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# Anthropology, Interviewing, and Communicability in Contemporary Society

by Charles L. Briggs

Developing an anthropology of interviewing could provide a rich focus for ethnographies of the contemporary and illuminate how anthropologists' modes of knowledge production intersect with practices that play crucial roles in the media, corporations, electoral politics, state bureaucracies, and a wide range of professions. Interviewing is informed by ideological constructions of discourse production, circulation, and reception, communicable cartographies that are widely shared by anthropologists and nonanthropologists. The capacity of interview-based texts to project maps of their purported sources, processes of encoding, modes of circulation, recipients, and legitimate modes of reception naturalizes interviewing, simultaneously imbuing interviews with power and shielding them from critical scrutiny. Analyses of David Stoll's attack on the veracity of *I, Rigoberta Menchú*, Américo Paredes's critique of ethnographic work on Mexican-Americans, and the author's interviews with Venezuelan women convicted of infanticide illustrate this process. An anthropology of interviewing has potential for illuminating such issues as the spatialization and temporalization of ethnography, the doubling of ethnography "in the field" and at "the desk," questions of scale, the science wars in anthropology, and the ways in which anthropologists mirror and are mirrored by other "expert" knowledge makers.

for Feliciano

Alas, issues of wide relevance to anthropology and the politics of contemporary life are trivialized when contained within the interview subgenre of the marginal genre "field methods." This domain is predominantly inhabited by those senior scholars who feel obliged to bequeath their memories of "the field" to future generations. The "poetics and politics of ethnography" (Clifford 1988; Clifford and Marcus 1986; Marcus and Fischer 1986) and subsequent work reframed rhetorics of "the field," once treated as elements of a professional toolkit, vis-à-vis critiques of professional authority and techniques for reproducing colonial, racial, sexual, and other inequalities. The interview seems never to have made it out of the domain of "mere methodology."

A challenge I issued 20 years ago to engage an anthropology of the interview as a means of facing issues of power and representation in fieldwork, *Learning How to Ask* (Briggs 1986), has been widely used in classes, and many anthropologists have told me that it helped them develop a more

complex understanding of interviewing. Nevertheless, it failed to provoke much anthropological research on interviewing (but see, e.g., Mertz 1993; Trinch 2003). Sociology was able to build on foundational critical works by Hyman et al. (1954) and Cicourel (1974) in generating sustained critiques of interviewing (see Gubrium and Holstein 1977, 2002, 2003), and sociologists have developed a field of "qualitative research" that takes critical understandings of interviewing as a central analytic focus (see Denzin and Lincoln 2005; Seale et al. 2004; Silverman 2004 [1997]). Although researchers aligned with conversation analysis and ethnomethodology eschew interviewing as a methodological device, interviews, surveys, news interviews, and other question-response modalities (such as doctor-patient interactions, social service encounters, and help-request calls) have provided an important source of data (see Clayman and Heritage 2002; Heritage and Maynard 2006; Houtkoop 2002; van den Berg, Wetherell, and Houtkoop-Steenstra 2003; Matoesian 1993; Maynard et al. 2002; Roulston 2006). Feminism fostered some of the most sustained critiques, exploring gendered dimensions of received interview techniques and considering alternatives (see De Vault 1999; Smith 1987). Anthropologists, however, seldom focus sustained, critical attention on how their interviewing practices produce subjects and objects, texts, and authority or what they might learn by making interviews and their place in society an object of anthropological inquiry.

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So why bother? I explore several reasons here. First, interest in interviews has mushroomed in the media, capitalism, and politics, with the result that many people now live in “interview societies” (Atkinson and Silverman 1997). Interviews are conducted and their results inserted into countless arenas of everyday life. Following Foucault (1997), Holstein and Gubrium (2003) argue that interviewing has become a panopticon—part of schemes of knowledge/power, surveillance, and discipline. Children now learn to conduct and write up interviews as part of the literacy skills taught in elementary school, and their teachers interview them as a part of their pedagogical practice. Our telephones ring with requests to participate in surveys; I get numerous requests from the American Anthropological Association, other professional associations, and my university each year to respond to surveys. A standard feature of the built environment for sites of consumption is the comment-card box. Supermarket cashiers are less likely to proffer us a formulaic “Have a good day!” than an equally formulaic microinterview: “Did you find everything okay?” I am not suggesting that all of these exchanges can be equated or that they are all interviews; my point is, rather, that examining why such interrogatory practices have come to be construed as a natural, normal, and effective means of producing knowledge and shaping social relations in such a broad range of institutional and mediated settings would seem to provide a fascinating focus for anthropological inquiry.

Second, anthropologists have discovered the value of exploring similarities, overlaps, and mimetic relations between modes of knowledge production in anthropology, other scholarly endeavors, professions (science, medicine, business, the media, etc.), and everyday practices. I argue here that interviewing is a key point of convergence between anthropologists and marketing and media consultants, reporters, writers, NGOs, and nonprofessionals in many countries, including the United States.<sup>1</sup> Ethnographic research on how interviews are conducted, displayed, and interpreted can therefore provide an important locus for researching anthropology’s position in relationship to other forms of knowledge making. In order to explore these relations, I wander somewhat promiscuously in these pages between interviews conducted by anthropologists, other scholars, the media, and other professionals in order to reveal these convergences. I would like to make it clear in advance that I am not equating surveys, opinion polls, news interviews, talk shows, and other phenomena with anthropological interviews, which are themselves internally heterogeneous. My goal is rather to identify both similarities and differences between question-and-answer-based modes of producing knowledge in a wide range of contexts and to explore the communicative ideologies that underlie these di-

verse social forms and imbue them with power. Indeed, by failing to cross the self-announced boundaries of these diverse knowledge-production practices, I would reproduce the assumptions that seem to imbue them with uniqueness, authority, and authenticity.

Third, anthropologists are often not the most sophisticated producers and consumers of interviews. Politicians, corporate officials, NGO representatives, writers, and some academics are now professionally trained to “work with the media,” and interviewing figures importantly here. Media trainers are on salary in many institutions, teaching elites how to maximize the chances that they will “get their message across.” These contrasting orientations help to point out the different ways in which interviews are ideologically constructed, the understandings of communication, interaction, knowledge, “the public,” and so forth, that inform and sustain these interview ideologies, and the creation by both interviewers and interviewees (along with researchers, editors, and producers) of discourse that seems to embody “natural” modes of self-expression and public communication. Exploring these gaps can also help us think about different ways to conceptualize interviews, thereby opening up alternatives for conducting, analyzing, and presenting them.

If my goal were simply to provide a better recipe for cooking up interviews, I could not break interviews out of this maligned pedagogical realm and reframe them as ideological constructs, analytic objects, and social practices. My aim is rather to retheorize interviewing in such a way as to shed new light on the ways interviews operate within anthropology, the ways they produce subjects, texts, knowledge, and authority, and their relationships to other contemporary practices. I start with three ideologies that inform our understanding of interviews and the way they are represented. I then provide a theory of *communicability* that explains how these ideologies can make interviewees’ words seem to be transparent, almost magical, containers of beliefs, experiences, knowledge, and attitudes. Next I use this framework in reexamining four cases that reveal the problem of interviewing with particular clarity. The first is the debate about *I, Rigoberta Menchú*. I point to the way a common set of ideological constructions of interviewing informs the text, its critique by David Stoll (1999), and attempts to defend Menchú. The next two examples are cases in which nonanthropologists seem to have developed broader and more sophisticated understandings of the interview than are common among anthropologists. The second comes from Américo Paredes’s analysis of Mexican-American interviewees’ overestimation of the communicative sophistication of the anthropologists who studied them and the latter’s scientization of stereotypes. The third case observes the training of professionals in what are marketed as more sophisticated understandings of interviews in order to help them convey their “message” to “the public” when they are asked questions by reporters. A final example comes from my own recent experience in interviewing women who have been imprisoned on infanticide charges.

1. Although interviews are widely used in institutions, including the media, in many countries today, differences are apparent in the ideologies and practices that shape them. My remarks should be taken as focused on the United States unless otherwise stated.

The conclusion suggests that sustained attention to interviewing as professional practice can inform such anthropological issues as ethnographers' constructions of space and time, fieldwork as "immersion" and doubling, science/antiscience debates, questions of scale, and explorations of similarities of methods between anthropology and other forms of "expert" knowledge.

## Public Discourse from Locke to Habermas

A key to both the pervasiveness and the social impact of interviews lies in their embodiment of three competing ideologies of language, subjectivity, and knowledge. The first has deep roots in modern liberalism. John Locke's (1959 [1690]) theory of language, knowledge, and mind played an important role in creating liberal notions of the state, subjectivity, and civil society (see Bauman and Briggs 2003). Locke argued that knowledge emerges as individuals contemplate the world and rationally order their thoughts, parallel to the way in which social and political autonomy accrues to "the Industrious and Rational" who subdue the earth and acquire property (1960 [1690], II.iv.§34). The individual, autonomous mind is also the privileged locus of communication: individuals must convey transparent and precise models of the contents of their minds to others.

Second, this inner-expression ideology goes hand in hand with a notion of public discourse. Jürgen Habermas (1989 [1962]) traces the emergence of what he calls the bourgeois public sphere, a discursive realm that is constituted through collective rational debate about issues that affect society; here individuals speak apart from particular identities, locations, interests, and opinions. As feminists have argued, the emergence of private and public domains in the eighteenth and nineteenth centuries played a central role in structuring social participation and exclusion based on gender (Fraser 1990; Landes 1988); it is important to point out that this opposition often operated in racial terms as well. If Locke's liberal construction of society and politics and eighteenth and nineteenth democratic ideologies was to be sustained, the public-private dichotomy required discursive mediations, ways of moving discourse between them that were ideally accessible to all citizens. A common ideology of communication constructs the speech of everyman (and I use "man" intentionally here) as requiring particular sorts of technologies to move it out of private domains and into the public sphere. Just as practices of writing and reading the printed word enabled particular classes of people to shift between these realms in the eighteenth-century United States (Warner 1990), twentieth-century classes in "public speaking"—and education in general—were supposed to lodge mastery of these transformative mechanisms in the habitus of citizens (Bourdieu 1977, 1991).

Third, the Romantic reaction to the Enlightenment's privileging of abstract, disinterested, decontextualized speech took the form of a nostalgic celebration of words constructed as

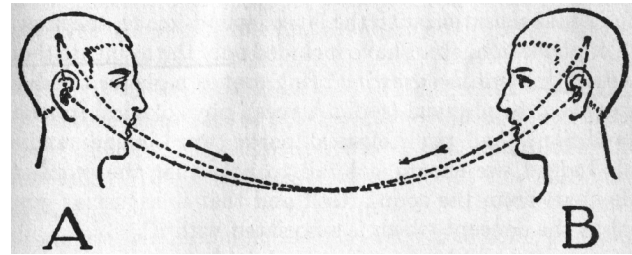


Figure 1. Saussure's (1959 [1916], 11) representation of "the speaking circuit."

organic forms intimately tied to their social milieu, from family to community to region to nation (see Bauman and Briggs 2003). The notion of authenticity, championed by Herder and the Grimms, privileged words that transparently reflected these social connections (see also Bendix 1997). Social interaction did not taint utterances, as for Locke, but imbued them with value. The notion that speech, meaning, and communication are *produced* through human contact became a central feature of twentieth-century ideologies of communication and knowledge in the United States. A key proponent of a view that privileges social interaction was a prominent sociologist of the early twentieth century, Charles Horton Cooley. As for Herder, the primordial locus of society for Cooley was "the family, the play-group of children, and the neighborhood or community group of elders" (1924 [1909], 24), and the natural basis of sociality was "face-to-face interaction," a term Cooley devised. Co-present individuals use speech and gesture to produce a natural alignment in social, cognitive, and moral terms, creating "mutual understanding of one another's points of view" (1924 [1909], 10).<sup>2</sup> Face-to-face communication became what Derrida (1974 [1967]) characterizes as a Western metaphysics of orality—it was construed as primordial, authentic, quintessentially human, and necessary. Although Ferdinand de Saussure (1959 [1916]) argued that the essence of language lay not in interaction but in *langue*, a decontextualized language system, the primordial construct from which he began was precisely "the speech circuit"—face-to-face interaction between two individuals (fig. 1). For Cooley, modernity constituted both problem and prospect. With increases in societal size and complexity, not all communication can be face-to-face; human beings were thus confronted with loss of communication and sociability. He suggested that printed material, railways, telegraphs, telephones, photography, and phonography created "a new epoch in communication, and in the whole system of society" (1924 [1909], 80). New technologies extended face-to-face communication and overcame miscommunication, promoting mutual understanding and social cohesion.

2. This concern with ideologies of "face-to-face interaction" and specifically the work of Cooley emerged in research undertaken jointly with Richard Bauman.

Vast extensions of electronic communication have often enhanced this nostalgia for the supposedly primordial face-to-face basis of communication and social life. "Interactive" modalities provide "users" with a feeling of agency—the sense that they can shape the flow of information and engage with a computerized interlocutor. Web sites' requests for comments seemingly enable users to affect what they are seeing. Chat rooms are a privileged site for inventing identities and forming relationships. That virtual dialogues might spark social and even intimate relations is simultaneously a source of revenue, a focus of popular culture (as in the movie *You've Got Mail*), and a locus of parental anxiety. Interview and call-in shows produce the illusion that radio and television programs are interactively, spontaneously produced. U.S. presidential campaigns include "electronic town hall" meetings, highly orchestrated media events that create the sense that face-to-face interaction between politicians and voters still lies at the heart of electoral politics.

Deborah Cameron (2000) suggests that people in the United Kingdom live in a "communication culture," where communication has been "problematized" (Foucault 1994, 598) in such a way that it becomes an object of analysis, anxiety, regulation, and reform. Communication thus gains visibility and force, and these three ideologies become modes by which we assess ourselves and others with respect to how well we are "communicating." Interviews magically appear to embody all three ideologies, producing discourse that seems to transform inner voices into public discourse by constructing particular types of subjectivity and inducing subjects to reveal their inner voices (attitudes, beliefs, experiences, etc.). At the same time, different types of interviews privilege one or more ideologies; the way they are conducted, analyzed, and presented tends to maximize their ability to embody notions of self-expression, publication, and social interaction.

