

Political Ethnography

What Immersion Contributes
to the Study of Power

Edited by
EDWARD SCHATZ

Foreword by Myron J. Aronoff

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- pieced with studying the impact of the manner in which this reality is represented in their texts on their knowledge (though see Patterson and Monroe 1998).
24. For an enlightening discussion, see Brubaker and Cooper (2000) and the Harvard Project on "Identity as a Variable": <http://www.wcfia.harvard.edu/misc/initiative/identity/>.
 25. See, for example, Geddes (2003); Laitin (2004); Little (1998).
 26. Vincent (1990, 388–402); Marcus (1998, 79–80); Trouillot (2003, 120–21).
 27. This call to rethink the relationship between history and memory has been recently heeded by sociology, history, and cultural studies; there has been a renaissance of writing on historical memory.

TWO

How to Tell an Axe Murderer: An Essay on Ethnography, Truth, and Lies

JESSICA ALLINA-PISANO

In August 2000 and again in 2005, the scientific journal of record *Nature* ran articles about "political scientists." The scientists in question were evolutionary biologists who had stepped outside their role as researchers to speak out against efforts by proponents of the intelligent design movement, who wanted their theory of creation taught in public schools alongside Darwinism (Gewin 2005). Around the same time, while teaching a seminar on nationalism at a liberal arts college in the northeastern United States, I encountered a dean who urged me to "allow the debate" as my undergraduate students contested not how or why, but whether the Third Reich had killed millions of Jews.¹ The intelligent design movement's attempt to cast its claims, based in biblical revelation, as a competing theory to be considered in science classes alongside research-based consensus,² together with the putative "debate" posited in the Holocaust denial literature, highlights a more general problem in the production and transmission of knowledge: amid a societal commitment to epistemological pluralism, how ought we to adjudicate truth claims about the world?³

In a world of epistemological pluralism, truth claims may have no ontological status: all claims and perspectives can be treated as *ipso facto* equally valid. As a result, they may be adjudicated in the political arena, subject to bald contests of power rather than analytical acumen or specialized knowledge evaluated within a common framework of accepted standards of research. There is arguably a danger in such instances that populism of the moment, or of a specific segment of society, may overcome knowledge that has been accrued, tested, and improved by communities of scholars over decades or generations.

The political context of these conflicts between particular truth claims, while anchored in a specific historical moment, points to a more general

epistemological challenge for ethnographers working within interpretivist traditions of social inquiry. Interpretive approaches narrate alternative epistemologies, telling us a great deal about how human beings understand and constitute the world. However, for problem-driven research (Shapiro 2005), there is also a need for approaches that can help social scientists reliably adjudicate truth claims about the world. If social research is to speak to real-world questions, it must include tools that allow its practitioners not only to make observations about how people think about and constitute their world, but also of the material conditions and social structures within which they make their choices.

Interrogation of the subject-object relationship is a central preoccupation for any researcher interested in careful and rigorous research. However, researchers focusing on certain types of questions may wish to place principal emphasis not on analysis of the subject-object relationship, but on making observations that are as “theory neutral” as possible (Wendt and Shapiro 1992) and from there, establishing causality. To put it a different way, social science in general and the study of politics in particular may sometimes require a commitment to uncovering truth—truth in the sense of *Beim Häuten der Zwiebel* (Peeling the Onion), rather than sacred revelation—in addition to elucidating social meaning.⁴

Toward a Realist Ethnography

Ethnography is better positioned than other qualitative social science methods to do precisely this—to peel the onion. All modes of knowledge production are in some way subjective, but the ethnographer is equipped to contextualize and interpret her subjectivity: to be conscious of the ways that her position conditions the observations she makes.

As Kubik notes in chapter 1, ethnography is especially good for examining politics in the margins and interstices of political life, including that broad swathe of politics that occurs outside of, parallel to, or unseen within formal institutions. In contexts where informal practices drive formal politics (Ledeneva 2006), where political actors deliberately hide their activity (Allina-Pisano 2004), or where the politics of less powerful people are the object of study (Scott 1990), ethnography may be the best or even the only viable approach. Where partially hidden, power-laden processes drive economic distribution, as under customary law, informal patron-client relations, “corruption,” and other regimes of reciprocity, ethnography may likewise be of particular help in uncovering causal mechanisms.⁵

The essential problem associated with truth claims made through eth-

nographic study is endemic throughout the study of politics: if the act of observation shapes the object of observation, then there is no “neutral” evidence. And if neutrality is deemed necessary for advancing causal claims, then no avenue of inquiry remains open to the scholar except deductive thought—and then only once its necessarily subjective first principles have been adopted. The notion that truth claims must, by definition, be founded upon “theory neutral” observation cedes nearly all ground in the production of knowledge to formal modelers and rational-choice theorists.

