic difference, etc.

The usual criticism of the second group (the purposely naïve analysts) is that: (1) even though they claim to start their analysis without previous expectations, preconceptions, and theoretical assumptions, in fact, they do have all or some of the above, but they keep them hidden⁵ (maybe even to themselves) (e.g. Wetherell, 1998); and (2) they may fall prey to what Bourdieu called the 'occasionalist fallacy', that is, the belief that each occasion is created anew (Bourdieu and Wacquant, 1992: 144n).

Like many stereotypes, these contrasts cannot capture the complexity behind

them. Our collective experience tells us that there is no homogeneous category of researchers who perfectly slot into one camp. Even the most inductive among us start from certain assumptions about how an interaction is likely to proceed and what elements constitute the nuts and bolts of any face-to-face encounters. The very process of transcription, out of which 'data' are constituted for the analysis of much of what we do, is, as Elinor Ochs (1979) pointed out, a theoretical enterprise, in which we put on paper or on a computer screen what we are trained to hear and see and we leave out what we cannot hear or see, or we cannot *imagine* as being relevant (see Thesis No. 4 or the inextricable link between analysis and documentation). Even the most interaction-oriented researchers are likely to encounter old dichotomies or essentialist categories at work in the field – I am thinking here of the interplay between hierarchy and gender documented by Elizabeth Keating (1994) in Pohnpei or Ben Rampton's (2003) rediscovery of 'class' in the talk of London adolescents putting on exaggerated 'posh' and 'Cockney' accents. Here it is appropriate to quote Mary Bucholtz and Kira Hall's (2004a: 375) remark that we need 'to distinguish between essentialism as a theoretical position and as an ethnographic fact'.

On the other hand, even the most theory-driven researchers, those who start out by accepting a series of principles that are meant to give meaning to human interaction (e.g. Grice's maxims, see Levinson, 2000b), may have become convinced of the utility of such principles after extended exposure to, collection and analysis of interactions in a variety of cultural contexts (e.g. Brown and Levinson, 1978). And those who engage in experiments do not just find what they want to find. They also must deal with counter-evidence or things that just don't fit in their encoding scheme. It is situations of this kind that bring about revisions of previously proposed universals (e.g. Levinson, 1996, 2000a, 2003).⁶

In general, contra to Boas's claim that the aesthetic impulse is restricted to those looking for 'laws' or exception-less generalizations, those engaged in collection and analysis of details also get pleasure out of elegant description or from analysis-for-its-own-sake. Which brings me to my next question: what is analysis? Or what is *an* analysis?

Units of analysis and evaluation procedures

This is a question that can only be answered if we are ready to commit to (1) some particular units of analysis (Thesis No. 5 or the commitment to explicit units of analysis), and (2) evaluative principles (Thesis No. 6 or the need for explicit evaluative principles).

Units of analysis abound in the study of language but research traditions vary in terms of the control over the units to be used. In some traditions, there is more control on the definition of the units to be used and on their application. Examples of such traditions are logic, with the notions of formalization and axiom, structuralist and generative linguistics, with the notions of sentence and

its constituents, conversation analysis, with the notion of turn or turn-constructional-unit, speech act theory, with the notions of locutionary, illocutionary, and perlocutionary act, and, more recently, the experimental work done by the Language and Cognition Group at the Max Planck Institute for Psycholinguistics, with the notion of frame-of-reference (FOR) (e.g. Levinson, 2003).

In other traditions, there is a looser fit between units and analysis. For example, the interpretation of 'speech event' within the ethnography of speaking ranges from single speech act (*à la* Searle) to complex rituals (e.g. Sherzer, 1983) and political meetings (e.g. Duranti, 1981). The notions of *habitus* (Bourdieu, 1977, 1990), is another example of a notion that has been hard to pin down, especially given the issue of the consistency of its philosophical roots (see Throop and Murphy, 2002). Ochs and Solomon (2005 and in this volume) are rare cases in which Bourdieu's theoretical concepts are carefully scrutinized and tested on the basis of video-recorded interaction between autistic children and their families and friends.