First, psychiatric, oral historical, and life-history interviews center on individual interviewees and the process of self-disclosure, painting interviews as powerful windows into a person's experiences, memories, and feelings. Culture-and-personality studies used interviews, as extended by projective techniques (such as Rorschach and Thematic Apperception Tests), to probe individual minds. Franz Boas sought to turn individual experiences and reflections into a collective cultural voice, severing texts from features that tied them to individual positionalities in culturally heterogeneous societies and to his own role in occasioning texts through his "requests and specific . . . questions" (1930, x).<sup>3</sup>

Second, other strategies foreground the *social interaction* of the interview, generating authority and authenticity by constructing the interaction in particular ways and making texts or broadcasts seem like direct embodiments of the encounter

3. Boas elicited texts through questions in face-to-face dictation sessions and asked particular individuals, such as George Hunt, to write texts in response to his questions and requests. See Berman (1996).

between interviewee and interviewer.<sup>4</sup> Reflexive ethnography and "dialogic anthropology" (Tedlock and Mannheim 1995) place the anthropological production of knowledge in interactional realms, suggesting that individual minds (of ethnographers and their interlocutors) cannot be adequately accessed, analyzed, or reported without revealing the relational emergence of discourse. Antiformalist perspectives privileging open-ended, in-depth interviewing suggest that authentic, true voices emerge only when they are minimally constrained by formal procedures and attempts to control interviewees. In the 1970s, some ethnographers rewrote their monological representations of culture as products of complex interactions (see Dumont 1978; Rabinow 1997). Women interviewing women became an important focus of feminist social science research because these interactions seemed to unlock suppressed self-expression, create more open, egalitarian, and honest exchanges, include women in public discourse, and open up female counterpublics (see De Vault 1999; Oakley 1981). Ruthellen Josselson (1996, 13) says of interviews with 30 women over 20 years, "There is something about our interaction that led these women to strive for naked truthfulness." At the same time that women scrutinized gender and power in interviews, essentialist understandings of woman-to-woman relations (Reinharz and Chase 2002) deepened the ideological placement of interviews in primordial face-to-face interaction. Ruth Behar's (1993) *Translated Woman* represents the communicable flow of the text in interactional terms from the two women's initial meeting through Behar's act, as the subtitle projects, of *Crossing the Border with Esperanza's Story*. Including interview transcripts with questions and responses in publications, along with descriptions of "the context," generates interactional authenticity, keeping the ethnography indexically tied to the social interaction and seemingly placing the reader in the middle of it as well. Including interviewees (often framed more as co-conversationalists) as coauthors not only identifies the anthropologist as an ethical subject but projects the interaction as constitutive of the text (see, e.g., Gudeman and Rivera 1990).<sup>5</sup> My point is that these diverse research and analysis strategies derive their logics and authority from their fore-

4. I am not arguing here that conversation analysis and ethnomethodological and related approaches cannot inform the study of interviews; I studied interviews interactionally (Briggs 1986), and a recent example is provided by Rapley (2004). Rather, I am concerned here with the way "face-to-face interaction" gets constructed as the primordial, natural setting for communication and interviews and analysis is limited to what takes place during interactions focused on questions and responses.

5. Community-based participatory research (see, e.g., Minkler and Wallerstein 2003) provides an interesting case. In that it involves research participants and others in planning, implementing, and analyzing research activities, it opens up extensive possibilities for examining discrepant communicabilities and exploring alternatives to dominant ideologies and practices. Nevertheless, most projects involve teaching nonprofessionals to conduct surveys and other types of interviews rather than critically exploring interview techniques.

grounding of the status of interviews (and other research practices) as social interaction.

With such celebrity interviewers as Charlie Rose and Larry King, regular viewers tune in to see interviewers and styles of interaction rather than particular types of content. These interviewers humanize news and newsmakers, letting us feel close to people and social worlds to which we lack intimate access. No-holds-barred interview styles have made Geraldo Rivera and Jerry Springer daytime-television staples, with Oprah Winfrey offering a more genteel alternative. Turning studio audiences into stand-ins for viewers seemingly includes us all in these charged interactions. To be sure, these are highly staged, with participants selected for the seemingly spontaneous utterances that they are scripted to utter, and their placement is rehearsed in advance and governed by generic and program-specific rules. The point is not just that the complex pragmatics that underlies their construction is not adequately characterized by one or more of these simplistic communicative ideologies; what is more interesting is that they are produced in such a way as to make them seem like spontaneous and unique interactions that force individuals to reveal their often bizarre individual worlds under the fascinated gaze of a shocked public.

Finally, interviews are commonly portrayed not just as ordinary conversations but as carefully structured to elicit inner worlds with minimal intervention and to maximize their value for public discourse (see Briggs 1986; Cicourel 1974; Mishler 1986). As a result, characterizing the way in which an interview is turned into public discourse becomes a key means of representing and authorizing interviews. Some interviews are defined in terms of the way they exclude as well as include publics. A generation ago, anthropological interviews often excluded the interviewees from the public that was meant to read them. Confidential interviews are defined by the promise not to make responses public. Nevertheless, many forms of interviewing are defined precisely on the basis of the way they turn private into public discourse. Nancy Fraser (1992) and others emphasize the multiplicity of publics, including "counterpublics" created by persons excluded from dominant public spheres (Calhoun 1992). Warner (2002) builds on Habermas (1989 [1962]) in suggesting that although public discourse seems to presuppose the existence of a public to which it is addressed, the public circulation of cultural forms creates publics. Nevertheless, this process depends on what Louis Althusser (1971) termed *interpellation*. His celebrated example is of a policeman calling out, "Hey, you!" Insofar as we recognize ourselves as being addressed by this statement and turn around, we are being interpellated as the subjects of state discourse. Publics are created when texts engage them in particular sorts of ways, inducing them to feel part of a social group that shares a particular orientation toward specific publicly circulating cultural forms. This Janus-faced existence of "the public" as both singular and multiple, already existing and performatively constructed, seems to have become part of media, popular, and other ideologies of com-

munication. Interviews appear to make it possible to bring these two sides together.

Surveys and opinion polls presuppose the role of highly scripted social interactions in revealing individual worlds and converting them into public discourse. They attend to interaction in that they spell out each aspect of the exchange and each word that interviewers utter; the goal, however, is to regiment the interaction so rigidly that no statistically significant differences emerge from interviewer to interviewer and interview to interview. In theory, the interactional component thus becomes scientifically irrelevant and can be virtually erased from the equation. Polls are read as "public opinion," as reliable means of taking from 1,000 to 2,000 private opinions and attitudes and using them to reveal a composite public voice (see Briggs 2007). Focusing on the interaction in an initial methodological section rhetorically serves to legitimize erasing the interactions and the specific identities of respondents. Surveys and polls thus both *find* preexisting publics and *bring them into being* by magically enabling them to speak, capturing their multiplicity, and producing a single "public opinion."

Thus, since interviews rely on all three ideologies, it is possible to make a particular type seem to embody one of them naturally and authentically without losing its relationship to the other two. Some strategies maximize two or three at the same time. Live on-camera news interviews seem to collapse the time of the event, the interview, and our reception of it. Celebrity interviewers such as Jerry Springer seem to thrust intimate individual worlds directly into the public sphere through the mediation of charged interactions. Taking calls from studio audiences or listeners/viewers recruits members of "the public" as interviewers, seemingly placing individual expression, questions-answers, and public reception within the same frame. The participation of the public addressed by the interview is directly evident both in its production and in its reception.

That interviews embody these communicative ideologies raises important problems that anthropologists need to tackle. First, all three ideologies naturalize the sort of representational claims identified by Gayatri Spivak (1981): I can accurately tell people about you because you expressed yourself in a genuine fashion, I was there with you when you did it (and I am the same person who is writing now), and your words are ready-made for insertion into public discourses (such as ethnographies) so that readers can feel as if the interviewees were speaking to them. Second, anthropologists are themselves so swayed by these ideological constructions that anthropological interviews largely remain black boxes (Latour 1987), technologies so widely accepted that you can just feed in questions and get quotations for your publications without worrying about the complex pragmatics that make them work. Our own assimilation of these ideologies thus limits the ways we interview and reflect on our own and other people's interviews—at the same time that it makes inter-

viewing an important mode of anthropological knowledge production and a marginal subject of anthropological inquiry.

## Communicability, Interviewing, and Participation

How do these ideologies become socially significant, and how do they relate to interviewing? I would like to explore these connections under the rubric of *fields of communicability*. The term *communicability* puns on various senses of the word. In normal usage, communicability suggests volubility, the ability to be readily communicated and understood, and microbes' capacity to spread. I add a new sense in which communicability is infectious—the way texts and the ideologies find audiences and locate them socially/politically (Briggs 2005). Communicability involves four components:

First, communicability refers to socially situated constructions of communicative processes—ways in which people imagine the production, circulation, and reception of discourse. Silverstein (2004) suggests that social interaction is structured by a dialectical interplay between pragmatic and metapragmatic features, between the way signs are placed in the world and the way they represent their own being-in-the-world. The notion of communicability suggests that the value of these cartographies lies in their claim both to map what is taking place in particular discursive events and processes and to reify certain communicative dimensions in particular ways and erase others, thereby creating subjectivities and social relations and attempting to shape how people will be interpellated (Althusser 1971) in relationship to these acts and processes. I will refer to projections of the way texts represent their own points of origin, modes of circulation, intended audiences, and modes of reception—contained within the texts themselves—as *communicable cartographies*. Public discourses attempt to specify, implicitly or explicitly, which publics should receive them and how, enabling them to influence but not to determine the ways in which people will imagine themselves in relationship to texts and how (or whether) the discourses will circulate (see Warner 2002).

Second, forms of communicability are placed within what Bourdieu (1993) calls social fields, arenas of social organization that produce social roles, positions, agency, and social relations and that shape (without determining) how individuals and collectives are interpellated by and occupy them. Communicable cartographies create positions that confer different degrees of access, agency, and power, recruit people to occupy them, and invite them to construct practices of self-making in their terms, and they operate quite differently in, say, clinical medicine than in law courts or television news.

Third, communicable cartographies are chronotopes (Bakhtin 1981), which project discourse as emerging from particular places (clinics, laboratories, academic units, etc.), as traveling through particular sites (such as conferences, classrooms, newspapers, and the Internet) and activities (doing interviews, analyzing and publishing data, etc.) and as

being received in others (coffeehouses, homes, cars, and offices). The temporalities they project can be linear and direct, such as a trip from lab to clinical trial to pharmaceutical factory to physician to consumer, or have the more multi-stranded and reversible temporality of Internet communication. My point is not that discourse moves between fixed points in a linear fashion; indeed, discourse pragmatics do not operate in such a simple fashion. In accepting communicable cartographies, however, we accept particular spatializing and temporalizing practices, recognize specific sets of spaces and temporal contours, and define ourselves in relationship to them.

Finally, this process is powerful, shaping and contestable; in spite of their basis in material and institutional inequalities, communicable maps achieve effects as people respond to the ways that texts seek to interpellate them—including by refusing to locate themselves in the positions they offer, critically revising them, or rejecting them altogether. As they receive a text, people can accept the communicable cartography it projects, accept it but reject the manner in which it seeks to position them, treat it critically or parodically, or invoke alternative cartographies. Access to symbolic capital (medical training, for instance), communicative technologies, and political-economic relations restrict, however, one's possibilities for appropriating or resisting communicable cartographies and circulating one's own schemes.

In order to grasp the ways in which particular types of interviews both embody and create communicabilities, it is useful to place them in relationship to those situated in other social fields, and I will briefly provide an example from science. Latour (1999) traces for us the powerful modes of classification and transformation that are needed to extract knowledge from nature and insert it into scientific spaces and texts. He follows a group of scientists as they take samples from a Brazilian rain forest, inscribe them in notes, drawings, and photographs, insert them into spaces in the laboratory, extract scientific information, execute shifts of scale, abstractness and generality, and create a text. He shows us the powerful epistemological and social work and technologies needed to retain a sense of referential stability and truth throughout the process—to convince scientists that the published text contains information derived from nature, transported faithfully through these material, spatial, and epistemological transformations according to standardized and accepted procedures, and accorded a definitive, authoritative form. Even as samples are detached and then “reassembled, reunited, redistributed” (p. 39), scientists construct “an unbroken series of well-nested elements” (p. 56) in which each action performed on a sample anticipates the role that it will play in later research activities. Although Latour's account is insightful, it also reifies scientists' communicable cartographies as valid accounts of the complex discursive/material practices in which they engaged, thereby reproducing their projected spatial and temporal features and subject positions. Actor-network theory (Latour (1987) itself would suggest that such

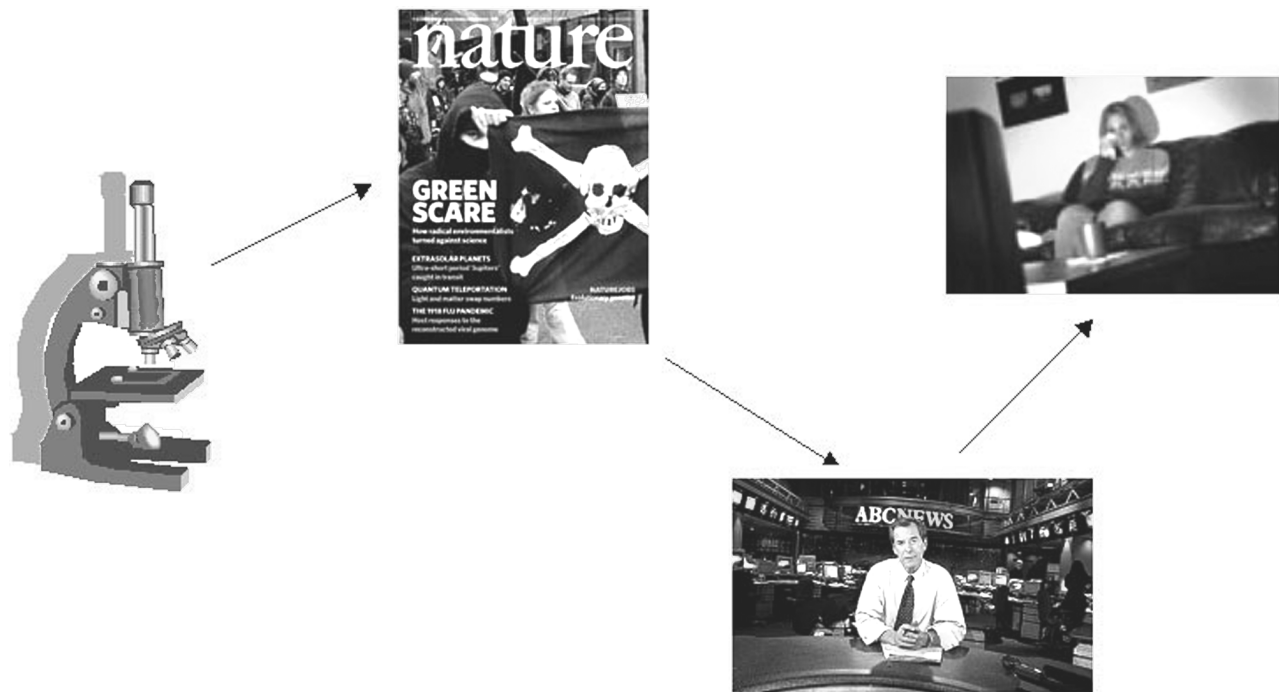


Figure 2. Common cartography of scientific communicability.

communicable models involve misrecognition (Bourdieu (1990a)—scientific knowledge does not follow such linear paths; actor-network theory itself reproduces the exclusion of seemingly marginal or subordinate subjects (see Haraway 1996). When scientific “breakthroughs” are deemed to be newsworthy, this linear flow of information seems to pass through reporters, who “translate” it into lay terms and beam it to ignorant but interested audiences (fig. 2).