If, however, as most philosophers of science since Kuhn (1962) would hold, observation is necessarily mediated by the act of observing, the study of problems in the world is possible primarily through an interpretive lens. In most formulations, an interpretive lens offers alternative epistemologies but no ontological claim to truth or objective description as such. For many social scientists, this is problematic for a number of reasons, not the least of which is that rational-choice theorists have themselves returned, of necessity, to the empirical world in order to narrow the domain of their claims: as others have argued, rational-choice approaches without reference to context produce not meaningful theory, but a single explanation for everything, everywhere (Green and Shapiro 1996).

This chapter makes a case for ethnography with a realist sensibility. It does not deny the value of interpretation in ethnographic approaches to social research. On the contrary, it suggests that an understanding of the ways in which people think about their world can be a necessary condition for the collection and use of reliable empirical data about them. In other words, it suggests not only that ethnography is capable of making meaningful contributions to social research, but also that it offers advantages for adjudicating truth claims and establishing causality that other approaches do not. Further, this approach builds upon longstanding traditions in social research: fieldwork-based research exploring local political and moral economies long has been a staple of political science as well as social anthropology.⁶

Following Shapiro’s understanding of scientific realism in general, realist ethnography proceeds from two assumptions: that “the world consists of causal mechanisms that exist independently of our study—or even awareness—of them,” and that ethnographic techniques “hold out the best possibility of our grasping their true character” (Shapiro 2005, 8–9). A realist ethnographer draws inferences not only about what political actors say they do, but also what she observes them doing.⁷ That observation, however, is necessarily conditioned by familiarity with the context that shapes the social meaning of action. In the language of many of my Russian-speaking

interlocutors in research, the ethnographer can offer a *vzgliad so storony*, an informed outside view. Such an approach allows the scholar to attend to the role of language in constituting social reality, but it does not require that she bracket material or structural factors.

Demands for theory-neutral observation, common in the field of political science in North America, have resulted not only in a practical problem for knowledge generation, but also in a bifurcation of social inquiry: with some exceptions, formal theorists and empiricists of various persuasions study formal institutions and the people that populate them. To the extent that those institutions tend to be loci of social, political, and economic power, the study of formal politics is the study of the powerful. And because those who study political elites often cleave to epistemologies that admit to ontological truth, their research findings are articulated as truth claims about the world.

In contrast, the ethnographic tradition of studying people at societies' margins, of examining the politics of the subaltern, most comfortably embraces interpretive approaches to knowledge. This approach recognizes self-understanding and social construction as the primary domain of social inquiry. The claims of this research thus center on the production of social meaning—how people see the world, and how those self-understandings constitute reality. The result is that while social research about the powerful claims a monopoly on the production of facts and causal inferences, research about the relatively powerless offers alternative epistemologies but not competing, or corrective, facts.

However, the study of the margins is important not only for communicating and interpreting the voices of less powerful people, but also because the knowledge produced in the margins may sometimes be, in the world of truth claims, more accurate than that generated in the center. People in the margins may have more than "another perspective" to contribute; as actors close to local processes of political change, they sometimes have more detailed information about certain types of phenomena than do political and social elites. Similarly, where key political processes unfold in the interstices of formal institutions (Ledeneva 2008)—in the hallway, on the telephone, over a drink—ethnography offers an opportunity not only for enriching our understanding of perspectives on politics, but also for identifying otherwise elusive causal mechanisms, for more firmly establishing what happened and why.

Realist ethnography charts a middle course between ostensibly "theory neutral" and "theory laden" observation.⁸ A realist approach acknowledges the role of the ethnographer in coconstituting the reality he studies.