A general problem with these notions might be the fact that those who proposed the unit of analysis did not themselves engage in detailed analyses that could be used as models of their theoretical framework. In the case of Hymes's (1972) notion of speech event, he let his students deal with the messy business of dividing up the world into speech event units – what about those parts of interaction or discourse that do not fit in such units? – and in the case of Bourdieu, in his analyses he oscillates between traditional structuralist methods (e.g. his analysis of the Kabyle house, Bourdieu, 1970, reproduced in 1990) and traditional sociological methods such as surveys based on questionnaires (e.g. in Bourdieu, 1985).⁷

An interesting question in such cases is whether the vague or ambiguous meaning of certain units of analysis is strategic. Another interesting question is whether the identification of tight control that I provided above for certain research traditions is absolute or relative. What appears consistent within a given tradition (or paradigm) is seen as inconsistent or problematic outside of it. For example, conversation analysts and linguists inspired by conversation-analytic insights and findings have replaced the notion of sentence provided by generative grammarians with the notion of 'turn constructional unit' because it is more appropriate, in their view, for the analysis of conversational data, e.g. it accounts for the fact that certain expressions routinely occur as complete(d) turns even though they are not considered full sentences (Ford and Thompson, 1996; Ford et al., 2002; Schegloff, 1996).

The concern with identifying units of analysis and agreeing on some form of evaluation of the analyses provided by researchers brings us back to a discussion that Chomsky (1965) defined in terms of what he called *descriptive* and *explanatory adequacy*.

Faced with the task of analyzing a strip of interaction of various dimensions and complexity – e.g. a threat, a promise, an exchange of greeting, a narrative –

we need to ask ourselves whether we have the right analytical tools to describe such a phenomenon. Do we have the right units? Do we have the right methods for collecting data that correspond to such units? Or, conversely, do we have units that can capture the full range of phenomena that our data could reveal?

The very notion of an adequate description is associated with an evaluation measure and such a measure is, however hidden, theory-driven, regardless of whether those engaged in description are aware of their own theoretical assumptions and implications. General criteria should be identified to assess not only whether a given description is valid (as opposed to misleading or mystifying) but also whether it is better than other *possible* descriptions. Such evaluative criteria may include: (1) the ability of a given description to provide a characterization of the phenomenon in question so that we can easily differentiate it from others, whether or not intuitively similar, phenomena; (2) the capacity of an analysis to offer generalizations; and (3) the potential to offer a measure of comparison of phenomena that appear different but turn out to be the same or appear the same but turn out to be different.

In order to know whether we have a good description, we also need to be explicit about what kind of information we want to provide a description of, and the conditions that would make the identification and collection of such information satisfactory. In generative grammar, the goal of description is to represent the knowledge that native speakers have of their own language. In the analysis of interaction, the goal cannot be limited to accounting for knowledge. It must also include an account of what participants *achieve* by using particular linguistic expressions and engaging in particular sequences of acts, whether linguistic or kinetic. In other words, the centrality of interaction presupposes an interest in the kind of 'work' that interaction gets done or that is done through interaction. Crucial in this endeavor is the dimension of *temporality*. We do not just ask 'why this feature/expression/act/turn/exchange/activity etc.?' but 'why this feature/expression/act/turn/exchange/activity etc. *now*?'

Given such an orientation, what kind of data do we need? Here I think that we haven't even started to have a real conversation about the relationship between data and theoretical orientation. Many researchers tend to be borrowers and lenders of data but the implications of the exchange system haven't been assessed yet. What is considered the 'same topic' can be constituted by very different sets of data, gathered through very different methods and for different purposes. For example, if we look at the study of greetings, a phenomenon that has attracted the attention of researchers in a number of social sciences for quite some time, we find a vastly different range of documentary techniques, including: (1) an exclusive focus on the items or expressions that are said to be used in a given language (Ferguson, 1976); (2) a description of a greeting exchange based on informants' characterizations (the Tuareg greetings analyzed by Youssouf et al., 1976); (3) a collection of (fairly idealized, 'cleaned') verbal exchanges with no reference to manner of documentation (Milton, 1982); (4) a collection of the non-verbal behavior of people greeting each other (Eibl-

Eibesfeldt, 1968); (5) a collection of greetings based on discussions with native speakers and recollections of what the researcher observed over an extended period of field research (Irvine, 1974); and (6) a collection of audio-visual recordings of people engaged in a particular type of greeting activity, supplemented by extended observation of and participation in the same activity as well as discussion with native speakers (Duranti, 1992, 1997a).