If the only communicable fields in which we were interpellated were those in which actors with specialized symbolic capital generate information that either passes us by or positions us as passive audiences, it would be hard to imagine ourselves as vital parts of a democratic society. Interviewing constitutes a central means for sustaining the sense that each citizen has the right to participate, be heard, and affect collective life. A key way in which interview communicabilities sustain democratic ideologies is by structuring television and radio news. CNN broadcasts headline news, finances, health, sports, politics, and entertainment segments; these diverse “beats” all rely on interviews. Television interviews convert the conquest of space by time (Harvey (1989) into powerful auditory/visual images as people on opposite sides of the world become talking heads that appear side by side on television screens. The technological and ideological construction of globalization as a “network society” (Castells 1996) that encompasses everyone on the planet is tied to the global media’s transformation of interviewees and viewers into global

citizens—it seems as if anyone, anywhere in the world, could become a subject/source of news and speak directly to you and me. Even if we might not be able to imagine ourselves in a laboratory making a scientific breakthrough, we just might be the next bystander interviewed on the evening news. We can, at least some of us, vote once in a while, but interviewing seems to insert us into democracy daily as we participate as audiences if not interviewees.

There is no one single field of communicability associated with interviews, but a number of widely shared features are evident:

1. Interviewing is envisioned as centering on an individual, who becomes the *origo*. We therefore do not ask where the discourse comes from—interviews spring from the social worlds of interviewees. Gubrium and Holstein (1997) suggest that the magical ability of interviews to reveal interior spaces constitutes an epistemological stance of naturalism—the notion that researchers can document the interviewees’ “natural” environment without disrupting it.
2. Interviewees produce this material simply by being interviewees—they need not possess any special status or expertise, only a set of experiences to convey.
3. Although they may not need professional qualifications to become interviewees, the process of recruitment, interviewing, and analysis generally involves inserting individuals into systems of social classification—according to gender, race, age, income, or relationship to a particular event (wit-



ness, victim, etc.). Subjects racialized as African American, Asian American, Latino/a, or Native American speak for all members of “their” race but not for “the population at large” or for whites. Immigrants and “public charges” can be interviewed regarding their (self-) exclusion from the categories of citizen and productive member of society. “Victims” must talk like “victims.” That this facet of interviewing seems to contradict the first feature is evident; our communicable imaginaries enable us to envision that anyone might be an interviewee (how democratic!) but not that everyone can talk about everything (symbolic capital structures democratic participation).

4. A feature of the seemingly democratizing thrust of interview communicabilities is the illusion that all of us are treated in the same basic fashion when we become interviewees—we are asked questions that we should answer. Communicability and pragmatics are often at odds here when high-status interviewees get to shape the conduct of interviews and the questions that are asked (frequently suggesting them themselves) and are accorded more deference; interviewer-interviewee power differentials are frequently heightened when interviewees are working-class or racialized.

5. Digital communication technologies have extended the spatio-temporal power of interview communicabilities. Reporters travel to interviewees’ homes or rely on reporters from local outlets and instantly produce and transport audiovisual images that seem to connect us without a spatial or temporal gap to people, places, and events around the world. Wherever reporters go, it seems natural that they will ask questions and people will answer them, thereby inserting authentic voices into the sounds and sights of the tsunami or the massacre.

6. Briggs, Cicourel, Hyman, Mishler, Oakley, and others argue that interviews are structured by contradictions and competing demands on interviewers and interviewees. Nevertheless, successful interviewers naturalize connections between the three ideologies—ideas and feelings appear predisposed to emerge in interaction and reappear on television, in journals, or as statistics. As do the complex systems of transformation that seem to transport a plant from rain forest to laboratory to scientific publication, complex practices and transformations of scale and modes of representation separate questions and responses from scholarly publications or broadcast transcripts, and analyses are rendered nearly invisible through seamless linkages. Casting interviews as a quintessential embodiment of all three ideologies helps to produce the sense that a complex array of practices stretching over multiple times and places forms a single coherent, integrated package: an interview.

7. Studies based on interviews do not ask us to read their conditions of production and routes of circulation but rather map them for us. Ethnographies project communicable maps, and interviews play an important role here. We have learned that ethnographies create subjects and objects, separate “us” from “them” by “a denial of coevalness” (Fabian 1983), and infuse texts with scientific authority (Clifford 1988; Clifford

and Marcus 1986; Marcus and Fischer 1986). Anthropologists use a broad range of rhetorics—from technical discussions of sampling, reliability, and “consensus” (Romney, Weller, and Batchelder 1986) to self-disclosure to portraits of close relationships with interviewees—to construct their interviews as having the power to collaboratively produce individual expressions that can be fitted into collective portraits and transported in professional texts and contexts. A key way in which “scientific” anthropologists distanced themselves from “humanists” or “postmoderns” was to turn communicable maps of interviews into measures of scientific rigor—unstructured, open-ended interviews standing for antiscience and formal interviews embodying real science. As my discussion of *Translated Woman* suggests, however, the latter similarly attempt to spell out the communicative terrain they claim to follow. Television news creates visual models of communicable trajectories right before our eyes, just as sociological articles invoke techniques of sampling, interview techniques, and statistical analysis. We are asked to accept interviews as being produced and reaching us through precisely the communicable cartographies they project.

8. Since communicable maps embody social fields, accepting them leads us to conceive of and relate to people (including ourselves), technologies, epistemologies, and places in terms of their communicable roles. Communicabilities project power, help create discursive forms that seem to embody it, and obscure its recognition.

In focusing on communicability, I am not attempting to present an account of the pragmatics of interviewing. I rather hope to show how communicative ideologies structure interviews ideologically and shape their social effects.

## The Epistemological Pitfalls of Communicability

An initial example of how the uncritical acceptance of interview communicability can create an epistemological quagmire is the controversy over David Stoll’s (1999) (in)famous critique of *I, Rigoberta Menchú* (Burgos-Debray 1984). When she met the Venezuelan anthropologist Elisabeth Burgos-Debray in 1982, Rigoberta Menchú Tum was a Guatemalan activist who had been invited by solidarity groups to visit Paris in order to draw international attention to the Guatemalan army’s war on indigenous communities. On the basis of interviews conducted in her Paris home over the course of a week, Burgos-Debray produced a first-person narrative. The book became a basic text in many college courses, a work that for many readers epitomized the effects of racism, exploitation, and violence on people of color in nations beyond the metropole. It drew great attention to Menchú’s many efforts to bring peace and justice to Guatemala and elsewhere, thereby contributing to her recognition with the 1992 Nobel Peace Prize.

The anthropologist David Stoll challenged the validity of *I, Rigoberta Menchú* on the basis of interviews he conducted in

the sites described by Menchú, arguing that many of the most poignant events described in the book were fabrications and distortions. He went on to attack postmodern and postcolonial scholarship, which, he argued, was undermining scientific investigation and the search for truth. As John Beverly (2001) and Mary Louise Pratt (2001) have observed, Stoll's critique was appropriated by neoconservative critics in challenging scholarship that focused on the way state violence and repression, U.S. domination, racism, and economic exploitation infuse structural violence into the lives of poor people of color. Many progressive critics have criticized Stoll's own truth claims and defended the veracity of Menchú's account, often arguing that the latter reflected collective rather than individual narrative practices and canons of veracity (see Arias 2001). Oddly, neither side has successfully challenged the basic terms on which both *I, Rigoberta Menchú* and Stoll's critique rely.

It seems clear that Burgos-Debray sought to produce a hybrid text. On the one hand, she wished to project a compelling story of hardship and violence told by a Guatemalan indigenous leader for a general audience; her support of Latin American revolutionary movements seems to suggest that the publication was intended to contribute to the solidarity movement. On the other hand, her approach was influenced by anthropological life-history documents, in which first-person narratives are used in providing reflections on cultural patterns, particularly rituals connected with the life cycle.<sup>6</sup> Burgos-Debray's (1984) introduction to *I, Rigoberta Menchú* provides an origin story for the text, recounting how she met Menchú, the week they spent together in her house, and her preparation of the text for publication. It appears that Burgos-Debray signaled her hybrid agenda to Menchú by initially giving her "a schematic outline, a chronology: childhood, adolescence, family, involvement in the struggle" (p. xx). The interviewer's continued interest in culture and her anthropological inventory of the necessary categories was apparent: "As I listened to her detailed account of the customs and rituals of her culture, I made a list which included customs relating to death. Rigoberta read my list." When Burgos-Debray failed to ask about death, Menchú returned with a cassette dealing with funerary ceremonies "because we forgot to record this." Burgos-Debray reports that "in order to transform the spoken word into a book," she transcribed the tapes, faithfully retaining every recorded word. Correcting the grammar but reportedly leaving the style intact, she compiled a card index of themes and used it in recontextualizing the material in "the form of a monologue" (p. xx). Deleting her own questions, Burgos-Debray "became what I really was: Rigoberta's listener."

This metanarrative involves a fascinating process of reversal

6. As the influence of psychoanalysis sparked interest in culture-and-personality studies in anthropology, life histories became an important disciplinary genre. See Dyk's (1967 [1938]) *Son of Old Man Hat* for an example and Clyde Kluckhohn's (1945) statement of the importance of autobiographies in anthropology.

that is organized around that leitmotif of modernity, the opposition between orality and literacy<sup>7</sup> (and both individual-expression and face-to-face-interaction ideologies): "Rigoberta has chosen words as her weapon and I have tried to give her words the permanency of print" (Burgos-Debray 1984, xviii). Echoing the Grimms' ideology of textual fidelity (Bauman and Briggs 2003), Burgos-Debray (p. xx) reports that she simply "became [Menchú's] instrument, her double, by allowing her to make the transition from the spoken to the written word." But this orality is shaped by a complex set of textual parameters.

One of the reasons that *I, Rigoberta Menchú* is able to speak, as Burgos-Debray (1984, xi) declares in advance, "for all the Indians of the American continent" lies in the introduction's projection of the text in such a way as to embody the foundational communicative ideologies I have described above. Deleting questions and other dialogic traces and reconstructing the responses as a monologue enables Burgos-Debray to present the text as the unimpeded unfolding of an individual voice and mind. This ideological construction is set up in advance in the introduction, where she declares, "I very soon became aware of her desire to talk and her ability to express herself verbally" (p. xv). She claims an almost magical power for the text (even in translation, it seems) not only to represent Menchú's voice but to make it come alive: "We have to listen to Rigoberta Menchú's appeal and allow ourselves to be guided by a voice whose inner cadences are so pregnant with meaning that we actually seem to hear her speaking and can almost hear her breathing" (p. xii). This statement constitutes a strong claim to have preserved the authenticity of Menchú's voice in what she characterizes as the journey from orality to print. Burgos-Debray suggests that Menchú digressed in the interviews so much that "I therefore let her talk freely and tried to ask as few questions as possible" (p. xix). At the same time that this statement purports to document a shift of control over the interviewing to Menchú, it erases Burgos-Debray's role in the production process.

Second, the ideology of face-to-face communication enables Burgos-Debray to project a text based on recordings made during one week in Paris as capable of capturing an exotic, violent world and bringing modern readers under its spell. Even if she denies that her role in the social interaction shaped the text, preserving its status as self-expression, she claims to have been there as witness—since she is the one who hears Menchú speak and breathe, she can now offer this

7. Havelock (1963, 1986), Goody (1977), and many others reify this distinction in contrasting modern, literate subjectivity with that associated with, respectively, the world of Homeric epic and the "illiterate societies" that traditionally formed the focus of anthropological inquiry. A wealth of literature critically evaluates these notions, from Derrida's philosophical deconstruction of Western metaphysics (1974 [1967]) to studies of vernacular narratives (Bauman 2004; Tedlock 1983) to work on the relationship between linguistic and political ideologies (Bauman and Briggs 2003), literacies (Collins 2003, Street 1995) and the emergence of the book (Johns 1998).

positionality to the reader. The two women lived in Burgos-Debray's house, sharing black beans and the tortillas that Menchú made each morning and evening in keeping with "a reflex that was thousands of years old" (1984, xvi). Making the tortillas made Menchú happy and brought back pleasant memories of Venezuelan childhood for Burgos-Debray. It would seem, however, that the two women may have experienced pleasure and memory differently. For Burgos-Debray, watching Menchú make tortillas reminded her of how "the women I had watched in my childhood [that is, the kitchen help in her elite home] made *arepas* [Venezuelan cornmeal cakes]." It does not seem to have occurred to her that making tortillas in the house of a rich *ladina* (a nonindigenous Latin American woman) might have stirred up some of the bitter memories that Menchú relates in her account of working as a maid in Guatemala City (pp. 91–101) or shaped the power relations between them. This face-to-face interaction creates a social world in which an authentic narrative can emerge without being contaminated by the social circumstances of the interviews: "For the whole of that week, I lived in Rigoberta's world. We practically cut ourselves off from the outside world" (p. xv). The face-to-face interaction embodied in the interviews and the context of sharing space for a week produced an intense relationship and a sense of confidence. This interaction enables Burgos-Debray to make the classic anthropological journey of cultural transcendence, and she thanks Menchú in the acknowledgments for allowing her "to discover another self" (p. xxi). Burgos-Debray becomes a rhetorical figure who allows us to imagine ourselves as part of this intimate interaction. Her characterization of how Menchú's meaning-laden "inner cadences" include us magically in the moment of articulation provides a metaphysics of presence that hybridizes inner-expression and face-to-face-interaction ideologies.