It does not, however, presume that the ethnographer's presence will always meaningfully alter the causal mechanisms being observed; the ethnographer must use her knowledge of context to reflect upon the specific ways in which her presence is likely to have shaped the evidence.⁹ Thus, realist ethnography refers to the process that guides research and writing, not to the product itself.¹⁰ The extent to which the ethnographer's presence influences others' behavior and elicits performances for the ethnographer may vary depending on the chosen field sites, as well as on the ethnographer's position in local and global hierarchies of social, economic, and political power. In the ethnographic tradition that takes a single, small village as its object of study, the researcher's presence may be likely to influence the reality being observed to a greater extent than would be the case in a field site defined by mass demonstrations.¹¹

Ethnographers cannot control for the effects of their presence on their interlocutors, but ethnography is better suited than other varieties of social science to identify the specific ways in which the researcher's presence shapes evidence. In other words, unlike many of their counterparts engaged in other types of social research, ethnographers are, with their contextual knowledge, in a position to know which types of biases their presence is likely to introduce. In most cases, research based upon statistical analysis cannot account for such effects, as users and compilers of databases are usually different individuals, and even collectors of statistics may not be sufficiently embedded in local realities to discern the ways in which numbers might be intentionally or unintentionally altered in the process of transmission. For example, it is well known in postsocialist contexts that businesses or government offices often keep more than one set of (different) books, but this does not necessarily change how users of databases treat aggregate commercial or government statistics. Likewise, in countries where communications infrastructure is radically uneven, there are likely to be substantial differences in regional figures that may have little to do with anything beyond an electricity outage, the absence of a copy machine, or some other obstacle that affects the collection and transmission of data but leaves no trace of the nature, magnitude, or direction of the bias introduced.

Ethnography is most often associated with participant-observation research, but participation in fieldwork settings need not implicate the researcher in the cocreation of social reality any more than an unobtrusive "neutral" presence would. Indeed, sometimes evidence-gathering without active participation can be more disruptive than silence. This may be particularly the case in postauthoritarian contexts. For example, once, at the end of a long day of meetings and conversations in post-Soviet eastern Ukraine,

one of my interlocutors reprimanded me for my lack of engagement at that moment: "You're like a ghost. And we don't like ghosts." Here, people interpreted my silence not as serious scholarly attention or fatigue, but as public comportment reminiscent of a member of the secret police engaging in surveillance.

Through a series of fieldwork-based vignettes, this chapter examines the kinds of claims realist political ethnography can make. These vignettes are drawn from research on land privatization in rural communities in the former Soviet Union between 1997 and 2006. During that period I conducted participant-observation, interview, and documentary research in an effort to uncover the incentives and causal mechanisms that, at the local level, drove shifts in postsocialist property regimes. This research was multi-sited, providing a view onto the beliefs and actions of different actors involved in the privatization process. I lived on a former collective farm to observe the process of farm reorganization at close range; I attended the meetings of farmers' organizations to try to understand the challenges faced by people who were trying to obtain and use land; and I spent time in county and regional offices, where I watched and listened to those charged with distributing land.

The chapter aims to show through narrative what sorts of things ethnography can tell us that other qualitative methods cannot.¹² The ideas presented here cohere around the problem of using "face value" statements in research. In particular, how do we interpret evidence produced by our interlocutors if our intent is not only to uncover the self-understandings of our subjects, but also to adjudicate and make truth claims about political and social phenomena? While the following discussion concerns truth claims, I do not propose here a paradigm for translating ethnographic work into generalizable theory: all theory based on ethnography is synecdoche—but so, on the other hand, is all theory based on random quantitative sampling.

If, in Robert Darnton's (1984) terms, minor episodes can illuminate the ways historical actors viewed their world, I suggest that such episodes can offer windows onto political realities that, in the absence of ethnographic knowledge, otherwise would remain hidden from view. The following vignettes engage the relationship between power and evidence. The first reflects upon the process of research, rather than presenting research findings. It highlights the epistemological challenges of doing fieldwork, illustrating how interpretation without adequate ethnographic knowledge can contribute to the reproduction of status quo power relations. The second vignette provides an example of how reading interview texts divorced from ethnographic context can lead to wrongheaded conclusions. The third shows how

participant-observation research illuminates the power relations that underlie competing truth claims. The fourth vignette suggests ways in which ethnography can help us identify ritual speech and read between the lines to uncover both social meanings and aspects of institutional change.