Do all these techniques provide us with the material on the basis of which to provide a descriptively adequate account of how greetings are performed (for a particular group, situation, activity/event)? There is no easy answer without some theoretical discussion about our goals and without an understanding of the model of interaction or communication that we subscribe to.

Theories and models

Theories and models are often confused or at least used interchangeably in the social sciences and humanities. In linguistics, for example, as David Crystal wrote, the term 'model' 'is often used in the sense of "formal representation of a theory"' (1997: 245).⁸ In anthropology, 'cultural theory' is sometimes used in opposition to 'cultural model'. D'Andrade (1995: 151, 173), for example, uses 'cultural theory' to talk about what the natives think and say about a particular domain of knowledge or social life (e.g. religion, politics, food consumption) and uses 'model' for the representation made by the ethnographer of what natives seem to know or do. But other scholars use 'cultural models' in place of 'cultural theory', as shown in the following quote:

Cultural models are presupposed, taken-for-granted models of the world that are widely shared (although not necessarily to the exclusion of other, alternative models) by the members of a society and that play an enormous role in their understanding of that world and their behavior in it. (Quinn and Holland, 1987: 4)

But not all cultural anthropologists agree that cultural models are 'widely shared' (see the work in 'socially shared cognition', e.g. Resnick et al., 1991; and in 'distributed cognition', e.g. Hutchins, 1995) and there are those who question the idea that all cultural models are *mental* models (Shore, 1996: 45ff.).

The ambiguity and lack of agreement on the distinction between theories and models in linguistics and anthropology are further amplified by the fact that many researchers have conflicting or negative feelings toward the very use of 'theory' or 'model'. Perhaps thinking that it is risky, presumptuous or in many cases premature to speak of 'theory' or 'model', many scholars adopt alternative terms such as 'framework', 'approach', or 'dimensions'. This preference has important implications. First, because it exposes the fact that just as not everything that is a 'theory' or 'model' for some may turn out to be a theory or model for others, there might be more 'theories' and 'models' around than we think. They may just be hidden under other names. Second, it shows that there is an ethics and politics of theorizing and model building, which has in fact been the

object of criticism by a number of scholars. For example, Donna Haraway (1989) discusses what she and other feminist scholars (e.g. Fedigan, 1986) see as the gendered aspects of models of human evolution. Some of the contributors to this special issue have been involved in a more recent debate over the way to talk about gender and sexuality (e.g. Bucholtz and Hall, 2004, this issue; Cameron and Kulick, 2003; Kulick, this issue). We can thus ask: who owns theory and modeling? And, if so, on which grounds and to what ends?

These questions lead us straight into the issue of what constitutes a theory and what constitutes a model of language, interaction and culture, or, would we be able to recognize a theory or a model if we saw one?

Theory

Let us think of a theory as a set of propositions, preferences, and attitudes toward a particular set of phenomena or entities. We are thus always engaged in theory-building whenever we are concerned with the logic of our research, the paradigm-in-practice that we use, or, rather, that we *must* use in order to get to talk about anything (I am using 'must' here in both the epistemic and deontic sense). Hence theories include an epistemology, that is, what we think we *can* know and what we think we *should* be able to know. And theories also include an implicit or explicit ontology, particular assumptions on the nature of the phenomena we are trying to understand and a set of associated practices (i.e. way of implementing those assumptions and, in turn, reinforcing them). How are these phenomena constituted? What makes (or does not make) them accessible, i.e. knowable? How are they filtered by our own actions, i.e. to what extent do we enter into the constitution of such phenomena as data?

For example, in talking about language, some believe that there are such things as 'sentences' and 'words' out there (e.g. in someone's brain or in the way in which a computer program can recognize them). Does it matter?

