

Reflections on Ethnographic Work in Political Science

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Abstract

The objectivist truth claims traditionally pressed by most political scientists have made the use of ethnographic methods particularly fraught in the discipline. This article explores what ethnography as a method entails. It makes distinctions between positivist and interpretivist ethnographies and highlights some of the substantive contributions ethnography has made to the study of politics. Lamenting the discipline's abandonment of a conversation with anthropology after Geertz, this review also insists on moving beyond the anthropological controversies so powerfully expressed in the edited volume *Writing Culture* (1986) and other texts of the 1980s and 1990s. I contend that interpretive social science does not have to forswear generalizations or causal explanations and that ethnographic methods can be used in the service of establishing them. Rather than fleeing from abstractions, ethnographies can and should help ground them.

INTRODUCTION

There is never nothing going on.

Thus teaches Socrates—the gas station attendant, not the philosopher—in an otherwise unremarkable movie called *Peaceful Warrior*. The phrase captures both the practical sensibility of many ethnographers and some of the problems ethnography poses as a method for the discipline of political science. Conventional political science tends to value parsimony, for example, but “there is never nothing going on” suggests the importance of richness, detail, and immersion. Ethnographers tend to view every happening as a potential moment for evidence gathering and/or rethinking the project’s premises. When textbooks are not ready for pickup at the Ministry of Education, despite repeated promises, that occurrence is a datum. When a meeting turns into an argument, a car breaks down in the local village, a television show is censored, an incendiary art exhibit is well attended, officials stop working at noon, statistics prove unreliable, a politician’s reasoning becomes garbled—these moments are all data for an ethnographer. When an interview does not go as planned—when people lie, evade, brag, or turn the tables on the interviewer, or discuss seemingly irrelevant material—that is also important information. What some researchers might consider obstacles can be a source of knowledge for ethnographers. But details can be messy and cause discomfort. They can also be tedious or too specific. Those in favor of ethnography celebrate the method’s disruptiveness. Others find such details unnecessarily distracting to the work of generalization. As Pachirat (2009a) argued recently at the Institute for Qualitative Multi-Method Research in Syracuse:

Ethnography as a method is particularly unruly, particularly undisciplined, particularly celebratory of improvisation, bricolage, and serendipity, and particularly attuned to the possibilities of surprise, inversion, and subversion in ways that other methods simply are not. If we think of the range of research methods

in political science as a big family, ethnography is clearly the youngest, somewhat spoiled, attention-seeking child, always poking fun at and annoying her more disciplined, goal-oriented, and outwardly-successful older siblings. Ethnography is the method who [*sic*] comes home to family reunions with the new mermaid tattoo, with the purple hair, with yet another belly button ring, and with a moody, melancholic artist for a girlfriend. At the dinner table, she is the method who interrupts her older brother’s endless description of his stock portfolio with tales of the last full moon party on Phi Phi Island in Thailand. Given that kind of unruliness, it’s no wonder that the older siblings and father figures of our discipline often revert to the language of “disciplining” and “harnessing” ethnography, of bringing her wild and unruly impulses under control by making her abide by the rules of the dinner table. In short, ethnography may be fun and exciting, but she might also get you excommunicated from the family.

One might take pride in the method’s unruliness, as Pachirat does, or attempt to fold ethnography into mainstream political science, as Laitin (1998, 2003, 2006) advocates. This article advances arguments that attempt to do a bit of both. At the risk of overindulging Pachirat’s metaphor, I want to keep the girl with the mermaid tattoo at the dinner table but on terms that enable a conversation, both spirited and respectful, that makes *all* interlocutors curious, generous, and alive to new possibilities (including the deromanticized possibility that ethnography, as some anthropologists have noted, may not be unruly at all). This review begins by exploring what ethnography entails. It makes distinctions between positivist and interpretivist ethnographies and suggests some of the substantive contributions ethnography has made to the study of politics. Lamenting the discipline’s abandonment of a conversation with anthropology after Geertz, this review also insists on moving beyond the anthropological controversies so powerfully expressed in Clifford & Marcus’s edited volume *Writing Culture* (1986)

and other texts of the 1980s and 1990s. I contend that interpretive social science does not have to forswear generalizations or causal explanations and that ethnographic methods can be used in the service of establishing them. Rather than taking flight from abstractions, ethnographies can and should help ground them.

WHAT IS ETHNOGRAPHY?

Despite important disagreements among ethnographers about what the practice entails, most concur that ethnography involves immersion in the place and lives of people under study. It requires a commitment—what some call a distinct “sensibility” (Pader 2006, Yanow 2006, Schatz 2009b)—to chronicle aspects of lived experience and to place that experience in conversation with prevailing scholarly themes, problems, and concepts. Ethnography also connotes a specific set of activities, such as “learning a local language or dialect; participating in the daily life of the community through ordinary conversations and interaction; observing events (meetings, ceremonies, rituals, elections, protests); examining gossip, jokes, and other informal speech acts for their underlying assumptions; recording data in field notes” (Bayard De Volo & Schatz 2004, p. 267). The term “participant observation” is often used as shorthand for the double nature of these activities, in which a researcher is both an actor and a spectator. [See Pachirat’s (2006) sophisticated formulation. For a critique of the concept of participant observation, see Bourdieu (1990).]