Vlad the Researcher

Toward the end of a period of research in a country of the former Soviet Union, I requested that a colleague at a local institute help me identify a student research assistant to copy newspaper articles on land reform. I had begun this job myself some time before, but field interviews increasingly occupied my time, and I had a limited number of months to complete that stage of my research. At that time, in this particular country, female university students faced a highly restrictive labor market in which employment often required extreme compromises of individual dignity. Job advertisements for foreign as well as domestic companies were explicitly directed toward young men, while the few ads for young women enumerated precise body measurements and required that applicants have "no hang-ups." Knowing this, I specified that I would prefer to hire a young woman. Instead, my colleague presented me with a male undergraduate—a student she knew and wished to help—for the job. He was qualified for it, and I hired him.

Vlad was a second-year student with an odd demeanor. Despite manifold obstacles to obtaining newspaper articles, including far-flung libraries, incomplete holdings of periodicals, and the ever-present problem of copying articles in libraries where reproduction services were understaffed or nonexistent, Vlad executed the task well. We met every week or two at the institute, where he presented me with a stack of articles that I would read and file with my other research materials.

Some time into his work with me, Vlad announced that he would be away for a week or two. He needed, he said, to "visit his grandmother" in another region. His grandmother was ill, and no other family members were available to care for her. We made arrangements to meet again when he returned.

That week, the local television news covered a real-life horror story. The body of a murder victim had been discovered at a market on the city outskirts, where it was being sold by the kilogram. Initial reports suggested that the murderer knew the victim, and that the murder had occurred following a personal dispute over money—in this case, three hundred dollars.

Not long after the television broadcast, I received a call from my

colleague at the institute, who requested that we meet immediately. An hour later, as we strolled through a park, my colleague told me that Vlad had been named the main suspect in the market murder. He was being held and questioned by the police. My colleague added that I was no longer welcome to meet with Vlad at the institute, and that I should find a different meeting place. In particular, my colleague emphasized that because Vlad had my business card with my local phone number written on it, I should not be surprised to receive a call from the detective in charge of the investigation.

A week later, Vlad called to arrange a meeting. I was unwilling, under the circumstances, to identify a private meeting spot. However, in a context in which some categories of people targeted by the authorities routinely fell through the cracks of the court system, and where more informal mechanisms of seeking justice and retribution were not unheard of, I was equally unsure about a public meeting. I chose a nearby café, where I settled up with and said good-bye to Vlad, who expressed interest in staying in touch with me. I did not hear from the authorities before I left the country a week later, and I have not spoken with Vlad or my colleague since that day.

This episode raised a number of questions that required immediate answers. Most crucially at the time, from the standpoint of the researcher, where does the truth lie, and if Vlad were the murderer, did he still need money? Here were two competing, incommensurable narratives: short of stretching the ironic trope to its conceptual limits, I saw no way to frame both Vlad's narrative (I am going to visit my grandmother) and my colleague's (Vlad is being held by the police on suspicion of being an axe murderer) as different understandings of the same core event. The existence of alternative epistemologies was both logically impossible and morally unacceptable. Furthermore, for practical reasons I could not remain agnostic on the question of what had happened.

In a modern perversion of Pascal's wager—it is safer for me to commit myself to the belief that Vlad is an axe murderer and live my life accordingly than it is to harbor doubt—I chose to act in response to my colleague's version of events, rather than Vlad's. This represented a compromise on my part. I lacked sufficient knowledge to make an informed judgment: I made my decision about likely truth on the basis of my colleague's authority, not firsthand knowledge. This decision was driven by practical, not intellectual concerns, but it was nonetheless an attempt to establish causality and adjudicate a truth claim on the basis of incomplete knowledge. Turning the analytical lens on myself, I surmise that I chose to accept my colleague's version of events not only for, in the American satirist Stephen Colbert's formula-

tion, its "truthiness,"¹³ but also because there was no obvious danger to myself in doing so (whereas the risks of association with Vlad were unknown but potentially serious). Furthermore, I shared multiple social ties with my colleague, and this likely influenced my response. I knew both my colleague and Vlad individually, but my colleague and I had a dozen other acquaintances in common. I was friendly with one member of my colleague's family and had worked with one of my colleague's coworkers. I had no information about the nature of Vlad's relationship with my colleague, or why my colleague had insisted I hire Vlad and not someone else.