We have beliefs about whether rules have a psychological reality and, if so, of what kind. We may also have a stance as to whether meaning is always defined in terms of speaker's intentions (Dennett, 1987) – plus or minus whatever conventions are being used to make those intentions, or some aspects of them, available to the addressees – and, if so, what to do with meanings that do not easily fit into an intentional stance perspective (Du Bois, 1993; Duranti, 1988, 1993; Ochs, 1982; Rosaldo, 1982). We also may have something to say about whether, when working on larger discourse units such as stories or narratives, those units exist for the people who produce them in ways that are comparable or matchable to the ways in which we understand them through our analytical apparatus (i.e. we are concerned with whether, as Bourdieu said, we are making our subjects into people like us; see also Sahlins, 1995). I would consider concerns of this kind as pertaining to our theory. Hence theories must make room for all kinds of questions and issues. They are thus rather spurious and not particularly elegant entities. For elegance to have a chance at all, we must turn to 'models'.

Models

Let us think of models as entities that are good to think-with. They are worth pursuing if they provide us with a conceptual apparatus that can be used to describe, and thus (better) understand or explain a given range of phenomena. Models are often thought of as *representations* but only in the very general sense of 'standing in' or 'standing for' the phenomena themselves or the logic of their functioning. As I will briefly discuss below, there are different types of models.

Despite Boas and other past and contemporary 'cosmographers', there is a long tradition of model-building or model-searching in anthropology as well as in sociology, psychology, and linguistics. From Max Weber's 'ideal type' (1949), a utopic construct to be used as a heuristic device to handle the inherently variable nature of socio-historical situations, to Lévi-Strauss's use of models as instrumental devices to freely manipulate features of experience in order to arrive at 'rational' and thus 'eternal' truths (Remotti, 1971).

Models are especially popular in cognitive anthropology where, not surprisingly, they are conceived of as representations 'of what happens inside people – in their minds, or psyches – that results in their doing what they do' (D'Andrade, 1995: 158). Echoing some of the characteristics of Lévi-Strauss's models, cognitive anthropologists' models are seen as heuristic devices for problem-solving (by the natives *and*, I would add, by the ethnographer):

A model consists of an interrelated set of elements which fit together to represent something. Typically one uses a model to reason with or calculate from by mentally manipulating the parts of the model in order to solve some problem. (D'Andrade, 1995: 151)

A classic example in cognitive anthropology is the Caroline Islands navigation model (Gladwin, 1970), but also the 'restaurant script' studied by developmental psychologists (e.g. Nelson, 1981) and by computer scientists interested in simulating human problem-solving strategies (see Schank and Abelson, 1977). A number of these analyses start from the assumption that we should be thinking of the brain as an information processing system, something akin to a computer.⁹

But models of human behavior do not always have to be representations of what goes on in the mind (Shore, 1996). Proponents of socially shared cognition or distributed cognition have argued that models need to take into account the interaction between the individual and the environment, including other individuals involved in a joint activity and the material resources used to achieve a given goal (Hutchins, 1995; Lave, 1988; Resnick et al., 1991). Let me give you an example. For you to decide where to go to sit in a room where a conference is being held, you must interact with the material artifacts available in the room and take them as providing you with a series of alternatives that have a meaning even though it is difficult, if not impossible, to trace such a meaning back to someone's intentions or mental representation. The people who physically

arranged the chairs (if they are not bolted to the floor), or those who gave the directions of how to place them, do not have to have a mental representation that captures all the subtle indexical meanings that choosing one seat over another has for any one of you, whether or not you are aware of it, including (1) different degrees of (culturally defined) *proximity* to some privileged person or location, such as the speaker as well as other relevant-others (e.g. acquaintances); (2) different degrees of (culturally defined) *visibility* in relation to present and future speakers; (3) different types of access to the (path that leads to the) exit in case the event turns out to be a real bore.

More generally, any time we examine interactions that make use of cultural artifacts, including new technologies (Keating, this issue), we are forced to rethink communication as something more than an exchange of thoughts from one individual (or group) to another. We must return once again to the centrality of interaction and its role in generating possibilities and therefore meanings. With this concern in mind, Herb Clark (1996) developed a theory of language as a form of joint action, in which collaboration plays a central part, whether we are making a promise or placing some money on a store counter before paying for something (Clark, 2003, and this issue).

We should thus be able to distinguish between models (or aspects of models) that constrain the boundaries of the inquiry (a constant preoccupation for linguistics and much of social sciences at least since Durkheim, e.g. [1900] 1960) and models that are open and allow for the expansion of existing boundaries.