Although much ethnographic work, both within and outside political science, tends to be interpretive, some is not (Kubik 2009). Moreover, what counts as interpretive, or whether the term best describes the myriad of approaches subsumed under the rubric, remains questionable. “Interpretivism” can refer to divergent methodologies (e.g., structuralism, hermeneutics, deconstruction, and poststructuralism), as well as to various techniques (e.g., semiotics, discourse analysis, ordinary language use analysis, and ethnography). These two dimensions—

ethnography’s relationship to interpretive social science and establishing what interpretivists share despite their differences—require elaboration.

Noninterpretive and Interpretive Ethnography

What distinguishes interpretive from noninterpretive ethnography? Take, for example, anthropologist Bruce Kapferer’s (1972) network analysis of an African factory. Kapferer reduced interactions (which were themselves taken largely at face value) to abstract network morphologies that were then used to explain political outcomes such as strikes. As John Comaroff has explained to me (Wedeen 2009, p. 92), structural functionalists likewise asked questions to obtain “native” data on practices:

Native responses were taken as indicative of the values they [informants] held. Nobody asked why, or what those statements meant, or what motivated them. The researcher then compared informants’ answers to patterns of social practice in order to devise ethnographic generalizations that could then be narrated (by the researcher) as “systems” and “structures.” For example, the ethnographer might ask informants what the rules governing devolution of rank were. If the natives said that male primogeniture was the pattern, then that was the ethnographic generalization. The researcher then counted cases in which it happened, and noted the deviation from the pattern, and how it was dealt with—and then wrote secondary rules. The result was ethnography. Again, there was no discussion of what those rules meant (in the case of rules involving devolution of status, an interpretive account might have included the rearrangement of power relations or a discussion of factional alignments). In these noninterpretive ethnographic accounts, the outcomes tended to be rationalized to fit the rules *ex post facto*. An interpretive ethnographer, by contrast, would look for the meaning of these rules in political communication, in the restructuring of power

relations, in public discourses about staffing regimes, etc. Despite variation among interpretivists, most contemporary ones would see these rules as variable, historically constituted, and subject to risk. To put it plainly: noninterpretive ethnography focuses on presumed values, and then looks for structure and system. An interpretive ethnography centers on meaning, and at least in many instances, on process and history.

This contrast between structural-functionalists and interpretivists in anthropology gets at important distinctions between noninterpretive and interpretive ethnographies. It also suggests how salient interpretive concerns can be to the study of politics, for a focus on meaning and context enhances our analyses of political communication and power. The contrast also brings to the fore a key difference between anthropology and political science. Most self-described “positivist” political scientists who conduct intensive field research (e.g., Allina-Pisano 2008; Wood 2003; Laitin 1986, 1998) do not appear to be directly influenced by structural-functionalist analyses, nor do they discuss the tradition against which scholars of the “interpretive turn,” such as Geertz, were writing. But they, like most field workers in the discipline, were exposed to Geertz’s version of interpretive social science, and in the case of the early Laitin (1977), were beholden to Wittgenstein and J.L. Austin as well. Scholars can thus incorporate certain aspects of interpretive analysis into their work in a way that the structural-functionalists of old could not have. They can even argue for the “complementarity” between ethnography and rational choice (e.g., Laitin 2003, p. 175; 2006, p. 27) without attending to some of the epistemological contradictions and incompatibilities that mixing methods might involve, a point to which I return below.

Whereas most contemporary anthropologists disavow the naturalist assumptions that informed earlier generations of field workers, many political scientists do not. Anthropologists question the possibility and desirability of

objectivity; avoid model building and hypothesis testing; attend to the ways in which disciplines can shore up the very unequal power relations they seek to describe or explain; and interrogate a presumed division of the social world into real, replicable observations and intersubjective “noise.” Political scientists who are committed to sustained fieldwork and who have read Geertz share important concerns with their interpretivist colleagues, including an attention to language, context, and meaning. Many tend, however, like structural functionalists in an older era of anthropology, to take language at face value, to presume shared values or common knowledge, and to treat fieldwork as raw data.

One of the reasons that Geertz can remain important to mainstream political science while subsequent trends in anthropology have been all but ignored is that Geertz named what he did science. He insisted, moreover, that the interpretive enterprise was capable of producing general knowledge about the human condition (Bunzl 2008, p. 55). Geertz rejected a natural-science understanding of what social science was, emphasizing that interpretive science made it difficult to come up with “explicit canons of appraisal” found in “biological observation” or “physical experiment” (1973, p. 24; cited in Bunzl 2008, p. 55). But, as the anthropologist Matti Bunzl rightly notes, Geertz’s interpretivism “did not mean that the production of anthropological knowledge proceeded randomly or that ethnographic research was nothing but subjective reflection. ‘Generality,’ to be sure, lay in the ‘delicacy’ of ‘distinctions’ rather than the ‘sweep’ of ‘abstractions,’ and ‘large conclusions’ could only be drawn from ‘small’ and ‘very densely textured facts’” (Bunzl 2008, p. 55 citing Geertz 1973, p. 26).