Third, just as Burgos-Debray suggests that it was Menchú and her voice that fashioned the text as authentic, direct self-expression, it was Menchú's desire and not Burgos-Debray's literary and political agenda that projected the text directly and unproblematically into the public sphere. "She talked to me not only because she wanted to tell us about her sufferings but also—or perhaps mainly—because she wanted us to hear about a culture of which she is extremely proud and which she wants to have recognized" (1984, xx). In dialogical play of first- and third-person pronouns, Menchú's interior world, the social interaction, and the projection to us, the readers, all come together seamlessly and naturally in the space of a sentence.

Particularly in the chapter entitled "The Construction of *I, Rigoberta Menchú*," Stoll (1999) explores the complexity of the text. Beyond suggesting minor changes to Burgos-Debray's account (such as asserting that 18½ and not 24 hours of recordings were made), he begins to explore the complex intertextual field in which the book is located. He suggests that Menchú's father may have recounted some events in similar terms (p. 194). Although he presents little detailed

evidence, he argues that Menchú's work with guerrilla groups, her time with Monsignor Ruiz and other progressive Catholics in Chiapas, and her interactions with members of solidarity groups may have shaped her narrative style and her views regarding the political value of stories. He quotes Menchú to the effect that she had the text read to her prior to publication, making some decisions on what should be omitted.<sup>8</sup> He discusses statements that Menchú made over the years about the text, remarks that reveal the complexity of the social and discursive relations in which the book is lodged and the quite different locations of the two women vis-à-vis the politics of race, class, and intellectual property rights. Unfortunately, this material does not lead him to reconceptualize how the text was made, appreciate the intertextual complexity of the book, explore the interviewee's loss of control over the recontextualization of her words, or reflect on the complex questions that arise in turning a life into intellectual property and negotiating rights to sell it globally in various languages. It rather provides evidence to suggest that Menchú is a prevaricator. To be sure, Stoll does not complicate his own interviews, which are intertextually enmeshed with those presented in *I, Rigoberta Menchú*. He just transparently transforms the experience of his respondents into public discourse.

Stoll deflects charges that inaccuracies in the text might be Burgos-Debray's fault in order to place the blame squarely on Menchú. He reduces Burgos-Debray's role to the question of whether the text might "have been seriously distorted" by Burgos-Debray. The rhetoric of "distortion" signals an effort to circle the wagons around the interview in the face of efforts to challenge its communicable construction (see Briggs 1986). Stoll shifts critical scrutiny away from Burgos-Debray by allowing her to retell her origin narrative for the text on the basis of an interview he conducted with her; here she reasserts the communicable fidelity of her role and places any issues of truth claims on Menchú's shoulders. Listening to two hours of the tapes also enables him to provide a definitive account of the interaction between Burgos-Debray and Menchú—a woman who wanted "to express herself, to overcome her experiences and get to a broader place than where they had her" (1999, 184). Extending the origin story, he suggests that Menchú was such a spellbinding storyteller and was in such great control of the process that Burgos-Debray had to set aside her anthropological paradigm. He suggests that "her only questions are to clarify details. Never does Elisabeth raise

8. Stoll elides a number of textual controversies. Burgos-Debray simply states, "I began by transcribing all the tapes" (1984, xix), but in her interview with Stoll she changes her origin story to suggest that she had help from "a Chilean friend" in transcribing the tapes (Stoll 1999, 185). The Guatemalan historian Arturo Taracena states that he raised the money for the transcribing and hired the transcriber—Burgos-Debray did not participate in the transcription (see Aceituno 2001). He similarly contends that he was involved in some of the interviewing and played a key role in the editing process, thus complicating the notion that the text is a transparent document of an intense encounter between two women.

new subjects, change the direction of the interview or prod a reluctant subject into continuing” (p. 188). Burgos-Debray’s claim that she had only planned to write a magazine article—that the idea of the book came to her only after the interviews were completed (p. 185)—leads him to conclude that its creation was, in the end, quite simple: “A young woman told a story . . . she created in 1982 with the help of Elisabeth Burgos” (p. 282). Menchú and Burgos-Debray are both complex subjects with multiple positionalities who undoubtedly had complex, multiple motives for participating in the recording and production of a text that shifted over time. Stoll reduces these motives to familiar interview communicabilities—Menchú had the need for self-expression and to insert her voices into public discourse, and Burgos-Debray used interviewing in making this passage possible.

As Pratt (2001) suggests, the rhetorical structure of Stoll’s book reifies *I, Rigoberta Menchú* as a set of referential propositions advanced by a single person. Stoll can achieve this massive oversimplification of the text only by projecting a familiar communicable cartography of interviewing. He uses the textual origin story that Burgos-Debray tells in the introduction and reiterated in the interview with him, along with a couple of hours spent listening to the tapes, to turn this common ideology into proof that it constitutes a faithful account of the construction of the text. Remarkably, he attributes this ideological construction of narration as truth-telling to Menchú and the Guerrilla Army of the Poor and then uses it—together with quasi-judicial canons of truth and evidence—as the sole mechanism by which *I, Rigoberta Menchú* can be evaluated. Ironically, part of his evidence consists of pointing to ways in which the text fails to fit this cartography—that the stories it contains have complex intertextual relations with myriad stories, discourse, and agendas and that the narration and editing were structured by different agendas and inequalities of power. Stoll could have used these fleeting insights to explore the multiple communicable maps that seem to have shaped the text, as associated with life histories, *testimonios*, the circulation of stories in Menchú’s family, the spaces she had inhabited, and the global circulation of revolutionary counternarratives. He could have gone on to make a structural point, showing that texts based on interviews do not conform to received communicable understandings of interviews but rather constitute contested terrains within a complex set of social, political, and intertextual relations. Instead, these traces of a problematic communicability somehow get converted into definitive proof that a woman who faced structural and military violence and went on to become an internationally celebrated defender of human rights is really just a liar.

## Pragmatic Complexities and Multicultural Publics

I have argued above that interviewing is an important site in which anthropological modes of knowledge production have increasingly intersected with those associated with educational

understandings of required literacy skills, media practices, the interactions of corporations and businesses with their customers, and everyday routines in social service and other institutions. Interviewing is thus a valuable locus for examining the exchange between anthropological and other forms of knowledge making and interpretive practices. A key basis for this intersection lies in the ideologies of self-expression, interaction, and publication that naturalize interviewing practices and imbue their products with value. I begin this section by exploring some of the ways in which interviewing practices go beyond these ideological constructs. The question is far more complex than the age-old game of pitting “ideology” against “reality,” tacitly advancing the idea that I know how interview practices are “really” organized as opposed to the distorted understandings of others. Rather, I am interested in the way the complex pragmatics of interview practices produce discourse that seems to embody individual subjectivities, be firmly rooted in face-to-face interactions, and seamlessly transform individual expressions into public discourse. By creating forms that match their own ideological constructs, anthropologists become less aware of elements that point to the complex subjectivities and social and intertextual relations that are created and enacted in interviews. I go on to show that some interview participants have developed more complex ideological constructions that enable them to create an emotive connection with their experiences and words, seem to be perfectly in interactional sync with their interviewers, and produce words and images that seem to fill slots that have just been waiting for them in public spheres.

Researchers have argued for more than half a century that ideological constructions of interviewing are not sufficient to contain the observed complexity of interviewing practices. Hyman et al. (1954) pointed out that professional understandings of interviews failed to capture the contradictory nature of concerns with reliability and validity. Cicourel (1974) documented in detail the complex practices used by both survey researchers and interviewees in circumventing the interactional and discursive contradictions imposed by attempts to standardize how questions are presented and tied to responses and in rendering their improvised violations invisible. More recently, Houtkoop-Steenstra (2000, 180) has used a conversation analysis framework in scrutinizing survey data; she argues that “the quest for standardization is no longer tenable, at least if its purpose is to generate not only reliable, but also valid, research data.” Briggs (1986) and Mishler (1986) show that differential power relations structure qualitative, open-ended interviews and conceal the effects of these inequalities, thereby preserving naturalistic ideologies of interviewing. We suggest that differences in social worlds and discursive practices create communicative difficulties; although responses are construed as reflections of interviewees’ inner worlds, their form and content are also tied to efforts to manage problems of power and competing frames of reference. Holstein and Gubrium (1995, 2000) suggest that particular sorts of interviews both require and produce particular

sorts of subjects, thereby informing the roles of interviewer and interviewee, the “voices” that are produced, and their relationship to each party in the course of an “active” process of producing knowledge. In short, the pragmatics of interviewing involve complexities that are not easily contained by representations based on basic and common communicative ideologies. The point is not, however, the gap between “ideology” and “the real” but rather that interviews and representations of them are produced in such a way as to make them seem to embody and confirm these communicable projections.

Richard Bauman and I have proposed a framework for analyzing such relationships. We suggest that discourse can be viewed not as restricted to a single, bounded context but as continually decontextualized and recontextualized—extracted from certain texts, genres, contexts, and social worlds and inserted in others (see Bauman and Briggs 1990; Briggs and Bauman 1992; Bauman 2004; Silverstein and Urban 1996). According to this view, power lies not just in controlling how discourse unfolds in the context of its production but gaining control over its *recontextualization*—shaping how it draws on other discourses and contexts and when, where, how, and by whom it will be subsequently used. The ability to shape recontextualization confers control over transformations of scale, as when words uttered by one person or a few people are transformed into statements regarding entire populations or societies and inserted in books, articles, or broadcasts, much as the working-class Mexicans interviewed by Oscar Lewis (1961) came to stand for “the poor” everywhere. Our framework suggests that the conversation analysts’ frequent insistence that interviews are “mundane talk” is a product of limiting the analysis to “the interview” alone (e.g., Rapley 2004). Although mundane interactional practices are certainly in play, the shaping of interviews by preceding events and texts (research proposals, formulation of questions, etc.) and their orientation toward recontextualization in quite different settings (such as scholarly publications or policy decisions) suggests that this “mundane” quality is a powerful illusion.

Employing this framework leads us to view “the interview” as only one set of facets of a larger process that minimally involves the discursive transformations that take place between posing a research question and publishing the findings. In surveys, questions recontextualize a previously formulated “instrument” that in turn recontextualizes a research agenda that is shaped by disciplinary and institutional norms, theoretical concepts, methodological guidelines, the interests of funding agencies, texts (journal articles, books, and conference papers), and conversations with colleagues. For medical interviewing, Cicourel (1992) argues that the questions doctors ask patients are shaped by nurses’ comments, charts, journal articles, manuals, and medical school classes. Mishler (1984) and Waitzkin (1991) stress the effect of power asymmetries and institutional constraints on the types of discourses

that can be recontextualized (as “clinically relevant”) with what sorts of effects in medical interviews and argue that physicians’ transformations of illness narratives (Kleinman 1988) thwart doctor-patient communication and constrain patients’ agency.

Bauman and I have suggested that recontextualization creates complex intertextual relations characterized by both *links* and *gaps*. Following Bakhtin (1981), reproducing texts involves transformations of form, context, and meaning that preclude exact replication. An interesting example of an understanding of interviews that projects smooth links between interview responses and published texts is provided by one of the chief architects and popularizers of polls, George Gallup. Gallup (1972; Gallup and Rae 1940) constructs polling communicability as beginning in the private worlds and experiences of a scientifically selected “surrogate for the population”; the social interaction with an interviewer (even over the telephone) transforms these private, largely unreflective inner states into reflexive reasoning and articulate speech. The process flows in a natural and uninterrupted fashion from its initial locus in “the people,” given their natural inclination toward reflection, self-expression, social interaction, talking to strangers, and weighing in on public matters, to the sampling, interviewer training, and question construction. Statistical techniques of data analysis ensure that gaps potentially introduced by individual respondents or interviewers are eliminated to allow interviews with between 1,000 and 2,000 respondents to reflect a preexisting “public opinion” that synthesizes the entire range of voices contained in the U.S. population, “helping the people speak for themselves” (Gallup and Rae 1940, 287).<sup>9</sup> Published polling data now commonly purport to spell out the nature of the links by specifying the questions, sampling techniques, recruitment of respondents, and margin of error.

Research on the pragmatics of interviewing suggests, however, that these ideological constructions of links must contend with the intertextual gaps that arise all along this imagined unilinear sequence. In Silverstein’s (1976) terms, these gaps are not simply pragmatic but metapragmatic attempts to frame and contain pragmatic dimensions. Gaps are therefore not technical or social failures to link texts but metapragmatic contradictions—points at which people’s construal of the pragmatics of interviewing conflicts with ideological projections of the process. Methodological cookbooks in anthropology, other social sciences, and professional fields envision any gaps that become visible as problems to be identified and eliminated. Concerns with “bias,” “distortion,” “leading questions,” “uncooperative informants,” ego-defense mechanisms, refusal to participate, and the category of “no opinion” reproduce interview ideologies by suggesting that interviews consist of smooth, automatic intertextual links un-

9. See Bourdieu (1979 [1972], 1990b) on how polls create “public opinion.”

less somebody misbehaves or something goes wrong. All gaps thus constitute problems when they cannot be suppressed. Interviewees, when they are less attached to interviewing ideologies, are often much more aware of these gaps and their social, political, and sociolinguistic bases than are interviewers (see Briggs 1986). Successful interviewing, as defined in terms of modernist interview ideologies, thus involves constant metapragmatic work to construe intertextual and social relations as links and to suppress awareness of gaps throughout the course of the research.

I examine here two cases in which nonanthropologists have developed much broader, more sophisticated cartographies of interview communicability than those of anthropologists. First, Américo Paredes (1977) insightfully explores how social-scientific analyses encode racial inequalities. Reviewing research widely criticized by Chicano/a scholars for reproducing denigrating stereotypes (Madsen 1964; Rubel 1966), Paredes portrays interviews as the sites in which anthropological misrecognition emerges. When anthropologists' questions touched on elements that figured in popular stereotypes (rejecting biomedical for "folk" medicine, living for the present, fatalism, and familial solidarity as displacing individual achievement), Mexican Americans<sup>10</sup> recontextualized stereotypes as caricatures. Instead of laughing at these intercultural jests, Paredes suggests, the ethnographers wrote them down and enshrined them as Mexican American culture.