Types of models: models of and models for

In addition to 'models of', there are also 'models for' (Creager et al., forthcoming; Geertz, 1973: 93-4). Cognitive models, such as mathematical models (and computer models) tend to be 'models of'. Examples abound: Chomsky's (1965, 1982) theory of syntax, Shannon and Weaver's (1949) model of communication, etc.

The standard examples of 'models for' are the organisms (e.g. inbred strains of mice) that biologists have used since the mid-20th century to predict how humans might react to certain environmental factors or to new drugs (Creager et al., forthcoming). But there are also plenty of 'models for' in the social sciences, the humanities, and the arts, especially in the form of metaphors.

In musicology, for example, scholars such as Berliner (1994, 1997) and Monson (1997) have been using 'conversation' as a metaphor to understand what jazz musicians do when playing together. Keith Sawyer (2001) reversed the metaphor and used improvisation (as in improvisational theater and jazz performance) as a metaphor for conversation. As Kenny Burrell and I documented throughout our course on the culture of jazz aesthetics (Duranti, Burrell and Flood, 2005; Duranti and Burrell, 2004), jazz musicians also use terms like 'language', 'conversation', and 'dance' as metaphors to explain to novices or

people who are not familiar with jazz what improvisers do and how they can manage coordination with the rest of the band.

What these examples illustrate is the need that people have to stretch the semantic and pragmatic boundaries of linguistic expressions in order to capture complex concepts and activities (Lakoff and Johnson, 1980; Lakoff and Turner, 1989). One of the advantages of 'models for', like all metaphors, is that they have a life of their own which frees them from our own original assumptions. We can explore the model in ways that are more adventurous than the ways in which we can explore something that we control very tightly. One possible generalization here is that there might be a tendency for 'models of' to be more constraining and closed areas of inquiry and a tendency for 'models for' to be more open-ended frames of inquiry.

Another type of model mentioned by historians of science is the 'case studies' as used in psychiatry and psychoanalysis – a distant relative of Boas's 'cosmography':

Cases, like model systems, are valued for their specific material reality, their uniqueness, and at the same time their typicality. Cases, it is assumed, capture individuals in all their complex uniqueness while at the same time rendering them in a generically analyzable form. (Creager et al., forthcoming)

Although there are plenty of 'case studies' in linguistic anthropology or discourse analysis, they are not treated as 'models' in the ways in which case studies have been theorized in disciplines such as psychiatry. I think this is a connection that is worth exploring.

For socio-cultural anthropologists, there are also native models that are readily available for the ethnographer to document and study. They are, in other words, public. The paramount case is the ritual – a point repeatedly made by Clifford Geertz (1973, forthcoming). The ritual was for a long time and continues to be for many ethnographers any of the following: (1) a meta-structure; (2) a micro-cosmos of the entire socio-cultural system (Geertz); (3) an X-ray of a culture's preoccupations, obsessions, pleasures, and fears (Turner); and (4) a testing ground for anthropology as a whole, including such abstract and abstracted dichotomies as hierarchy and equality, autonomy and relatedness, collaboration and competition, tradition and innovation (see Ortner, 1984).

But if one takes rituals to be models of society or culture (or both), then we would have to admit that models surround us (from the morning coffee to graduation, from checking e-mail to visiting relatives for Thanksgiving) and we risk drowning in them, or, rather, we risk not being able to tell them apart.

It is my belief that this and the other issues that I raised in this article need our close analytical attention if we are committed to making the interface among language, interaction and culture a fundamental debate within the humanities and the social sciences.

APPENDIX

Language, interaction and culture: seven theses

THESES 1 OR THE PRIMACY OF INTERACTION

Among the many, potentially infinite approaches to the study of human action as revealed by and realized through communication, we focus here on perspectives that share a theoretical and empirical commitment to interaction as the presupposed ingredient and product of any human affair, from the most public (e.g. a loud argument in the middle of an open market) to the most private (e.g. the silent problem-solving we engage in when we try to pour water without spilling it). We believe that human action (from the lifting of a spoon in preparation for testing the content of a dish to the declaration of war) always involves some form of interaction and that therefore any model, theory, and method aimed at explaining or simply describing what humans routinely do in the course of their everyday affairs must have some way to include 'interaction' as a dimension that needs to be referred to, theorized, and empirically reckoned with, e.g. 'captured' or 'inscribed' through some form of documentation (see Theses 4 and 5). We 'interact' with other humans as well as with the natural and cultural environment. Whether or not these interactions are of a different kind and have different prerequisites and implications is a potential object of discussion.