As much of anthropological work in the 1980s and early 1990s disavowed the importance of generality and came to celebrate specificity and complexity (e.g., Clifford & Marcus 1986), political science deserted anthropology, disparaging its reflexivity as “navel gazing.” Highlighting the situated and provisional nature of all interpretation, likening ethnographic

writing to fiction, placing the discipline's main method in doubt for its complicity with imperial projects, and championing self-reflexivity did not endear anthropology or ethnography to political scientists. The particular form that reflexivity took may have posed problems for anthropology as well, producing at times what anthropologists themselves have lamented in retrospect as "descriptive analysis of the most limited, self-referential, explanation phobic sort" (Comaroff, unpublished manuscript, p. 9) and reducing "ethnography to a solipsistic literary practice, one so obsessively reflexive as to be of no interest to anybody outside of itself, not even to its natives" (Comaroff, unpublished manuscript, p. 5; cf. Sangren 1988). With the exception of the prominent scholar James C. Scott (1977, 1985, 1990, 1998, 2009), few political scientists derived inspiration from anthropology's self-interrogation. And of course political science was encountering its own independent transformations. Whereas anthropology repudiated both structural functionalism and early interpretivist critiques of it, political science turned increasingly away from behavioralism and toward rational choice theory. The discipline of anthropology rejected many of the scientific aspects of the social sciences as political science embraced them anew. Ethnographic work, to the extent that it existed in political science, tended to be trimmed down to fieldwork interviews and/or subordinated to game theoretic models (on the latter see Laitin 1998, Smith's 2004 critique, Hopf's 2006 critique, Varshney's 2006 critique, and Laitin's 2006 rebuttal).

Recently, however, there has been renewed interest in ethnography in political science, perhaps best encapsulated by the edited volume *Political Ethnography: What Immersion Contributes to the Study of Power* (Schatz 2009a). The volume is clearly beholden to Scott, whose work, even when not explicitly ethnographic (e.g., 1990, 1998), is powered by anthropological theory. Scott has inspired students of interpretive social science and contributed to a burgeoning interest in ethnography. As he said in a recent interview in the *Chronicle of Higher*

Education (Glenn 2009), "most social science, it seems to me, is not permissible without ethnographic inquiry of some kind. You can't explain human behavior behind the backs of the people who are being explained. If you want to understand why someone behaves as they do, then you need to understand the way they see the world, what they imagine they're doing, what their intentions are." Political scientists (even ethnographers in the discipline)—not to mention historians—might take issue with some of these statements, including the assertion that ethnography reveals intentions or that intentions are graspable. But the point is simply that Scott's work has helped to set an agenda, one that has also been fortified by pressure from movements such as Perestroika to incorporate ethnography and other interpretive methods into the discipline. New textbooks, edited volumes, a National Science Foundation workshop, classes in interpretive methods, panels, and a methods café at the American Political Science Association's annual meetings—the latter spearheaded in large part by the efforts of Dvora Yanow and Peregrine Schwartz-Shea—have drawn institutional attention to the possible contributions of interpretive social science (and ethnography in particular) to the discipline. These efforts coincide with an ever-growing attention to methods more generally, and to a push for "multi-method" work, in particular.

No one has been as vocal as Laitin (2003, p. 175; see also 2006, p. 27) in calling for the "productive complementarity" among different conceptual and methodological orientations. For Laitin, "narrative" approaches (such as ethnography) are by themselves inadequate. When combined with large-*n* statistical work and formal models, however, they can help generate robust findings (discussed in Pachirat 2009b, Hopf 2006). Importantly, though, calls for productive complementarity tend to subordinate the epistemological concerns of narrative approaches to the aims of science. Ethnography is often deployed in the service of the very sorts of objectivist aims that current ethnographic approaches in anthropology and interpretive political science challenge (Wedeen 2009). In

order to understand the promises and pitfalls of adopting multiple methods that mix epistemological orientations, it is worth understanding what interpretivists have in common and how these commonalities shed light on current debates about multi-method research.

INTERPRETIVE SOCIAL SCIENCE

Despite the capacious character of the term interpretivism, there are four attributes that most interpretive social scientists share these days. (These four attributes are excerpted from Wedeen 2009, pp. 80–82, in slightly modified form.)

First, interpretivists view knowledge, including scientific knowledge, as historically situated and entangled in power relationships. Power is generally not simply about leverage in such accounts, but also connotes intersubjective relationships that are diffuse, omnipresent, and often acephalous. Foucault, for example, traces how power works in excess of state institutions or particular elites, operating through discursive processes that suffuse all aspects of life. Power passes through institutional space as well as microspaces of health, education, science, theories of language, ordinary communication, and so forth (see also Wedeen 2008). It is located in and generated through social scientific categories and the assumptions underlying them, in legal definitions of personhood and their widespread dissemination, in the administrative routines of colonial bureaucrats, in psychological understandings of madness, sex, and family, in practices of worship, activities of peer review, etc. In this sense, power is hard to measure, although it is observable. Moreover, observations are not objective or external to the conditions that produce scholars doing the observing, but this does not mean they are unreliable—a point to which I return below.

Second, interpretivists are also constructivists in the sense that they see the world as socially made. The categories, presuppositions, and classifications referring to particular phenomena are understood as manufactured rather than natural. There is no such thing as ethnicity

or race, for example, outside of the social conditions that make such classifications meaningful. What counts as a phenotypical distinction or a “cultural difference” is a product of the discursive and institutional environment within which such distinctions make sense. The title *How the Irish Became White* (Ignatiev 1995) exemplifies this interpretivist sensibility. Although a number of social scientists avow constructivist commitments, the radical constructivism of many interpretivists entails privileging the history of categories over the fact of groups (Brubaker 2004). For example, instead of studying the history of homosexuals, this approach advocates studying the history of the category “homosexual,” i.e., how the category’s emergence and repeated invocation helped summon the group into existence. An attention to classification invites interrogating how social scientists themselves stabilize or fix categories of group affiliation—how analysts help to produce groups as substantial entities through their scholarly or policy-making (see Yanow 2003) practices. In this sense, the task of an interpretivist is often to analyze the sort of work done by categories such as black and white or Sunni and Shi‘i—that is, to analyze the logic of the relationships and the effects of the categories—while accounting for how they come to seem natural and taken-for-granted, when they do.