The interviewees had sophisticated senses of the performative pragmatics of interviewing. They sought to locate themselves, their words, and the anthropologists in relationship to two publics: racist whites who circulate Mexican stereotypes and both "Anglos" and Mexican Americans who challenge the racial status quo. Their communicable maps looked ahead to explore how interviews could intervene in public discourse. Paredes's article points to the sophisticated models of interviews and of social-scientific research in general common in working-class communities of color and of the ways in which interviewing has helped legitimize unequal access to education, legal protection, employment, electoral politics, and health care. Double-consciousness (Du Bois 1990 [1903]) seems to provide a basis for developing critical understandings of dominant interview communicabilities. For Anglo anthropologists, communicable naïveté reproduced racial and class inequalities. The researchers, however, believed themselves uniquely qualified to address publics in social-scientific texts and policy debates—Mexican Americans could speak only to one another and to anthropologists. They accordingly believed themselves able to interpret responses as answers to their questions and to have unique rights to recontextualize the fruits of these encounters in public discourse. By virtue of their capacity for imagining prefabricated, bounded ethno-racial groups and interpellating individuals to

speak for them, interviews are crucial for multicultural projects.

A second example is the way some technocrats are taught to talk when interviewed by reporters. Journalism projects the idea that reporters find information that should be public because it will interest or help "the public" and then transmit it to that public. Reporters prompt individuals to reveal what they know or feel when suddenly asked a question even if they know that their response or silence will face public scrutiny. Ideologies of individual expression lead us to see one person on camera asking questions and another responding as a collaborative production of discourse in that interactive context. Emotively charged responses show that the reporter and the question have located a subject to which the respondent is deeply attached and obtained a response from deep within the subject. Nevertheless, press practices are organized by logics that violate these communicable projections. In pre-interviews, reporters often call to ask, essentially, for interviewees' sound-bites, announcing their stories' "angles," what sort of information they seek, what other people have said, and what narrative slot interviewees are expected to fill (victim, angry consumer, reassuring official, etc.); reporters sometimes provide questions in advance. The potential sources decide whether they can fill those particular slots and with what sound-bites. If the sound-bites are good, then journalists begin the interview and relicit the bites, which fit their stories because they have been constructed to do so. Press releases attempt to lay out a pragmatic path for reporters, with lead, quotes, experts, and people-in-the-street all provided; these are then recontextualized in questions and answers (see Jacobs 1999).

Technocrats are increasingly being trained to complicate standard ideologies of journalistic interviewing. In *Feeding the Media Beast: An Easy Recipe for Great Publicity*, the former reporter and media consultant Mark Mathis (2002, 90) demonstrates how to use the media as "a marketing proposition. You are selling a product to a newsperson who will in turn sell it to her audience." Mathis presents "media rules" that will convert media novices into savvy media manipulators who get free and favorable publicity. One, the "rule of difference," demands that information presented to the press be unique and novel—carefully constructed intertextually vis-à-vis what is already circulating in order to differentiate itself. Second, according to the "rule of emotion," affectively charged statements draw media attention and are likely to appear as sound-bites. Third, the "rule of preparation" suggests that interviewees build relationships with reporters, finding ways to bring the story to their attention, identifying key elements, and constructing sound-bites; the performance of these mini-scripts must embody "planned 'spontaneity'" and not seem overprepared or canned (pp. 88, 106). Finally, the "rule of resource" teaches interviewees how to induce journalists to recognize them as producers of expert knowledge—

10. I use Paredes's (1977) term.

that is, as major conduits in the communicable cartography that defines a particular field.

Research that I am conducting on news coverage of health issues in Cuba, the United States, and Venezuela<sup>11</sup> suggests that officials project communicabilities that cast them as expert producers of specialized scientific knowledge. Nevertheless, several officials stated in interviews that they used the press less to inform “the public” than to influence four or five “decision-makers.”<sup>12</sup> Policy and funding organizations monitor news to see what “the public” is thinking. Messages directed at them thus come camouflaged as the *vox populi*. A community-clinic CEO said, “It builds, I think, some credibility that ‘hey, this is interesting, you know, this does apply to us.’ And, you get your name out there and then when you apply for a grant, I think it helps.” He provides reporters with doctors and patients to interview, thus enabling journalists to project stories as traveling from medical experts to the public; having a layperson on camera shows both that the public is interested and how viewers should interpret the news. The point is hardly that Mathis has grasped how interviewing and the media really work and that anthropologists should adopt the approach that he and other media consultants literally sell. It is less a matter of the “ideal” versus “the real” than of how these competing ideologies enable interviewees (and purveyors of press releases) to collaborate with reporters in producing audible and visible icons of dominant communicable projections. My aim here is to suggest not that anthropologists are naive but that reifying communicative ideologies as reliable maps to what takes place in interviews blinds us to the fact that we co-create cultural forms that seem to embody our own communicable preconceptions.

## The Prison Interviews

In 1994, while researching a cholera epidemic in a Venezuelan rain forest and state discourses that blamed poor mothers for the deaths of infants (Briggs and Mantini-Briggs 2003), I was summoned as a Spanish-Warao translator for an adolescent, Herminia Gómez (pseudonym), who had been jailed for infanticide (Briggs and Mantini-Briggs 2000). The assumption was that she did not understand sufficient Spanish or could not express herself in court, and the interview was to enable her to tell “the real story.” “I understood perfectly,” she told me. Gómez detailed the accusations against her, exposed their contradictions, related the structural-cum-physical violence that had led to the loss of her newborn, and described the suppression of her counternarrative. The conversation led me to investigate why judges, police, reporters, and laypersons are so fascinated with infanticide accusations and so quick to turn suspects into monsters—in the process visiting police stations, courtrooms, morgues, newsrooms, living rooms,

and, finally, a Caracas prison holding other infanticide convicts.

As a white, North American, middle-class male anthropologist interviewing poor Venezuelan women convicts, I was confronted with a dilemma. One interviewee, Gladys González (pseudonym), was a 32-year-old lower-middle-class university student when convicted of killing her newborn son in 2002; she had received 20 years. A social worker escorted González into a tiny office with two flimsy plastic chairs. Acknowledging differences of power and perspective, I proposed a positionality and a project. Disclosing my own loss of a daughter, Felicianita, I suggested that we speak as two parents whose lives had been transformed by losing children. Agreeing, González began by characterizing her identity and “horrible” life as imposed not just by her son’s death but by the social life of the stories that others told about it; she constructed a counternarrative by retelling her story in dialogue with them.

González described the construction of “the scandal” by the police, the press, forensic physicians, the public defender, the prosecutor, the judge, and the neighbors and reported that her attempts to explore “the why question” had been silenced. Her institutional storytellers supplied many whys—delinquency, drugs, insanity, depravity—none of which resonated. Her sense of abjection was compounded by the refusal of fellow inmates to help her face “the why question.” When she arrived at the prison, other prisoners “talked and talked and talked. But when another woman [accused of infanticide] arrives, you go out of style and you get put in second place. Now it’s just something ordinary. They don’t look at you, they don’t talk to you. You are in your room and your time has already passed. At first, there are rumors everywhere. They don’t listen to you: ‘She did it.’” Before she arrived, the staff had told other prisoners her story. An invisible celebrity, González was the object of constant narrative production but lacked an audience for her own narrative. After another woman accused of infanticide arrived, only the silence remained. I asked, “Do you talk about [the why question] with others accused of infanticide?” “Yes, we talk about it, but without going very deep at all. Because the other women already know all about each other.” Infanticide stories are so powerful that even other women convicted of infanticide cannot listen to one another without hearing monster stories. González started to silence herself: “I don’t know. And when you don’t know about an issue, you shouldn’t speak. So I think it best to remain silent.” In the end, the imposed silence outlaws the right to speech and knowledge. “The why question: I think about it everyday. It’s the thing that kills you. I don’t deserve to know why.”

As she reviewed the narratives that oppressed her, González began to challenge their spatio-temporal reductionism—spanning only the induced abortion and the moment she threw her son out the window—and restricted dramatis personae. She was not trying to declare her innocence or blame others. Her counternarrative focused less on the crime itself

11. Collaborators in California include Daniel Hallin, Robert Donnelly, and Cecilia Rivas.

12. Altheide (2002) makes a similar observation.

than on finding a way of telling her story that would enable her to rejoin an imperfect society, sharing its aspirations and contradictions and hoping that “at some point it might be of value to someone.”

These interviews have affinities with the modalities urged by De Vault (1999), Mishler (1986), and others. They differ, perhaps, in following the prisoners’ take on interviewing. Correspondence between the interviewing practices of anthropologists, attorneys, and activists led me initially to accept the assumption behind the charge given me in 1994—that an interview in Warao would permit unconstrained self-expression. My point is not that these women’s modes of counter-narration and/or attention from a white North American was therapeutic; we did not define our interaction as therapy. What I am interested in here is what these women taught me about interviewing. The potential power of these interviews lay not in attempting to disconnect their voices and narratives from other stories, sites, and practices—as if they could enjoy the luxury of a version uncontaminated by those told by detectives, neighbors, judges, reporters, and hundreds and thousands of popular commentators. Rather, they detailed multiple cartographies of communicability as a means of exploring what they wanted to talk about—the construction of oppressive narratives, the suppression of their counternarratives, the embedding of the interview narratives in the others, and possible ways of using these accounts in restoring their dignity. They deconstructed in detail the words that they were said to have produced in exchanges with detectives, lawyers, judges, reporters, psychologists, and others, the representation of this speech in a wide range of narrative forms, and the use of reified notions of communication in such a way as to infuse these words with truth and authenticity.

## Conclusion

In the end, focused attention on the role of interviewing in anthropological knowledge making can provide us with valuable perspectives on such issues as the spatialization and temporalization of ethnography, the doubling of ethnography “in the field” and “at the desk,” questions of scale, the science wars in anthropology, and the way anthropologists mirror and are mirrored by other “expert” knowledge makers. In *Routes*, James Clifford (1997) argues that culture was traditionally seen as rooted, as tied to a particular locality, thereby marginalizing movements of culture, bodies, technologies, and epistemologies. This characterization also applies to anthropological interviewing. Dominant communicable cartographies of interviews see them as emerging from individual minds and social interactions, fixed and bounded in space and time. In order to have “voices” and contribute to “public opinion,” interviewers must go to interviewees (even if telephonically) and collaborate with them in producing discourse. Close curtain: end of interview. The interviewer can then go to another individual or population in another space. Subsequently, the researcher claims the right to juxtapose

these voices and convert them into public discourse, that is, to make them travel.

Nevertheless, interview texts, akin to modes of classification and statistics, seem naturally to produce immutable mobiles (Latour 1988), forms that appear to traverse geographies and genres without losing authority or shifting meanings. Producing this mobility takes a great deal of work. Bowker and Star (1999) argue that statistics and classifications accrue complex indexical histories while moving through institutional sites and getting connected with competing interests and practices; technocrats make histories seem invisible and forms seem transparent, mobile, and unencumbered. Similarly, surveys and polls sever data from these indexical histories but link them to highly selective genealogies—abbreviated accounts of the communicable path reportedly traveled in their making. Lee and LiPuma (2002) argue that circulation is an anthropological blind spot; focusing on the production and reception of discourse, scholars have often seen circulation as a purely mechanical process, one that does not require ethnographic scrutiny.

To be sure, discourse extracted from ethnographic interviews does not claim the same mobility, abstractness, or stability as a statistic or a diagnostic category. Indeed, most ethnographers rather claim that interviews form part of larger ethnographic projects, including participant-observation and other activities (examination of archival records and the like); thus, interview results must be circulated within larger textual packages. Contrasting strategies for representing indexical histories of interviews provided a key battlefield in the science/antiscience debates of the 1990s. Ethnographers influenced by reflexive, dialogic, feminist, and postcolonial turns used communicable cartographies to turn contradictions and complexities in their knowledge-making practices and the inequalities accruing to different positionalities in research projects into objects of scrutiny. Anthropologists claiming to defend scientific anthropology against “antiscientific” contamination attacked their “pomo” (postmodernist) colleagues for not drawing up prescribed communicable cartographies and following them to the letter, and they demanded radical strategies of indexical pruning. To be sure, ethnographers are often called to account by their colleagues and others to demonstrate that what particular people say in response to specific questions in particular contexts can be validly elevated to the status of generalizations regarding a particular “culture.” Good ethnography requires determining the relationship between things said in interviews and the circumstances of their production and projected circulation. My point is therefore not that all ethnographers uncritically recontextualize interview responses as direct expressions of inner selves but that the communicable ideologies that shape anthropologists’ understandings of interviewing often inform their attempts to scrutinize interviews critically and to infuse their products with authority and value.

The Janus-faced character of interviews constitutes ethnography as “immersement,” to use Marilyn Strathern’s (1991,



1999) term, a simultaneous presence “in the field” and “at the desk” such that each site partially inhabits the other. Interviewing reveals the doubling effect of this presence—anthropologists-as-interviewers continually speak to anthropologists-as-writers, situating texts simultaneously in readings of “the literature,” grant proposals, interviewer-interviewee interactions, conferences, and publications. Being both interviewers and writers affords temporal as well as spatial doubling, situating ethnographers simultaneously at the time of writing and at the time of interviewing; interviewees get stuck, of course, in the spatio-temporal confines of “the interview.” Since this doubling confers authority, it seems far from surprising that interviewees also attempt to position their discourse in multiple spatio-temporal junctures—Paredes’s Mexican American tricksters are similar to other interviewees in anticipating anthropologists’ audiences and trying to shape anthropological texts, their readings, and their social/political effects. The Janus-faced character also helps create and naturalize anthropologists’ impressive techniques of scalar projection, enabling statements by a few people “in the field” to be recontextualized as insights into humanity, globalization, violence, development, the body, etc. Interviews jump scales in both directions—from broad generalizations to interview questions and from responses back to generalizations—precisely because they inhabit multiple “levels” simultaneously.

One recent strategy for scrutinizing anthropology’s claims of authority has been to locate ethnography in terms of its similarity to other technocratic (legal, financial, etc.) endeavors in terms of methods, objects, and perspectives (see, e.g., Mauer 2005; Riles 2004; Strathern 2000). Once again, this reflexive move has not led scholars to scrutinize interviewing, one of the central overlaps between our disciplinary practices and those central to other scholarly fields, medicine, the law, criminology, social work, and journalism. Riles (2004) suggests that anthropologists can learn to “unwind” their own knowledge base—to distance themselves from the positivist assumptions that they share with the technocrats they study—by paying attention to the latter’s attempts to reveal the assumptions on which they rely. Exploring similarities and differences in interviewing ideologies and practices could become an important site for unwinding anthropological assumptions and practices.