Although I was embedded in some of the social relations that constituted my colleague's and Vlad's world, I was not embedded deeply enough. I knew no one in Vlad's circle other than my colleague. To my knowledge, I had not met the person who had been killed. Had I been more familiar with Vlad's circle of friends, I might have noticed if one had gone missing. In this situation, I faced a practical need to adjudicate truth claims, but I lacked deep ethnographic knowledge of the relationships that could have cast light on the question at hand. With better contextual knowledge, I would have had a stronger empirical basis for my conclusion: either one of Vlad's friends was missing, or none was. Lacking that information, I made a choice that seemed safer: to accept the story as told from above.

Most positive social science positions the researcher as a mid-twentieth-century Switzerland. While embeddedness in any community creates biases in the researcher, the absence of ethnographic knowledge creates its own bias. Without such knowledge, we are likely to make judgments based on whatever evidence is most easily accessible. In the case of Vlad, the authorities had a ready-made—and, to my mind, plausible—narrative available for consumption. Vlad, in contrast, had provided only a thin story that held up weakly—both in aesthetic terms and analytically, in view of the claims made by my colleague and, putatively, by the state. This is in some ways no surprise: it may be the case that the production of narratives to explain and excuse itself is the ordinary condition of power. If this is so, social science research without ethnography risks reproducing the status quo power relations that feed it.

DT

At end of the 1990s homemade signs could be seen all along the Ukrainian stretch of international highway E40. The highway enters Ukraine at the Polish border near the city of L'viv and winds through western Ukraine

to the capital, Kyiv. From Kyiv, the road continues across the eastern territory of Left Bank Ukraine to Kharkiv before it turns southeast through the Donbas to the Russian city of Rostov. In the east, the signs, small squares of cardboard propped on chairs in front of houses that closely line the road, read "*Kupliu DT*" ("I'll buy diesel fuel"). The acronym *DT* stands for diesel, or *dizel'noe toplivo*. The Russian verb and abbreviation were intelligible to nearly all who used this portion of the road.

Such signs could be found both along major highways and along some back roads in Ukraine: they signified difficult times and a fortuitous location on a road along which fuel trucks traveled. The informal diesel trade went unrecorded by any legally registered enterprise or government organization. Nonetheless, its survival depended on the roads, trucks, and fuel of the formal economy: people living along such roads purchased diesel from supply trucks passing through and resold it locally at a slightly higher price. On the reverse side the handwritten cardboard signs nearly always read: "I'll sell diesel fuel."

In early spring of 2000 I passed several such signs during a visit to a remote district of eastern Ukraine. A Russian-speaking passenger in my vehicle—a professor of philosophy at an agricultural institute and officer of an organization that assisted local farmers—wondered aloud what the signs meant. He added that he thought *DT* might mean *dorozhnyi transport*, or automotive transportation. In his mind, the cardboard signs read: "I'll buy your car."

A similar reference appeared in popular culture just a few days later. This reference, aired on national television, likewise suggested a certain distance from rural concerns on the part of the urban intelligentsia. The broadcast was a contest of a popular Russian-language genre of improvised comedy skits. It included a reference to the same confusion expressed by the professor of philosophy: "I put diesel [*soliarku*] in the car instead of gas [*toplivo*]. I thought *DT* meant *devianosto tretii* [93 octane]."¹⁴

Neither my passenger nor the comedic butt of his own joke possessed knowledge necessary to interpret the abbreviation correctly—the way its user intended it to be read.¹⁵ The comedy skit, which was a riff on Soviet versus post-Soviet driving cultures as well as urban-rural cleavages, suggested that such ignorance was presumed to be widespread enough among a particular portion of society to amuse a television audience. But most Russians and Ukrainians familiar with rural life or with automobiles would have recognized the acronym *DT*. Even someone unfamiliar with the meaning of the acronym but knowledgeable about rural economic conditions would have guessed at least one truth: that the vast majority of village resi-

dents, including those engaged in the informal diesel trade, would not have so much free capital as to purchase a vehicle from a passerby.

By many measures, the professor of philosophy would seem to have been an expert on rural affairs. He was, after all, a founding member of a rural nongovernmental organization and the type of professional to whom Western scholars and lending institutions routinely turn for local information. I happened to recognize his mistake because I conducted research on farms and frequently discussed diesel prices with tractor operators and farm chairmen, and also because I drove a Russian vehicle and regularly used gas pumps in the area. In the absence of contextual knowledge gained through participant-observation research, however, I might have accepted the philosophy professor's statement at face value, as an accurate interpretation of textual meaning for the signs' intended audience.