A third and related attribute is the tendency of interpretivists to eschew the individualist assumptions that characterize much rational choice and behaviorist literature. Although some interpretivists do stress the importance of agentive individuals (e.g., Bourdieu 1977), others question the very meaning of agency, or they compare divergent, historically contingent notions of what counts as agentive action (Butler 1997, Asad 2003, Mahmood 2005). Despite this range, no interpretive social scientist could assume, as many rational choice and strategic action theorists do, a maximizing or optimizing cost-benefit calculator who can be divorced, for the sake of general propositions, from actual historical processes. Ideas, beliefs, values, “preferences,” and decisions are always embedded in a social world, which is constituted

through humans' linguistic, institutional, and practical relations with others (Wedeen 2002).

Fourth, interpretivists are particularly interested in language and other symbolic systems—in what is sometimes termed culture in the literature. Despite conceptual ambiguities inherent in the term, promising developments in practice-oriented anthropology have led “culture” to be understood and operationalized as “semiotic practices.” Culture as semiotic practices can be thought of as an abstract theoretical category, a lens that focuses on meaning rather than on, say, the fact of prices or the tallying of votes (Sewell 1999, Wedeen 2002). It is not that votes and prices have no meaning, of course, but that a semiotic practical approach would study these phenomena in terms of the distinct meanings they index and generate, whereas an economist might take prices, and a political scientist votes, at face value. (Admittedly, some political scientists are interested in how and what votes signal; they would be well-served by engaging directly with the theories of signification central to interpretive social science, for such theories can help clarify relationships among thought, language, and action.) An understanding of culture as the production of meaning also refers to the work done by language and other symbols—how symbols are inscribed in activities that operate to produce observable political effects.

A number of political scientists have been interested in the work language and symbols do. In Laitin's (1977) analysis of language shifts in Somalia, for example, he argues that language not only reflects but also shapes the way people see the world. In the authoritarian circumstances of Syria, Wedeen (1999) shows how the flagrantly fictitious slogans characteristic of the leader's “cult of personality” operate to enforce obedience, induce complicity, produce the terms within which some resistance takes place, and tire citizens out, rendering prior political commitments patently absurd. Language and symbolic displays of power can be said to operate here as “independent variables.”

Language can also work to reflect, exemplify, or demonstrate important political phenomena,

such as how Senegalese (Schaffer 1998), Ugandans (Karlstrom 1996), or members of Chilean social movements (Paley 2001) understand what democracy means. In this sense, language can be said to function as a “dependent variable” (to use the vocabulary of conventional political science). Many studies show how language and symbols can do both—representing political phenomena and generating political consequences. For instance, Schaffer (1998) uses the example of Senegal to explore the differences between elite and ordinary citizens' notions of democracy. Schaffer finds that Senegalese elites tend to invoke the word democracy (or its French equivalent *démocratie*) in ways similar to the usage of many political scientists. A democratic system is one in which elections are contested and outcomes uncertain. By contrast, lower-class, less-educated Senegalese use the Wolof equivalent, *demokaraasi*, to mean “equality” or the attainment of “collective economic security via mutuality” (p. 85). Schaffer argues that the difference between the concepts of *démocratie* and *demokaraasi* reflects the existence of “institutions and practices that are only partially overlapping” (p. 85). Schaffer also suggests that varying meanings of democracy may have consequences for how elections are implemented and what reactions they elicit.

As the above discussion makes clear, ethnography adds value to political analyses in part by providing insight into actors' lived experiences—how workers on the assembly lines in Egypt (Shehata 2006) or in a meatpacking plant in the United States (Pachirat 2008) experience hierarchy, labor, and the possibilities of upward advancement; how Palestinian refugees in Lebanon understand heroism and martyrdom (Khalili 2007); how poor residents of Cairo make use of informal networks, marriage arrangements, and government subsidies (Singerman 1995); or how respondents to survey questions “interpret the issues they are asked to express opinions about” (Walsh 2009, p. 169). Ethnography is also an excellent way to gain traction on actions that at first glance might seem irrelevant or simply too ordinary for comment—such as the ways

in which foot dragging among peasants can be a mode of resistance (Scott 1985) or how quotidian social gatherings in authoritarian circumstances can be a form of democratic practice in the absence of a democratic regime (Wedeen 2008). Far from simply being a method of choice for those interested in comprehending ordinary happenings, ethnography can also be used to analyze exemplary events and ongoing dramas, including phony elections (Wedeen 2008), genocidal violence (Fujii 2009), protest (Gould 2009), and the political relevance of witchcraft (Schatzberg 2001, Bertrand 2002, Ashforth 2005). Ethnography is thus a compelling means to produce general knowledge about the “microfoundations of collective action” (Wood 2003, 2009, p. 199)—even when that action is stability-enhancing rather than world-upsetting or transformative. Ethnography is also able to address the difficulties of what Kuran (1995) called “preference falsification” by filling in the gaps between official demonstrations of obedience and ordinary experiences of unbelief (Wedeen 1999)—what Scott (1990) has famously called “hidden transcripts.”¹