My goal in this essay has not been to propose my own view of interview communicability and impose it as the “true” or “proper” way to conceptualize and conduct interviews or as a map of the way interviewing “actually” unfolds. It might be possible, however, to suggest some basic principles for complicating anthropological interviewing without falling back into the cookbook genre (teaching practitioners to bake better interviews):

1. Think of interviews not as events that take place in a particular spatio-temporal location but as dimensions of the larger set of practices of knowledge production that makes up the research from beginning to end. Instead of taking the nature and boundaries of “the interview” as self-evident and

knowable in advance, treat the role of interviews in the project as an object of inquiry.

2. Pay explicit attention to the communicable cartographies that inform interviewing, recognizing that they will be multiple and differentially understood and valued by different participants; researchers themselves embrace contradictory communicable logics.

3. Without pretending that power differences can be “leveled” or their discursive effects prevented, address interview communicabilities and practices explicitly with interviewees. Such exchanges are excellent ways of discovering where differences lie and opening up creative options.

4. Attend to ways in which interviewees and people who refuse to participate attempt to subvert the communicable cartographies and pragmatic constraints that researchers use in attempting to structure interviews.

5. Use this reflexive effort to inform the project as a whole, including its definition of the research problem and modes of discovery and analysis. Qualitative researchers can use insights into their own assumptions and those held by other participants in making changes throughout the life of the project. Quantitative researchers should use serious pretests as an opportunity for exploring communicable misrecognitions, misfits, and alternative cartographies rather than just for fine-tuning questions.

My concern has been to lay out a research agenda for an anthropology of interviewing and to suggest why all ethnographers might want to participate. Even if they never conduct interviews, anthropologists will miss fundamental insights into anthropological knowledge making if they refuse to think critically about the fact that ethnographic material generally presupposes and reifies particular types of interview ideologies and practices. I have further suggested that critically examining anthropological interviews requires thinking about how they are imbricated with other sites and forms of interviewing. Given the centrality of interviewing to contemporary life in the United States and many other countries, its critical examination would make a vital contribution to what Paul Rabinow (2003) calls an anthropology of the contemporary. Failing to undertake an anthropology of interviewing would continue to assert weak intellectual-property claims on behalf of Anthropology<sup>TM</sup> to widely distributed knowledge-making practices instead of examining how anthropologists share a productive if problematic terrain with a host of others.

## Acknowledgments

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## Comments

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In this rich and wide-ranging paper, Briggs develops a number of issues—all apparently centered around a discussion of “interviews”—that need to be separated, in my view, before their larger implications can be appreciated. We might begin by noting that although the interview appears as both hero and antihero in many of the cultural projects he discusses, Briggs does not, as he reminds us many times, seek to offer a theory of the interview himself. His focus, rather, is on the ideological uses of information gathered through discursive encounters of varied genres, of which “the interview”—in its strict sense—is that generic subtype around which many social-scientific practices are organized. His more specific focus is on a commitment central to modernist ideologies of language, the idea that if what a person utters in a social interaction is evaluated mainly for its “informational” content and reduced (through writing or other technologies) to sentence-propositions, such a reduction allows a particular kind of control over social history.

Such a reduction allows the one who formulates it considerable autonomy and distance from a specific encounter with an interlocutor (a historical episode) and from the series of encounters that precede and follow it (as a social process) simply because the social-historical datum has been replaced—or at least backgrounded—by a propositional model of one of its phases. And once sentence-propositions thus extracted are metapragmatically reframed and reformulated as psychological data (as samples of “beliefs, experiences, knowledge, and attitudes” attributable to known individuals) or as sociological data (samples attributable to entire groups), they can be reinserted in subsequent social history in a trajectory of interventions that appear to conquer time by replacing their referents with these samples, over and again.

In the hands of pollsters such techniques conquer space as well or, rather, replace a multisited space of social interactions with the univocal and placeless figure of “public opinion.” Thus transformed, the fuzzy boundaries of all of the settings in which social processes unfold are regrouped in relation to the space of a “public,” presumed to exist as the enabling topos of a national imaginary, and readily used to exclude some of these settings from its sphere, and all of the enacted stances and positionalities differentiated within and across these settings are statistically normalized as a collectivized

“opinion” used as a norm against which particular stances and positions appear extranormative.

Briggs argues that such modernist ideologies of language inflect the practices of many social sciences, including anthropology. But here the specificity of “the interview” can easily distract us from the range of genres upon which these ideologies work and therefore from the wider implications of this argument. As far as anthropology goes, all of the arguments that Briggs brings to bear on the interview apply just as easily to “participant observation,” insofar as the latter is also a method of sampling encounters and generalizing from samples. It is true that in contrast to the method of interviews, the method of participant observation does not require detailed reports of what was said in the encounters sampled. But this hardly means that the social encounters through which participant observation unfolds are any less mediated by uses of language than are interviews. It is simply that the role of contextualized language use is even less fully documented and analyzed when the results of this method are documented in ethnographic reports.

What, then, is the relevance of “communicability”—in Briggs’s sense of the term—to the social processes that anthropologists study and the social processes by which they report the findings of their studies? The five-point definition that Briggs gives of “communicability”—that it constructs models of social phenomena, that it unfolds as a process within social fields, that it projects cartographies of its circulation, that it locates itself in specific Bakhtinian chronotopes, and that it generates positionalities that may be inhabited or rejected by participants—all point toward a broader view of how processes of using language (and other semiotic forms) connect places and times populated by persons to each other and, as processes that unfold in a massively parallel multisited fashion, contribute through their interconnections to the action-shaped and interpersonally meaningful patterns we call “culture.”

If the kind of anthropology that Briggs discusses falls short by failing to overcome its attachment to pieces of this larger process or, indeed, by failing to grasp, both theoretically and practically, the existence of the larger process from which these pieces are drawn, these failures do not distinguish the social process of doing anthropology in this mode from social processes in general. Getting clearer about these larger horizons is the main challenge to which this paper gestures and to which the problematics of “the interview” draw our gaze only in an initial way.

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Once more Briggs interpellates our “anthropological wisdom”—those widely accepted dimensions of academic prac-

tice which are embedded in everyday certainties and routines. Twenty years ago he urged us to learn *how* to ask; now he invites us all to take another step and learn *what* to ask (of ourselves). He does so by means of a twofold movement: First, interviews are reflexively approached both as a means and as an object of knowledge production. Second, the critical analysis of interviews is proposed not only as a methodological but also as a theoretical challenge. Thus, although Briggs's title anticipates the purpose of questioning democratic illusions, he undertakes a through questioning of anthropological illusions as well.

Inspiring texts raise questions in several directions, but my comments can tackle only two of the topics about which Briggs's article has made me think and wonder: (a) the notion of communicability that is used to put interviews in historical context as an everyday genre in mass societies and (b) some lessons extracted from the "promiscuous"—in Briggs's terms—act of comparing anthropological interviews with the practices of other disciplines and professional realms that also aim at making sense of life/reality/society through a question/answer format.

To understand the social life of discourse, I find imperative the five components that, according to Briggs, are involved in communicability. I consider effective as well his framing communicability against the "three ideologies of language, subjectivity, and knowledge" which "become modes by which we assess ourselves and others with respect to how well we are 'communicating.'"

However, the epistemological status of Briggs's notion of communicability remains awkward. Is it an ideological product—the result of hegemonic metapragmatic discourses about social discourse that aim at being "readily communicated and understood"? Is it instead the process by which "texts and ideologies find audiences and locate them socially/politically"? Is it rather a practice, as when Briggs states that "interview communicabilities sustain democratic ideologies"? Or is it mainly an anthropological trope for condensing a critical theory of discourse à la Norman Fairclough—a theory approaching discourse as a text, as a discursive practice, and as a social practice and acknowledging that any discourse performs textual, ideational, social, and identity functions? We have critical theories of discourse that aim at such a condensation. I believe that Briggs's major contribution may be that of identifying communicability as an epochal metapragmatic standard based upon the disputed yet prevalent linguistic ideologies that feed our democratic illusions, varied as they are. If this is the case, I wonder if Briggs's bottom line is that interviews can be seen, in a Foucauldian sense, as modes of knowledge production and normalization that play for modern life and contemporary society the role that confessions played in the Middle Ages—a discursive genre resulting from and feeding current hegemonic forms and fields of communicability much as confessions resulted from and fed the supremacy of theological truths.

Regarding the shared and distinctive features of anthro-

pological interviews as compared with interviews with "distinct pragmatic and ideological underpinnings," Briggs identifies an interesting tension. On the one hand, interviews show "a number of widely shared features" because of "deeply held feelings/ideas about how we produce discourse." On the other hand, since "there is no one single field of communicability associated with interviews," we can assume that interview communicabilities are part of different fields and that interviews perform distinctively in each of them, despite their "widely shared features."

In this last regard, Briggs claims that anthropologists are "more naïve than many of their fellow producers and consumers of interviews." But is it a matter of naïveté? Or is it instead a matter of agenda and of the linguistic games that the fields within which we constitute ourselves as subjects allow us to play? I introduce here Wittgenstein's idea of linguistic games, but I mean the routinized forms that we are trained into for entextualizing and contextualizing interviews, forms that bring about performatively the distinctive pragmatics of different fields of communicability.

Moreover, if—as Briggs states—"communicabilities project power," it seems to me that communicative hegemony *sensu* Briggs projects differential power not only within each field but also among different fields. As a result, different fields of communicability and strategies with them are not alike in rank when it comes to producing and recycling commonsense understandings. From this point of view, Rigoberta Menchú's case shows not simply "the epistemological pitfalls of communicability" but power disputes to achieve/protect/define discursive hegemony within the anthropological field of communicability and practice. I therefore wonder whether "the I, Rigoberta Menchú affair" is a leading case for analyzing the epistemological pitfalls of communicability or rather a proxy for the stakes of academic politics.

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Briggs provides a timely reminder about the limitations of the interview as a method in anthropology and its appropriation by the media and by other academic disciplines. While I agree that anthropologists should attend to the way in which interviews "produce subjects and objects, texts, and authority," I think that a focus on interviewing is too narrow. I would argue first that some of the problems inherent in interviews may be offset by "triangulation" of interviews with participant observation, life histories, archival research, visual methods, etc. Secondly, while the interview is clearly more enmeshed in the "public sphere" and everyday notions of "communicability" as outlined by Briggs, the other methods we use are subject to similar constraints and forms of appropriation. Rather than seeking to document this latter

point, I turn to the nature of the research training which has shaped the current generation of graduate students.

Over the past decade postgraduate training in anthropology in the UK has undergone a major transformation. First, the reduction of financial support has meant that fewer British students can afford to obtain a Ph.D. Second, the research councils which define the nature of disciplinary training have substantially shortened the training/registration period: students must now obtain their Ph.D. within four years of registering. Third, students are formally introduced to research methods via a taught course and have little time to learn the craft before going into the field for 12 months or less. Concurrently, there has been a blurring as disciplinary boundaries as other disciplines have “appropriated” ethnography or ethnographic methods to suit their needs. Anyone familiar with sociology, history, development studies, the sociology of medicine, etc., will be aware that the form of “ethnographic research” championed by these disciplines often bears only a limited resemblance to anthropology (though of course there are notable exceptions). Indeed, the same criticism could be made of anthropology, which has, at least partly under the influence of the funding councils, “appropriated” participatory research, focus groups, and life histories (ESRC 2005). Given the limitations on funding for postgraduate training, it does not come as a surprise to find that some of our students fail to realize that research practices “produce subjects and objects, texts, and authority.”

Our inability to change the policies of the funding councils leaves us with limited choices. Briggs provides one approach by restating the limitations on the use of interviews which anthropologists should be aware of. Such an approach is to be welcomed and is certainly necessary; it is something many of us attempt to do from time to time (Campbell 2001, 2006). However, in view of the constraints which shape postgraduate education, it is unlikely to be enough. It is also necessary to rethink the way we train our research students. Large anthropology departments in the UK that have succeeded in retaining core funding from the research councils are better placed to teach their postgraduates. This is because we have relatively fewer students and it is possible to draw upon a larger staff to teach, advise, and supervise research students. Even so, it is not clear whether the training we provide adequately grapples with the problems identified by Briggs or, indeed, whether we provide the kind of training our students need. Resource constraints and growing bureaucratic requirements (including various audits and the research assessment exercise) placed on departments continue to limit training in research methods. With the encouragement of the funding councils, smaller anthropology departments have sought to maximize resources by joining up with other social science departments on a regional basis to train postgraduates. However, if interdisciplinary efforts result in a generic approach to research at the expense of instilling in a student an awareness of the strengths and limitations of disciplinary practice, the result is likely to be problematic. Ideally, social science

postgraduates and staff should have the opportunity to be properly trained in a range of research methods, but this is not happening, and the result is an eclectic and problematic appropriation of methods that produces problematic results (cf. Campbell and Holland 2005).

Briggs’s argument that interviews are inextricably enmeshed in and framed by popular discourse about “the individual,” the public sphere, and the search for authentic public voices is well taken. We should be constantly questioning the assumptions which underpin our methods, whether we rely on interviews or use interviews with other methods, while simultaneously examining and embracing the limits of our knowledge.

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Briggs points to the importance of keeping in mind that interviews are not just systematically and artfully completed at a particular time and place but in a sense walk about the lives of those they are about and those who are interested in those lives.

Sociologists especially have told us a great deal about the technical reliabilities and validities and the everyday pragmatics of interviewing, having examined its use not only in scientific surveys and other research interviews but also in innumerable practical circumstances such as doctor-patient interactions, legal proceedings, and news gathering. From this literature it is clear that interviews are complex, collaborative enterprises that entail activity well beyond information transfer. Interviews are not just windows on the world of experience but create windows of their own as an artifact of the encounters in which they unfold. So much for one side of interviewing’s democratic illusions.

There is another side that anthropologists would do well to examine critically. Sociologists’ instinct is not to venture too far “afield” from the interview encounter. They typically have not walked about the interview encounter, except of course to get there and to leave. Meandering through the communicative cartographies of interviews sometimes takes a great deal of time. Besides, it is likely to be inspired by another research agenda, a concern for what Pierre Bourdieu might refer to as the “field” of the interview process and its information. In fairness to some qualitative sociologists and anthropologists concerned with the everyday pragmatics of knowledge, I should add that this is a research focus to which Briggs, Richard Bauman, Dorothy Smith, and others concerned with the multisided ethnography of interviewing have made important contributions.