THESES 2 OR THE RECOGNITION OF THE HISTORICITY OF CURRENT TERMINOLOGY

We are aware of the importance of the terms used in our discussion and, in particular, of the arbitrary choice of the triad 'language, interaction and culture' including the long and debatable history of the three terms with which we are starting the present discussion. Two of the terms, in particular – 'language' and 'culture' – have been claimed, criticized, renamed and debated by a number of disciplines and sub-disciplines within the humanities and the social sciences (another arbitrary and debatable distinction). Such debates have increased our understanding of the objective complexity and of the inter-subjective indexical value of concepts such as 'language' and 'culture', while simultaneously creating a terminological ambiguity and, in some cases, a terminological vacuum, including a series of avoidance behaviors (e.g. how not to use the word 'culture' in talking about 'cultural' phenomena or how to avoid the word 'language' in talking about 'linguistic' phenomena). 'Interaction' has not had the same intensive critical scrutiny (outside of the criticism of 'interactionism' in sociology) perhaps because of its taken-for-granted or, alternatively, its under-analyzed status in past and current debates (including theoretical debates on contemporary socio-ethical-political issues).

THESES 3 OR THE PROBLEMATIC RELATION TO ESTABLISHED DISCIPLINES (E.G. PSYCHOLOGY AND SOCIOLOGY)

Any empirically founded study of human interaction must attend two needs: (1) the need to refer to or take into consideration human consciousness – which

includes intentionality as well as human awareness and self-understanding – and (2) the need to avoid assuming a theoretical-methodological stance exclusively or even primarily founded on cognitive processes (assumed to be lodged within the (individual) mind. These two needs occupy themselves with realms of study traditionally associated with well-established disciplines, psychology and sociology in particular. Working within one of these traditions presents advantages and disadvantages, including those generated by having to enter an already established discourse, rich in presuppositions, methods, and beliefs – some of which we do not share.

THESES 4 OR THE INEXTRICABLE LINK BETWEEN ANALYSIS AND DOCUMENTATION

Any type of 'inscription' (including 'transcription') both presupposes and (re-) creates the conditions for particular types of analysis. The recognition of this link comes with different kinds of commitment. We may have a commitment to a continuous updating of our documentary techniques depending on the type of phenomena we want to capture (phenomena → technique) or a commitment to experiment with different techniques to see what kind of phenomena they can reveal (technique → phenomena). Most of the time, there is an interaction between these two commitments, although the effects of the interaction may be more or less immediate (e.g. time-wise) and more or less perceptible by the researchers themselves.

THESES 5 OR THE COMMITMENT TO EXPLICIT UNITS OF ANALYSIS

Among those working on interaction (regardless of the methodological and theoretical emphasis), there is a wealth of units of analysis. In some traditions (e.g. conversation analysis) there is a considerable social-institutional control on the units to be used. In other traditions (e.g. ethnography of speaking, cognitive anthropology), the units are more varied and they may change inter- as well as intra-subjectively. In some authors and approaches, there is even a sense of avoidance of formal commitment, with terms being used without any rigor. Students (and readers) must thus reckon with such lists of historically and semantically related terms as: language, speech, speaking, discourse, talk, talk-in-interaction, conversation, dialogue, exchange, exchange type, turn, speech event, speech situation, speech act, activity type, linguistic practice, style, genre, register, participant (or participation) framework. We must first commit to making our units of analysis explicit and then to assess their potential for cross-contextual, cross-cultural, cross-linguistic analysis.

THESES 6 OR THE NEED FOR EXPLICIT EVALUATIVE PRINCIPLES

Among students interested in an interactional approach, there is a recurrent call for clear statements regarding the criteria for the acceptance or rejection of a given analysis. For example, we need to say what constitutes evidence for asserting that feature *f* does *a* or is used to accomplish *a*. Criteria that have been used include the fact that: (1) something happens a number of times in a given

set of data; (2) it appears under similar (contextual) conditions; and (3) it is followed by a restricted and hence predictable set of phenomena (e.g. acts by participants, features of their talk). We need to establish whether these are necessary and sufficient criteria. We also need to clarify the nature of the relationship between what we call the 'conditions' (see 2) and 'consequences' (see 3) (e.g. whether we are implying causation).