Much ethnographic work in political science has been done by scholars in comparative politics (sometimes in conversation with political and social theory). The ease with which some comparativists embrace ethnography is no doubt due in large part to the subfield’s longstanding tradition of fieldwork and its vexed but ongoing relationship to area studies. Although participant observation techniques remain less accepted in American politics, some Americanists have recently adopted them (e.g., Fenno 1990; Glaser 1996; Soss 2000; Walsh 2004, 2007; and Warren 2005—all cited in Schatz 2009b). In doing so, these scholars follow a

previous generation of urban-politics specialists (largely in the American subfield), who derived inspiration from the “Chicago school” of sociology but did not pick up on its ethnographic trends. Instead, this earlier generation shared with the Chicago school a focus on the city, a left-leaning reformers’ vision of what was changeable, an interest in consent and social control, and an insistence on the importance of institutions, context, and history (e.g., Levi 1977; see also her comparative analysis, Levi 1997; Katznelson 1982, 1992. For a history of Chicago sociology, see especially Abbott 1999). In international relations, there has even been what scholars have referred to as an ethnographic turn. Social constructivists, in particular, have asserted that participant observation permits researchers to study the processes through which institutional norms are reproduced (Klotz & Lynch 2007). Citing Cohn’s (1987) work with nuclear-weapons scientists and Barnett’s (1999, 2002) experience as a political officer at the United Nations, Klotz & Lynch underscore how “specialized language socializes individuals into a bureaucracy” (p. 38).

According to Vrasti (2008), scholars associated with this move have been reluctant to learn from the troubled but productive conversations that have animated debates about ethnography in anthropology since the 1986 appearance of *Writing Culture*. Vrasti offers a trenchant critique of ethnographic contributions made by well-regarded feminist and social constructivist authors in international relations (e.g., Cohn 1987, 2006; Enloe 2000, 2001; Moon 1997; Neumann 2002, 2005; Pouliot 2007; Zabusky 1995). She laments the reduction of ethnography’s complexity to “an empiricist data-collection machine,” a “style of writing,” and a “theoretical sensibility,” which does not take advantage of ethnography’s “radical promise” (Vrasti 2008, pp. 279 and 300; for an exploration of possible connections between anthropology and radical anarchism, see Graeber 2004). But Vrasti’s plea for adopting the insights of critical ethnography, and for reading anthropological theory “post-Geertz,”

¹The notion of an “offstage,” where hidden transcripts circulate and resistance flourishes, and an “onstage,” where people perform an inauthentic self, problematically presumes a genuine self. For this reason, some scholars (e.g., Mitchell 1991, Abu-Lughod 1990) criticize the use of theatrical metaphors to describe politics. Scott also presumes that the regime is unaware of these transgressive practices, that they are “opaque” except to the researcher. Wedeen (1999) demonstrates otherwise.

oddy has her stuck in that fertile but rather dated debate of the 1980s, when path-breaking books such as *Writing Culture* (1986), *Anthropology as Cultural Critique* (Marcus & Fischer 1986), *Tales of the Field: On Writing Ethnography* (Van Maanen 1988), and *The Anthropology of Experience* (Turner & Bruner 1986) helped reconceptualize ethnography's political implications.

This article joins Vrsti in appealing to political scientists to read anthropology. But it seeks to move beyond the important reflexive turn that political ethnographers in political science celebrate. Vrsti (2008, p. 283) writes:

Although critical ethnography lacks a coherent set of political commitments and political principles, most proponents understand this to be the textual translation of fieldwork experience, where the perfect correspondence between reality and its representation is obviated through a commitment to radical perspectivism and essential reflexivity. This allows practitioners to openly engage (and sometimes struggle) with textual heteroglossia and cultural criticism even at the cost of sacrificing narrative authority and being accused of fictionalism.

Vrsti's sentiments are echoed in the works of many others, including the vivid, engrossing discussion of everyday power relations on the kill floor of a midwestern slaughterhouse (Pachirat 2008). As recent works in anthropology have pointed out, however, efforts at "radical perspectivism" and reflexivity have often devolved into what Comaroff (unpublished manuscript, pp. 17–18) calls "fractal empiricism," by which he means

the description of acts, events, experiences, and objects in the phenomenal world as the observer hears, sees, senses, records them—in all their concrete fragmentary, unruly manifestations—without reducing them to any more coherence than is required to render them into words. Which is to say, scarcely any at all. . . . This, by implication, amounts

to collecting narratives, images, and practices, actively seeking to avoid "imposing" any authorial order upon them, or to find meaning "beneath" their surfaces, thus to allow other worlds, others in the world, even other things, to speak and act for themselves.

Interpretive ethnographers who have participated in this "critical ethnographic turn" ironically run the risk of closing off interpretation—forsaking as well potentially illuminating engagements with political and social theory.