If we are to take seriously the idea that interviews are not only about but also part of society (see Atkinson and Silverman 1997), we need to ask ourselves questions that relate to

participants' activities (Gubrium and Holstein 2002). What do the respondents who produce the knowledge they provide in interviews take account of in the process? How do interviewers formulate for themselves and for others the information they receive? Of particular pertinence is the question of what social landscape this information circulates in and how stakeholders shape that information for various purposes. If Elliot Mishler and others have drawn attention to political asymmetries in interviews, we should extend this to the intertextual fields within which interviews and interview information are embedded.

Briggs's own work on the ethnography of information is instructive and eye-opening. Let me add one example from my own research on the social organization of accounts in human service institutions to make a point. A narrative ethnographer by trade, I have been in the habit of listening to and following personal stories in various social settings, many of which derive from interviews. (I am not kind of narrative ethnographer who is especially interested in my own story, but I do recognize the contribution of those who are [see Gubrium and Holstein n.d.].) The point is that rather than agonize endlessly about the ultimate truth of interview information we would do well to ask ourselves what shape the interview material produced in society takes as it moves about our own and others' lives.

Years ago, in ethnographic fieldwork in a residential treatment center for emotionally disturbed children I called "Cedarview," I observed the social construction of the disturbed child. There was a great deal of interview material presented in the process, and my aim was to document how the information was transformed into accounts of emotional disturbance. While the information was discussed and debated at length by staff members, it was also subject to the perennial question of "how to put it" in both internal reports and reports submitted to outside agencies. The representational work entailed was precisely part of the communicative cartography of the information. This was not a question of laundering or otherwise biasing information but rather a matter of taking account of the field of stakeholders to which the information was pertinent. Staff members were good at what they did precisely because they considered the issue in their reportorial work. Interestingly enough, representatives from outside agencies would occasionally participate in meetings in which interview material was presented and offered friendly advice to staff members on "how to put it" for acceptable outcomes when submitted to them. In Harold Garfinkel's (1967) words, there were good organizational reasons for "bad" interview information.

By now, the idea that meaning is not only contextual but intertextual and multisited is a principle of interpretation. What we need more of is concerted documentation of the extended field apparatus that realizes this, especially in relation to interview material. Ethnographers, who rarely suffer democratic illusions, are particularly well suited to the task.

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The reporter, wrote a German visitor to the United States in the 1920s, has "become almost a public figure. In middle and small-sized towns, even private travelers are being attacked by reporters and are asked the most incredible questions." The news interview, as Schudson (1995) has shown, originated in the United States in the late nineteenth century, becoming institutionalized as a knowledge-producing practice in the press by about the end of the 1870s. It was connected with the rise of a mass-circulation commercial press and the transformation of journalism from a form of political rhetoric into a form of "impersonal surveillance," as Schudson puts it—a means for circulating information about social life to a public of anonymous spectators. It played a central role in the emergence of journalism as a profession and of news as a distinct form of knowledge and culture. It helped to differentiate the reporter from the editorialist and thus to differentiate the journalistic field from those of politics and literature, to which the editorialist, once the dominant figure in journalism, was closely connected. It made the journalist, as the German visitor observed, a public figure, giving journalists standing to demand comment regardless of barriers of status and authority or boundaries of privacy or group membership.

In communication and media studies as in anthropology, systematic research on the interview is relatively rare. Besides Schudson's historical account, the main research has been done by interactional linguists (Clayman and Heritage 2002), who show the ways in which institutional norms (e.g., of journalistic neutrality) are jointly performed and sometimes contested by interviewer and interviewee.

Media interviews are not all of a piece. Live television and radio interviews are forms of public performance, unlike the telephone interviews that print reporters use to gather information. "Hard news" interviews are very different from the kinds of interviews (e.g., with celebrities) that dominate "soft news" programs. Soft-news interviews enact intimacy rather than distance. Hard-news interviews deviate dramatically from the practices of everyday conversation; the rigidity of turn-taking in hard-news interviews, for example, serves to validate the neutrality of the interviewer (Clayman and Heritage 2002). Interviewing conventions vary across cultures, historical periods, and situations in ways that, as Briggs suggests, can tell us much about shifting ideological assumptions about public communication and who has a right to participate in it in what way. As Hall (1973) observed, a journalist does not necessarily interview a militant trade union leader in the same way he or she interviews a cabinet secretary.

"Interviewing," as Briggs says, "constitutes a central means for sustaining the sense that each citizen has the right to participate, be heard, and affect collective life." In some sense,

the interview clearly has democratizing implications, or at least a tendency to dissolve social hierarchy. The fact that political officials have to submit to public questioning by journalists is part of the process by which the mass public is integrated, obviously in mediated ways, into the structure of political power; it is connected historically with the rise of mass parties and also of mass warfare, in which public opinion was a central strategic concern. The kinds of power relations enacted in interviews vary widely, however. In the sixties and seventies, for example, there was a shift from often deferential toward more adversarial interactions between journalists and officials. One manifestation was the drastic shrinking of the "sound bite" in TV news as journalists asserted their right to chop the words of interviewees into increasingly smaller fragments (Hallin 1992).

The journalist, like the anthropologist, also has a deeply ambivalent relationship to those at the bottom of the social hierarchy. Journalists often see their role in terms of giving a public voice to ordinary citizens and to people on the margins. Briggs's discussion of his prison interviews recalls what Lincoln Steffens (1931, 317) said about crime reporting:

Our stated ideal for a murder story was that it should be so understood and told that the murderer would not be hanged, at least by our readers. . . . It is scientifically and artistically the true ideal for an artist and for a newspaper: to get the news so completely and to report it so humanely that the reader will see himself in the other fellow's place.

Yet journalists are often selective about when and how the ordinary citizen is admitted to public discourse; sound bites of politicians in election coverage shrank to 9 seconds by the 1980s, for instance, but those of voters shrank to 4 seconds. And the representation of the powerless in news is as often a form of exploitation or commodification as a means of giving voice. Much of what Sontag (1973, 110) writes about the camera can apply as well to the microphone or notebook: "Its main effect is to convert the world into department store or museum-without-walls, in which every subject is depreciated into an item of consumption."

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Briggs invokes Silverman's (1993) idea of the "interview society," the notion that in modern societies the interview is a ubiquitous discursive form. In part, Silverman meant this term to be something of a rebuke to his fellow sociologists. Much as Briggs does, he wanted researchers to look critically at the interview as a practice and to understand the ways in which that practice is culturally and socially embedded. One reason to make this point is that the work in qualitative sociology that Briggs seems to see as something of a model

for anthropologists has sometimes taken the notion of the interview society as a source of legitimation for a preferred model of interviewing rather than as a call to inspect in a critical way our reliance on particular methods. From this viewpoint, the interview is seen as *interactionally* unproblematic because in the interview society people have become enculturated in such a way that they know what an interview is and what is required of them within it. Briggs is aware that there is here a strongly Romanticist ideology of the interview that might have repaid some more detailed investigation.

It would be foolish to question the ubiquity of the interview, but a more questioning view might still be useful. In the United States, surveys of survey participation do indeed suggest very high rates of "exposure" to the interview. Such figures need, however, to be adjusted for the bias that derives from asking people who, through their agreement to participate, have already displayed a willingness to be interviewed (Bickart and Schmittlein 1999). Making such adjustments tends rather to shift the perception of the all-encompassing interview, as does the well-attested decline in survey response rates across most Western countries. Moreover, in the era of "reality television" and "citizen journalism," the place of the interview seems rather less ensured than it was. It might be the case that rather than social scientists' aping the media, the media in their search for ever greater "authenticity" continually appropriate social science formats such as the interview and the observational study.

Briggs tends to homogenize the interview by focusing on interviews, such as research interviews, the knowledge from which is intended to be dispersed widely beyond the immediate social context in which it is produced, as opposed, for example, to job interviews or doctor-patient encounters. The point here, of course, is not that interviews of the latter kind fall beyond Briggs's purview. It is that differentiating between kinds of interview helps us form a clearer view of how the interview came to be embedded in society. For example, in the early years of the twentieth century, interviewing candidates for city-funded positions was an important reform aimed at mitigating the effects of the "spoils system" when political administrations changed. However, even with other kinds of interview, the differences between types may be as important as their similarities. Compared with research interviews, media interviews are usually short, are not theoretically driven, and are rarely compared systematically one against another. Even when long interviews take place, there is often an unstated but well-understood agenda from which both sides of the interview profit (e.g., audience ratings for one, publicity for the other). In many cases, it is precisely factors of this kind that drive the dissemination of the interview beyond its original production.

It is probably no accident that the growing popularity of the interview as a research method coincided with the widespread availability of portable tape recorders. Being able to capture data unproblematically in real time preserves the appearance of the interview as a spontaneous, revelatory, and

naturalistic event. An important parallel shift involves the extent to which the tape recorder has become a device not for recording sound but for producing text. It is perhaps strictly speaking such texts that are disseminable from the interview. While Briggs recurrently refers to the "interview," it might make rather more sense to see the transcripts, tapes, and disks that recording makes possible as the immutable mobiles that allow interview-elicited knowledge to circulate in the manner Briggs describes.

"How can we do better interviews?" is, of course, not a bad question. Our familiarity with the interview as a method, coupled with its ease of recording, has helped deproblematize the interview itself as a dynamic, interactional, and, one might also want to say, motivated encounter. One is suggesting here not a return to positivistic notions of "technique" but a concern not to lose precisely the kind of detailed, reflexive practice on the interview itself that Briggs himself in his early work brought to our understanding of it.

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According to Briggs, anthropologists have tended to use interviews as a non-problematic, even preeminent, method while playing down issues of "power and representation." As he acknowledges, this is puzzling, since for some years sociologists have acknowledged, with varying degrees of enthusiasm, the following propositions:

1. Interviews do not give us direct access to the "facts" or to events (Rapley 2004; Kitzinger 2004).
2. Interviews do not tell us directly about people's "experiences" but instead offer indirect "representations" of those experiences. As Byrne puts it, "Few researchers believe that in the course of the interview, you are able to 'get inside someone's head.' What an interview produces is a particular *representation or account* of an individual's views or opinions" (2004, 182).
3. The interview is collaboratively produced: both interviewer and interviewee use their mundane skills. The interviewee is not a passive "vessel waiting to be tapped" (Gubrium and Holstein 2002, 151). The other side of the coin means that it is somewhat naive to assume that open-ended or non-directive interviewing is not in itself a form of social control which shapes what people say (Hammersley and Atkinson 1983, 110–11).

However, the "open-ended" interview remains the most widely used form of data collection in sociology. As Briggs recognizes, perhaps the attractiveness of the interview method to social scientists reflects the extent to which we all live in an interview society in which this format is continuously reproduced throughout everyday life (Atkinson and Silverman 1997). Briggs extends this analysis, arguing that "inter-

viewing is a key point of convergence with marketing and media consultants, reporters, other technocrats, writers, and NGOs in many societies, including the United States." In doing so, he lays out a fascinating research agenda for what he calls an "anthropology of interviewing." To this end, he develops a broader understanding of interview pragmatics and details two cases in which non-anthropologists seem to have developed much broader and more sophisticated interview communicabilities than are common among anthropologists.

Using his own research in Latin America, Briggs proposes a more democratic version of the interview which allows respondents to portray themselves as "complex subjects." In his conclusion he offers a valuable set of basic principles for "complicating anthropological interviewing." Consistent with Holstein and Gubrium (1995) and Rapley (2004, he shows how interviewees fashion their categories from researchers' categories (e.g., "Tell me your story") and activities (e.g., "uh huh").

Briggs undoubtedly offers a valuable deconstruction of the interview. But what follows? If categories are utilized in particular contexts rather than simply pouring out of people's heads, any method we use (even content analysis) cannot transform what interviewees say into anything more than a category used at a particular point in some interview. It follows that, if we are interested in institutions other than interviews, we should study those institutions themselves. As Harvey Sacks put it, this means "attempting to find [categories] in the activities in which they're employed" (1992, 27). Sacks's detailed arguments have largely been ignored by most qualitative researchers, but it is wrong to assume that this means that Sacks is completely out on a limb. In particular, some influential contemporary ethnographers contest the conventional assumption, deriving from the early work of Howard Becker, that interviews give us direct access to people's perceptions and that the role of observation is merely to see if such perceptions and meanings are "distorted" (Becker and Geer 1960).

For instance, in a book devoted to the writing of ethnographic fieldnotes, we find the following pointed comment: "Ethnographers collect material relevant to members' meanings by focusing on . . . naturally occurring, situated interaction in which local meanings are created and sustained. . . . Thus interviewing, especially asking members directly what terms mean to them or what is important or significant to them, is *not* the primary tool for getting at members' meanings" (Emerson, Fretz, and Shaw 1995, 140). Following Sacks, we can carry this argument even farther. Jonathan Potter (1996, 2002) has roundly criticized researchers who use his own approach (discourse analysis) for depending too much on interview data and has argued for greater use of naturally occurring data (see also Silverman 2007). Potter shows how interviews, experiments, focus groups, and survey questionnaires are all "got up by the researcher." Instead, he proposes what he humorously calls The Dead Social Scientist Test. As he describes it, "The test is whether the interaction

would have taken place in the form that it did had the researcher not been born or if the researcher had got run over on the way to the university that morning" (1996, 135). None of this commentary is intended to assert that interviews can never be useful or revealing. However, following Potter, I suggest that "the justificatory boot might be better placed on the other foot. The question is not why should we study natural materials, but why should we not?" (2002, 540).

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In his book *The Weight of the World: Social Suffering in a Contemporary Society*, Pierre Bourdieu poses the following questions (Bourdieu et al. 1999 [1993], 1):

How can we offer readers the means of understanding—which means taking people as they are—except by providing the theoretical instruments that let us see these lives as *necessary* through a systematic search for causes and reasons they have for being what they are? How can we give explanations without pinpointing individuals? How can we avoid making the interview and its analytic prologue look like a clinical case preceded by a diagnosis?

He emphasizes that research must serve "understanding," and after 600 pages of interviews with people who suffer he concludes that "a reflex reflexivity," a sociological "feel" or "eye," is more important than systematic methodologies or handbooks (p. 608). Being aware of what we do when we penetrate the private lives of our interviewees and of the constructions we produce, not the banal debate between positivists and critical scholars, is, for Bourdieu, constitutive of social science research. I believe that Bourdieu is right, and so is Briggs in his critique of much interviewing in anthropology, sociolinguistics, and the media. Briggs illustrates the pitfalls of interviewing when claims are made about access to "true, authentic, or real" opinions and when the power relations between interviewer and interviewee and the context of the communicative interaction, the dialogue, are neglected.