THESIS 7 OR THE REFLEXIVITY OF INTERACTION

Interaction is a dimension that exists not only in the world of 'data'. It also exists in the process whereby data are constituted. There is, in other words, a familiar and recurrent interaction that takes place between researchers and the material they work with. This 'material' includes other humans (those whom experimental psychologists call 'subjects') and the documents produced by researchers for their study, including transcripts. The discussion of this order of interaction includes the role of particular forms of documentation and the type of analysis that they afford (see Thesis 4) as well as the role, conditions, and effects of the relationships established with the people we study. Reflexivity transfers the omnipresent political and ethical dimensions of human affairs to the context of researchers' choices.

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NOTES

1. The symposium, *Theories and Models of Language, Interaction, and Culture*, which is discussed in the Introduction to this special issue, was held at UCLA and hosted by CLIC on February 27, 2004.
2. After reading a first draft of this article, Teun van Dijk raised the issue as to whether by proclaiming the primacy of interaction I am implicitly assigning to it a special status in the study of language use or discourse (a position that he would object to). Is 'interaction' more important than 'meaning', 'understanding', 'form', 'order', or 'strategy' (to limit myself to some of the terms mentioned by van Dijk)? But for me the issue is not whether interaction should replace terms like 'meaning' or 'structure', but whether we really understand its place in the constitution of individuals, practices and institutions, just to mention some of the common loci of social and cultural inquiry.
3. According to Boas:

While physical science arises from the logical and aesthetic demands of the human mind, cosmography has its source in the personal feeling of man towards the world, towards the phenomena surrounding him. We may call this an 'affective' impulse, in contrast to the aesthetic impulse. ([1887] 1940: 644)
4. Silverstein (1986) develops a complex argument about the relationship between

Boasian 'cosmography' and Sapir's research program, which, in Silverstein's view, attempted to reconcile empathetic descriptivism with a universalist-typological agenda.

5. According to Wetherell:

Schegloff argues that analysts should not import their own categories into participants' discourse but should focus instead on participant orientations. Further, analytic claims should be demonstrable. Schegloff's notion of analytic description uncontaminated by theorists' categories does not entail, however, that no analytic concepts whatsoever will be applied, as the example of his own analyses demonstrates. Rather, concepts such as conditional relevance, for example, or the notion of accountability, or preferred and dispreferred responses are used to identify patterns in talk and to create an ordered sense of what is going on. (1998: 402)

6. Written texts, that is, articles and books, are misleading with respect to the role that theoretical concepts and existing models have for researchers because they often contain no obvious traces of their construction over time. We know that references to theoretical concepts or discussions of a particular theoretical claim or generalization are often added *after* an analysis is completed or close to completion, sometimes in anticipation of possible objections by colleagues and journal reviewers, other times to counter specific criticism encountered after submitting a manuscript for publication.
7. The few (and brief) analyses of human action that are better examples of Bourdieu's theoretical apparatus are anecdotal or so decontextualized that one has no way of knowing where such mini-vignettes originated, like the following characterization of a waiter's habitus:

His body, which contains a history, espouses his job, in other words a history, a tradition, which he has never seen except incarnated in bodies, or more precisely, in the uniforms inhabited by a certain habitus that are called waiters. This does not mean that he has learned to be a waiter by imitating waiters, constituted as explicit models. He enters into the character of the waiter not as an actor playing a part, but rather as a child imitates his father and, without even needing to 'pretend', adopts a way of using the mouth when talking or of swinging his shoulders when walking which seems to him constitutive of the social being of the accomplished adult. (Bourdieu, 2000: 154)

8. David Crystal traces the first use of the term 'model' in linguistics to Charles Hockett in a discussion of models of description in morphology (Crystal, 1997: 245).
9. According to Dennett:

The inferences we attribute to rational creatures will be mirrored by physical, causal processes in the hardware; the *logical* form of the propositions believed will be copied in the *structural* form of the states in correspondence with them. This is the hypothesis that there is a *language of thought* coded in our brains, and our brains will eventually be understood as symbol manipulating systems in at least rough analogy with computers. (1987: 34; emphases in the original)

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