Interestingly, Comaroff (unpublished manuscript) and Bunzl (2008) both use Anna Tsing's influential *In the Realm of the Diamond Queen* (1993) to identify the seductions and limitations of this turn. Because her monograph has also inspired ethnographically minded political scientists, their criticisms have especial relevance here. Crediting Tsing for her exquisite, insightful prose—she offers her readers an abundance of descriptive detail, articulated from various vantage points, and with an uncommon artistry—Comaroff and Bunzl nevertheless note that she deliberately makes no gesture toward authorial explanation, other than to insist on the importance of reflexivity in the practice and writing of ethnography. Comaroff (unpublished manuscript, p. 18) asks what the "anthropological value-added" of her narrative choices are, and what makes her ethnography a work of anthropology rather than "literary nonfiction"? My reiteration of these questions is not only meant to demonstrate how far anthropological debate has moved since the 1980s and early 1990s, but also to offer a cautionary tale to political scientists. Political scientists who want to "theorize the role of the ethnographer in the ethnography" (Pachirat 2009b, p. 144; see also Shehata 2006, p. 246) and those who worry about such reflexivity on strategic grounds (e.g., Bayard de Volo & Schatz 2004) may both be reinventing an anthropological wheel that has already rolled away. Although recognizing the ways in which an ethnographer is coformed by her field experiences is crucial to the practice of ethnography, humility—a sense of

the provisional and partial, power-laden and transformative aspects of all research—is not an excuse to shy away from explanation or theorizing. Nor should it restrict theorizing to an account of the researcher’s position in her research. (For an important attempt to produce a theory of reflexivity, see Burawoy 2003.)

Instead of deriving inspiration primarily from the anthropology of the 1980s and early 1990s, we might want to chug ahead to the anthropology of the 2000s. Advancing does not mean repudiating all of the important lessons from *Writing Culture*, many of which anthropological theory in 2009 takes for granted. Rather, moving on suggests discarding assertions and strategies that now seem stifling, tired, or wrong, and building creatively on what seems useful and true. In doing so, we need to tack back and forth (to use Geertz’s metaphor) between the theoretical and the empirical, the abstract and the concrete, acknowledging the tensions and contradictions laid bare by fieldwork while maintaining analytic sovereignty over them. Rather than reflexivity as the personal insertion of the “I” into a fieldwork story, one might adopt a sense of epistemological reflexivity toward the discipline, posing questions about what bounds the discipline and normalizes its modes of inquiry, rendering other possibilities unsayable, unthinkable, irrelevant, or absurd. And rather than romanticizing ethnography’s potential contributions to political science or insisting on its particular penchant for radicalism (a position most anthropologists would disavow), this review stresses the importance of theoretical estrangement—what Bertolt Brecht called *Verfremdungseffekt*—a distancing effect made possible by an active cultivation of one’s critical and innovative faculties. Ethnography in this sense is “dual,” made up of what the “natives” say and what the researcher interprets (Pitkin 1993, p. 261). And interpretation requires both a theory and a healthy skepticism about its explanatory efficacy. By navigating between concrete details and conceptual abstractions, we can refine and undermine, negate and create novel explanations about politics.

ETHNOGRAPHY AND TRUTH CLAIMS

Good ethnography does not exist in isolation from theory, including theories of language, power, political action, and truth. And political scientists with a long-standing tradition in political theory might take advantage of their own disciplinary location to avoid some of the problems bedeviling past ethnographic work. Modes of political theorizing might also help to clear some of the conceptual fog that has hitherto obfuscated discussion about ethnography’s significance for political science. In this light, political theory might help clarify the fraught discussion of “truth,” its relationship to ethnographic research, and the conundrums both pose for political science.

By definition, there is no perfect correspondence between reality and representation, which means that some political scientists’ attempts to use ethnographic observations as if they were raw material are necessarily flawed. But this tendency is no less problematic among quantitative social scientists when they treat quantitative data sets as unmediated sources of knowledge. The choice of proxies, the use of one definition as opposed to another, a reliance on information that is itself already ground through various interpretive mills make such endeavors akin to the ethnographic task of sorting and distilling, ordering and making intelligible observations that can contribute to a logically coherent explanation, or to criticize prevailing theories, or both. If in ethnography “there is never nothing going on,” the task of explaining may be particularly complex, which is why the ethnographer needs theoretical scaffolding in order to structure findings. In political science, that scaffolding has tended to come from Weber/Geertz (e.g., Laitin 1986), from theories of performativity derived from Wittgenstein and J.L. Austin (Laitin 1977, Wedeen 2008), from Foucauldian understandings of the “work” discourses do (Scott 1998; Wedeen 1999, 2008), from a productive engagement with Gramsci’s notion of hegemony (Laitin 1986, Scott 1990), from

Wittgensteinian ordinary language use analysis (Schaffer 1998), from conversation with Habermasian public sphere theory (Fernandes 2006, Wedeen 2008), from assessments born of “critical liberalism” (Jung 2008), and from Arendtian notions of the political (Wedeen 2008), to name a few.

Some interpretively minded ethnographers in the discipline work self-consciously with the problem of representing others’ representations. Although such self-consciousness can lead to a frustrated relationship among political scientists of different stripes, it can also enable productive, critical dialogs and disagreement. Celebrating ethnographic writing as “fiction,” as 1980s scholarship in anthropology did, may pose irreconcilable difficulties for political science, especially if one understands fiction as “made up” rather than “made” or “fabricated” (Rabinow 1986, p. 243). And if “fiction” means simply that ethnography is “made,” that is not particularly useful, since the same can be said of any human endeavor. The broader contention about interpretation by literary theorist Stanley Fish (1980) and echoed by anthropologist Paul Rabinow (1986), however, is clarifying and helpful. Here is Rabinow on Fish’s “What Makes an Interpretation Acceptable?” (1980, p. 338):

He [Fish] argues that all statements are interpretations, and that all appeals to the text, or the facts, are themselves based on interpretations; these interpretations are community affairs and not subjective (or individual) ones—that is, meanings are cultural or socially available, they are not invented ex nihilo by a single interpreter. . . . All interpretations, most especially those that deny their status as interpretations, are only possible on the basis of other interpretations. . . . Fish argues that we never resolve disagreements by an appeal to the facts or the text because the facts emerge only in the context of some point of view. It follows, then, that disagreements must occur between those who hold (or are held by) different points of view, and what is at stake in a disagreement is the right to specify what the facts can hereafter

be said to be. Disagreements are not settled by the facts, but are the means by which the facts are settled.