Interviews are spotlights on people's lives, beliefs, ideologies, and opinions—spotlights because they represent only a unique dialogue at a specific place and time—and, often enough, interviewees respond what they believe the researcher expects to hear. Michael Billig (2001) has, moreover, convincingly claimed that all of us produce ideological dilemmas; thus everyone endorses conflicting opinions and beliefs, sometimes throughout the same interaction and most certainly throughout a longer process.

That interviewees attempt to fulfil the expectations of interviewers is, of course, true in the case of surveys on subjects such as racism and anti-Semitism: Who would readily admit being racist or anti-Semitic? In Austria, for example, such

polls suggest that ca. 7% of the population respond explicitly to such questions with "Yes, I do not like Muslims, Jews, Turks, Roma, and so forth." To conclude, however, that only 7% of the Austrian population are prejudiced is certainly wrong, as other research, using other data and methods (Wodak et al. 1990), has clearly proven. Thus, a single source of data is never sufficient.

Interviews, however, also give people a chance to voice their beliefs and to tell their stories, which would otherwise never be heard. This is an important motive for much social science research, and in this way oral history interviews, interviews which capture everyday life experiences, interviews with marginalized groups, etc., have a salient function: to make the invisible visible and break the silence. As long as we are aware of what we are constructing, this function of interviews is very important, as Briggs also recognizes. Interviews are one way of producing access, of democratization in a broad sense. Other communication modes and genres, such as blogs, Internet discussion forums, chat rooms, and so forth, are also quite successful (Wodak and Wright 2006). These new genres have opened up more possibilities of networking if one has access to cyberspace and is computer-literate.

In conclusion, a true story: When I was researching a crisis intervention centre in Vienna 1976–80, I participated in staff activities and tape-recorded open group therapy sessions (Wodak 1986 [1980], 1996). I also interviewed the patients and the therapists several times and got to know them well. They told me a lot about their lives and their perceptions of the centre. Long after the study had been published, I met a former interviewee on the street, and he asked me why I had not used his name when quoting from his interview. I explained that this was because of ethical conventions, but he was hurt and angry: he wanted people to know that he had been able to cope with his crisis, and he was proud that his voice was being heard and read. This story illustrates why interviews remain important, albeit under the conditions spelt out above.

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## Reply

I looked forward to reading these commentaries with a mixture of eagerness and anxiety. Anthropologists have not, by and large, laid very effective claims to producing authoritative statements about interviews. Crossing terrain that is largely dominated by sociologists is a precarious business. To have one's efforts scrutinized by some of the very sociologists who have shaped critical understandings of interviewing, along with leading scholars of the media (Hallin) and critical-discourse analysis (Wodak), not to mention fellow linguistic anthropologists (Agha, Briones), presents both a rare opportunity for a cross-disciplinary, truly international debate about the subject and a moment of scholarly vulnerability. I feel



privileged that my essay has occasioned these sophisticated responses, grateful for the questions they raise, and somewhat surprised that I was not smitten in a cross-disciplinary crossfire.

One of the major tasks for commentators is to broaden research trajectories, and my interlocutors have succeeded admirably. I have been chided for focusing too much on interviewing, particularly on research interviews. I am bemused by calls for the restoration of elements of the argument that prepublication reviewers thought superfluous, a reminder of the complex interdiscursive processes that shape journal articles. Rather than attempting to defend my text, I would like to accept the challenge to enlarge its scope—indeed, it seems necessary for my project. *Current Anthropology* enables writers to gauge the ways in which their words might be received. In some of the comments I see emerging a familiar reading, most clearly articulated by Campbell, that I am “re-stating the limitations on the use of interviews which anthropologists should be aware of.” He points out that this is nothing new. Having attempted this task 21 years ago, I would heartily agree. I am only the author, and I will lose my control over the text in a few hours. But if I have any say in the matter, I would like to discourage this reading in favor of placing essay and commentaries in a larger frame.

Let me draw an analogy to the “poetics and politics of ethnography” critiques of the 1980s (see Clifford and Marcus 1986; Marcus and Fischer 1986). Critics identified the ideological—and, I would add, communicable—construction of ethnography, in which the ethnographer was fashioned as the perfect listener: questioning the premises of her/his “own culture,” traveling to foreign spaces, extracting words, customs, objects, etc., synthesizing them as a coherent “culture,” and bringing them to other “modern” readers by constructing texts using standard tropes and rhetorical structures. Clifford (1988) showed how ethnographies naturalized this communicable process in the poetic features of texts. Revealing these ideological-textual formulae and the way they erase the complexity and power differentials of fieldwork seems to have robbed anthropologists for a decade of their disciplinary jewels—ethnography and the concept of culture. Ethnography eventually resurfaced with new strength as a mode of inquiry that is valuable precisely in that its complex, problematic status becomes an explicit part of producing knowledge about culture, not just a weakness to be circumvented.

Unfortunately, interviewing did not figure prominently in these discussions, nor was it separately tried, convicted, and rehabilitated. Pointing out the complexity of interviews, warning of their problems, applying “use with caution” labels, and calling for their replacement with “natural materials” (Silverman) will not produce this sort of critical transformation. There are, I think, two vital steps required to reposition interviews not as problematic tools but as crucial and productive sites for deepening scholarly projects:

First, the poetics-and-politics critiques scrutinized ethnography as practices for knowledge-making and ideological

modes of representing them, and they explored relationships between ethnographic and other modes of producing knowledge. This process has been undertaken of late in such work as Annelise Riles’s research on Japanese technocrats, which explores “the points of affinity between technocratic and social scientific knowledge practices that provide the ground for premises about the nature of knowledge that are shared between anthropologist and technocrat” (2004, 394). As a result, I must distance myself from the commentators when they suggest that research interviews are no different from other research practices (such as “participant observation”). Recent research in conversation analysis and ethnomethodology shows that interviews are sites of collaborative knowledge production. Even in surveys, respondents and interviewers use multiple discursive practices in interactively accomplishing the tasks they are assigned rather than just enacting a single set of explicit procedures (Drew 2006; Maynard and Schaeffer 2000; Maynard et al. 2002; Myers 2004; Suchman and Jordan 1990).

The value of interviews thus emerges from their capacity to juxtapose diverse modes of knowledge production. Researchers draw on their specialized knowledge of research topics, other research activities (“participant observation,” for example), knowledge of the literature, experience with other interviewees, and so forth. Interviewees recontextualize knowledge drawn from multiple practices and then, with varying degrees of explicitness, represent how they produce it and what makes it interesting, credible, and important. In my research into why so many people classified as “indigenous” died of cholera in 1992–93 in eastern Venezuela, for example, Fernando Rivera (a pseudonym) projected his interpretation as emerging from shamanistic dreams, therapeutic interventions, clinic visits, and long experience of anti-indigenous racism (Briggs and Mantini-Briggs 2003, 242–44).

Thus, to Agha in particular I would respond that, if I examine it in depth, I cannot help but place interviewing within a broader set of knowledge practices. In order to document what other people claim to know, I need to attend to their representation of the way they acquired their knowledge. I access research foci through my own knowledge-making practices. Interviewing is “good to think” because it operates at a metalevel, collaboratively generating a unique intersection between knowledge-making practices without ever quite dominating them. Interviews thus pose issues raised by Riles and others, providing us with a field (to invoke Gubrium’s invocation of Bourdieu) in which these similarities and differences are ideologically projected. We thus attempt to ideologically isolate our knowledge-making practices, pretending that we can gain access to social life without engaging our own (unnatural?) modes of inquiry. Interviews are certainly not the only way of calibrating similarities and differences between knowledge-making practices, but, if we do them and interpret them well, they force us to attend to the way people construct these relations.

Here is the catch—the structure of the interview subor-

dinates, to varying degrees, other practices. Interviews break down when interviewees refuse to subordinate other sets of practices—to invoke them only insofar as they fit within and contribute to interviewing. In my initial research with artists in New Mexico, the Lópezes refused to subordinate the practices they used in inserting materiality into discourse and vice versa to my questions (on discourse and materiality, see Keane 2006). It was only when I subordinated interviewing to wood-carving, learning to carve and waiting until they talked about their work, that they allowed me to ask questions—about issues that they had already raised (Briggs 1986). In response to Briones, this is why I joined the debate on *I, Rigoberta Menchú*—because of its reduction of vast differences of race, class, and nation to the question whether Menchú was lying. The illusion of co-participation in the interviews enabled Menchú and Burgos-Debray to juxtapose a wealth of diverse practices of knowledge production in what could be construed as a single, specifiable mode of production, one that could be organized according to a referentialist, mimetic logic. Since Stoll's critics seem to share a similar cartography of the experience-to-interview-to-text process, it is hard for them to trace the transformation of struggles over voice, power, race, and history into a narrow politics of truth designed to discredit a leading figure in a struggle for social justice in which the stakes are higher than in "academic politics."

Second, several commentators consider my goal to be adding to our knowledge of the complex pragmatics of interviews. I did delve into one aspect of interview pragmatics. Gubrium notes that sociologists are reluctant to look beyond "the interview," and the same is true of conversation analysis and ethnomethodology. I argued that what is said in interviews is deeply shaped by discourse upstream and downstream as it recontextualizes previous discourse and anticipates how questions and responses will be recontextualized. But my objective in the essay was to leave pragmatics aside in favor of providing new perspectives on ideologies of interviewing. We have gestured in the direction of general characterizations of ideologies of interviewing for half a century (see Hyman et al. 1954), but researchers seldom document the ideological contours of particular interview-based research projects. Interviews not only juxtapose diverse knowledge-making practices but represent them and their perceived role in producing the discourse, ideologically configuring relations between these different modalities.

I do not claim to have exhausted our understanding of the ideological dimensions of interviewing; I have rather tried to draw attention to one facet of them. In response to Briones, let me nail down the epistemological status of communicability. Communicable cartographies project ideological representations of the pragmatics of particular interview projects. In the guise of simply telling us what is taking place, they construct interviews as producing information in particular ways, transforming it into specific kinds of texts (statistical summaries or life histories, for instance), circulating it to audiences, and projecting how it should be received. Lee's

reference to technology is intriguing; not just tape recorders but telephones, computers, the mass media, and the Internet contribute to the idea that interviewing extracts responses and inserts them into public discourse. Ideological constructions of "communicative technologies" help us extend and naturalize these projections, sustaining the illusion that an interviewee's voice is directly inserted into published texts or television news broadcasts. Hallin skillfully outlines how journalist ideologies of professionalism structure communicable cartographies of news interviews. At the same time that each communicable map purports to be unique, these projections draw on broader types of interview ideologies and basic ideologies of language and communication, as discussed above. *Pace* Briones, they include hegemonic dimensions in that researchers draw on the interview ideologies that dominate in their professions and use them to claim power in interviews and the authority to recontextualize responses; cartographies help institutions and their agents (pollsters for Gallup, professors in universities) to claim intellectual property rights over what emerges. They provide a basis for imbuing interview materials with value and authority, enabling 1,000–2000 brief telephone conversations to represent "public opinion" or enabling Oscar Lewis (1961, 1966) to jump scale from conducting life-history interviews to projecting a global "culture of poverty." Nevertheless, as Wodak reminds us, ideological projections are multiple and often competing, and they always offer at least some resistance to assimilating what is said to the researchers' map.

Researchers' communicable cartographies provide rather simplistic pictures of what is taking place. The idea that discourse springs from interviewees' heads or face-to-face interactions and then moves in unilinear fashion to the pages of professional publications draws our attention away from the intersection of multiple knowledge-making practices and their competing representation. The complex pragmatics of interviews do not conform to these maps. Nevertheless, post-interview procedures strip away the rich indexical traces of diverse knowledge-making practices in order to make the material seem to embody a particular social-scientific cartography. There are, of course, exceptions. Behar (1993) tracks conflicts and convergences between herself and Esperanza regarding how they were producing knowledge and where they thought that this material should travel. By removing the signs of these diverse ways of producing knowledge and imagined trajectories, we preserve the illusion that interviewing is a question of narrowly bounded "methods" that stand alongside others ("participant observation," "natural materials," etc.) as "tools" for producing information. Ethnographies become rich when their ethnographic projects are richly described—when their complexities as sites of knowledge production become sources of insight into the social worlds they explore. Reductionist communicable cartographies produce thin descriptions of interviews (if any at all) that hide their productive capacity as much as they preserve the intellectual

property rights of researchers and obscure the way they hierarchize modes of knowledge production.

By pointing to the need for an anthropology of interviewing and critical perspective on our use of interviewing to produce knowledge, I do not mean that all researchers need to fuss endlessly over minute features of interview transcripts or focus on interviews in lieu of what they are supposed to be documenting. What I do suggest is that we can learn a lot more when we come to view interviewing not as a magical passageway to other people's minds or as a problematic tool but as an excellent source of insight into the process of learning about what we claim to know.

This brings me to the question of pedagogy raised by Campbell. Graduate training has not been similarly curtailed in my neck of the woods. The problem I see is that our understandings of "methodology" or "research practices" are inadequate to the scale and complexity of the projects that my students undertake in examining how bodies, germs, capital, technologies, forms of violence, and media circle the globe and get woven into everyday lives, individual and institutional, wherever they go. The ways in which knowledge of avian flu is constructed by chicken farmers, clinicians, local public health officers, World Health Organization officials, congressional committees, journalists, politicians, geneticists, and economists are extremely diverse, but they intersect and interact constantly. No prescription for better interviews or calls for "multi-sited ethnography" (Marcus 1998) or the inclusion of "participant observation" and "natural materials" can solve this dilemma. I do not claim to have provided a silver bullet that can do the job. Tracking communicable cartographies does not enable us to transcend them or their limits—to free ourselves of ideological constructions of discourse. Entertaining a wider range of more complex communicable models, exploring how our communicable cartographies shape our interview projects and how we perceive them, and following other participants' maps can, however, provide a much deeper understanding of the production and presentation of knowledge. Treating interviewing as a highly productive site for exploring intersections between knowledge-making practices does, at least, enable us to begin to expand our understanding to match the complexity of the phenomena we are examining.

To Agha I would therefore reply that taking a more intense look at interviewing actually forces us to come to grips with the "larger horizons" associated with other ideologies, genres, and discursive practices. We can contribute significantly to our understanding of culture if we reposition interviewing not as just another tool or method but as a particularly illuminating site for querying the nature and limits of our own will to know, right in the midst of engaging it.

—Charles L. Briggs

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