Perhaps most relevant for political scientists is the insight that interpretations are “community affairs and not subjective (or individual) ones.” Because meanings are “cultural or socially available,” they are replicable—in the sense that some political scientists care about replication. Subsequent researchers can go to the field, and even if they do not talk to the same people, they can nevertheless be made aware of the range of meanings relevant to a particular phenomenon under study, because meanings are socially, not simply individually, accessible. Scholars’ own interpretations of these meanings also only make sense within socially available (and therefore contestable) standards, including standards for what counts as a fact and what does not, for “facts emerge only in the context of some point of view.” Even a seemingly straightforward fact such as “Napoleon Bonaparte died on May 5, 1821” presumes a specific world of language in which the Gregorian calendar has authority and death is understood to mean the cessation of life on earth. In Pitkin’s words (1993 [1972], p. 178), “empirical investigation presupposes conceptual definition” (in this case, definitions of time and death), and conceptual definition requires what Wittgenstein calls a “life world.”

There are also different kinds of facts, a point that is underspecified in the Fish/Rabinow discussion but that Pitkin (1993) discusses at length. Her key example is the famous argument in Book I of Plato’s *Republic* in which Socrates and Thracymachus address the question: What is justice? The sophist Thracymachus contends that “justice is the interest of the stronger.” He explains that in every society the norms of what is and isn’t just are set by the ruling elite, which is acting on behalf of its own interest. Pitkin (1993, p. 170) summarizes Socrates’ formulation as follows: “justice is everyone having and doing what is appropriate to him.” The dispute is often understood as a disagreement about “is” and

“ought,” in which Socrates’s normative claim about what justice ought to be is juxtaposed with Thracymachus’ empirical claim about what justice is. But Pitkin argues convincingly that their fundamental disagreement is not an *is/ought* one. Both men are discussing what justice *is*. Socrates answers the question as if he were talking about the grammatical meaning of the word “justice,” whereas Thracymachus is answering the question sociologically, in terms of the things people call just or unjust. As Pitkin writes, “the word ‘justice’ does not *mean* ‘in the interest of the stronger,’ and Thracymachus is not suggesting that it might. Thracymachus is trying to tell us something about the things or situations people *say* are ‘just.’ Socrates, by contrast, is trying to tell us *what people are saying* about a thing *when* they call it ‘just,’ what they are saying *by* calling it ‘just’” (p. 170). Theirs is “a dispute about facts, over the implications of two different kinds of facts” (p. 178).

Some statements are not primarily about truth or falsity, as J.L. Austin points out in his discussion of performatives—language that performs the action named, such as “I bet,” “I promise,” “I warn you.” And there are studies, inspired by Foucault, that chronicle how truth claims work. Positivists in political science often ask interpretive social scientists how they can trust what interpretivists say to be true. One of the reasons that it is so difficult to respond to such a question is that it is hard to know what aspect of the claim-making the skeptics are referring to. The conceptual claims? The causal argument? The observations? The notions of objectivity that underlie such questions paper over not only the historical evolution of and philosophical contention about what objectivity means (see Daston & Galison 2007), but also the ways in which claim-making works. In the second half of Wittgenstein’s *Philosophical Investigations*, there is a picture-puzzle that can be seen either as the head of a rabbit or as the head of a duck, or, for that matter, as a picture-puzzle. Wittgenstein himself calls the image a duck-rabbit. If the subject does not see the ambiguity in the picture, she will see a duck or a

rabbit. If she does see the ambiguity, she might say, “Now I see it as a duck” or “Now I see it as a rabbit” or “I saw it as a duck before, but now I see it can be a rabbit as well.” Pitkin (1993, pp. 100–101) uses Wittgenstein’s duck-rabbit example to ask what the objective facts of the world are, as distinct from what particular people would or could say about them:

Is the man who has not perceived the ambiguity in the duck-rabbit *seeing* a rabbit, or is he seeing a duck-rabbit picture puzzle *as* a rabbit? He would say the former; we might say the latter, and so would he in retrospect, after he had discovered the ambiguity. Is it obvious that one of these must be the real, objective truth? One might want to say: The experimenter’s view is “truer” because he has some knowledge which the subject lacks—namely that the picture is a trick picture. The real truth, which even the subject would acknowledge if he were well informed, is that he is seeing a duck-rabbit as a rabbit. But one might, alternatively, want to argue that truth is a matter of interpersonal, intersubjective agreement, what both men could agree on. In that case the subject’s view is “truer.” For the experimenter *could* say of the subject “he sees a rabbit” if he were trying to give a phenomenological account of the subject’s experience. But the naïve subject would not, no matter what he was trying to give an account of, say “I am seeing a duck-rabbit as a rabbit.”

The point is that when people disagree about the facts, they may be able to find shared vocabulary or some common ground on which to agree (Pitkin 1993, pp. 101–102). But agreement does not mean that there is a neutral objective truth. As Austin (1962, p. 101; cited in Pitkin 1993, p. 102) says, sometimes there is “no *one right* way of saying what is seen,” because “there may be no one right way of seeing it.”

Ethnography can give us access to this multiplicity, registering the phenomenological account of the subject’s experience while also crediting the analyst’s distance from and

knowledge about that account. But ethnographers abdicate theoretical responsibility when they simply focus on the experiential dimensions of “native” testimony or the intersubjective agreement between the ethnographer and her subject out of which such a phenomenological account is fashioned. Political scientists who abjure intersubjective and phenomenological considerations, by contrast, run the risk of producing arguments with little connection to politics on the ground—and with unexamined consequences for ordinary people. Both kinds of scholars can end up bracketing investigation of the broader social world through which facts, interpretation, meaning, *and* scholarly adjudication happen.

An interpretivist is attentive to the social construction of facts (even in the seemingly straightforward case of Napoleon’s death), to a world in which different kinds of facts exist (as is exemplified in the Socrates and Thracymachus debate), and to different ways of seeing (as Austin points out). These commitments do not imply that interpretivism cannot make “falsifiable” claims or that ethnographic research is irrelevant to the project of generating them. Some interpretivist arguments are falsifiable and others are not. For example, the argument in *Ambiguities of Domination* (Wedeen 1999, pp. 152–53) can be falsified by demonstrating the existence of a noncharismatic regime in which tired slogans and empty gestures foster allegiance and actually generate people’s emotional commitments to the regime. Recognizing ambiguity, as many interpretivists are wont to do, should not be confused with “unfalsifiability.” Nor should unfalsifiable be confused with untrue. Indeed, there are many potentially true statements that cannot be falsified and important statements to which the categories of truth and falsity do not apply. Ethnography *may* be helpful in determining what happened, when events themselves are open to question. But its value lies in its capacity to do much more than that. When Scott (1985) analyzed how poor peasants and landlords recounted events, for example, he was less interested in whether their narratives were true than in how the

disagreement worked to constitute a moral economy of village life (Pachirat 2009a discusses Scott in this light). Ethnography can situate truth claims in a broader context. Ethnography can also show us how such truth claims operate.

CONCLUSION

“There is never nothing going on” is an invitation to embrace the richness of fieldwork experience, but it is not a license to surrender analytic control. An ethnographic interpretation might underscore the tensions and contradictions of everyday life, but its burden is to maintain theoretical sovereignty over those complications. The dinner table I imagine is one where the ethnographer questions the very terms of debate that prevail, where epistemological reflexivity trumps personal therapy, and where underlying assumptions among both positivist political scientists and interpretivists are subject to vigorous interrogation. The dinner table is a place where the ethnographer’s practices are respected but not romanticized, where the scientist’s claims about objectivity are subject to conceptual and historical scrutiny, and where all parties practice what Connolly (2008) calls “presumptive generosity.” Such generosity may require learning unfamiliar vocabulary (see Yanow 2009), cultivating curiosity, and preserving a sense of humor and humility. It also means being open to being pressed—about the added value of complexity or parsimony; the possibilities, limits, and desirability of replicability; and the multiple ways in which an argument can be generalizable—providing accounts of how and why the world is as it is (see Wedeen 2004). Large-*n* work and game theoretic models do this by attempting to specify law-like patterns governing human action. Ethnographers motivated by Wittgenstein or ordinary language use analysis do this by clarifying concepts that tell us how various communities think about and construct their worlds. Ethnographers beholden to Foucault do this by analyzing the “work” discourses do—their underlying assumptions, omissions,

implications, and effects, as well as their historical conditions of possibility. Ethnography can demonstrate that previous generalizations were wrong (thereby producing new ones), replicate findings (but not necessarily encounters), explicate mechanisms that can have wide-ranging application, and bring new ways of seeing and understanding into plain view.

Disciplinary borrowings are frequently anachronistic, but they need not be. In the same way that some anthropologists not only appropriated from but also challenged the discipline of history by relocating its contemporary insights on anthropological ground (e.g., Cohn 1987, 1996; Comaroff & Comaroff 1991, 1992, 1997; Stoler 1995, 2002, 2009), political scientists have choices to make and opportunities to embrace. Coming late to ethnography has its advantages, allowing political scientists to avoid some of the pitfalls that their predecessors in anthropology encountered. To learn from anthropologists who study smuggling gangs in the Chad basin (Roitman 2005), AIDS in gold-mining communities in South Africa (Morris

2008), the power of symbolic displays in Thailand (Morris 2009), piety movements in Egypt (Mahmood 2005), archaeological practice and nation-building in Israel (Abu El-Haj 2001), the afterlives of revolution in Indonesia (Siegel 1998), or the politics of recognition in Australia (Povinelli 2002) is to be introduced not only to new empirical worlds but also to novel ways of understanding phenomena of central concern to political science—in the examples above, crime, change, value, religion, desire, science, and liberalism, respectively. By balancing concrete empirical examples with theoretically motivated discussions of Foucault, Derrida, and Hegel, these anthropologists unsettle taken-for-granted assumptions and provide us with new language for tackling perennial issues. What makes these writings so compelling is not the ethnographic work per se—some of these monographs are rather thin on that level—but their innovative theses and their attention to mechanisms that induce solidarity, community, prejudice, passion, envy, discipline, strategic choice, and dominance. In short, the very stuff of politics.

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