For a social scientist, the analytical consequences of such a judgment could be potentially serious. If one takes *Kupliu DT* to mean "I'll buy your car," and if one sees such signs at regular intervals along a highway, two logical inferences likely result: first, that there is a large informal market in automobiles; and second, that people living along the E40 live fairly wealthy lives in which cash is widely available and the risk of spending money on a car with an unknown mechanical history is not a grave concern.

Further, if a social scientist conducting qualitative research solely in the form of interviews had spoken with the philosophy professor and accepted his interpretation of *DT*, these inferences might shape her interpretation of subsequent interview texts. In the light of the philosophy professor's interpretation, would the former mechanic who lives in a house where a *DT* sign has been posted be presumed to be lying when he describes the enormous expense and difficulty involved in commuting to his job in a nearby town? What about the pensioner next door who cares for her granddaughter after the deaths of her daughter and son-in-law and who laments her complete isolation and difficulty making ends meet? Is she merely "putting on the poor mouth"—performing poverty for the benefit of the researcher? How would the researcher decide?¹⁶ Ethnography has a role to play in interpreting such a lamentation if social inquiry is to be concerned not only with how the pensioner sees or presents herself, but also with the economic consequences for her village and the world if she—as one of hundreds of thousands, if not millions like her—is unable to provide an education for her granddaughter. Here, the type of knowledge acquired through ethnography allows the researcher to discern more intelligently the material condition of such a village street, and to avoid a misinterpretation that might so easily be produced through the use of other qualitative methods.

Plov

Near the end of the 1990s I lived for a time on the territory of a Russian collective farm undergoing privatization. I was there to study the process of land distribution at close range. The village attached to the farm sat in a wooded area dotted with lakes, a showpiece of natural beauty and a popular local destination for hunting, trapping, and fishing. However, the peace of the landscape for people, if not for animals, seemed at odds with social life in the village, in which some people seemed to lie to one another about everything.

Like life in other small places with scarce resources, village society meant a kaleidoscope of fabulae—a positivist's nightmare. The benches lining picket fences where old women shelled peas were carnivals in the Bakhtinian sense, as tales of the director's investments, the neighbor's cow, and the "American" villager's lottery-won green card were spun into gossip that challenged village hierarchies. In such an environment, where village-level great power politics seemed to govern every public utterance, it was difficult to know which statements might have been tactical maneuvers directed toward neighbors, and which were performances with my presence as a researcher in mind.¹⁷ In that village, the benches outside each fence together constituted the space that Scott identifies as the site of reproduction for private subordinate speech—the hidden transcripts of the dominated (Scott 1990, 70–107). However, even among relative equals—subordinates who lived autonomous lives in one sense but whose ability to heat their homes or travel to the hospital depended to a large extent on the farm director's goodwill—performances were often contradictory.¹⁸

As I interviewed or spoke with dozens of people on the farm; attended early morning meetings of farm specialists, at which the farm director set out the problems and tasks for the day; chatted for hours with the director's secretary; went swimming with the older women's singing and drinking group; hitched rides with people my own age when, after a day in town, I could avoid the 7-kilometer walk to the village from the bus turnoff on the highway; and on weekends went to the farm disco where "old maid" eighteen-year-olds hoped for love or at least marriage, I wondered how I would begin to understand how privatization was changing life in the village.

Part of my answer came after an ill-fated picnic by the river one afternoon. A relative of my host had, in the summer heat, killed a duck the day before and hung it in her yard. That afternoon, she built a small fire and briefly cooked the fowl. Duck fat was mixed with rice to make *plov*,

and every member of the party washed it down with copious amounts of moonshine made from sugar beet. Only I abstained from drinking, as I had scheduled a meeting with the farm director for the end of the day. When I returned from the meeting with the farm director, my host asked me if I had told him where I had been that afternoon. I said I had not, and she replied, "Correct."

Clarity about political economy in the village arrived in the following days, as I fell seriously ill. In the first days I passed through the usual stages of acute food poisoning, made worse, among other things, by the absence of access to a stove where I could boil (and thus render potable) water from the shallow village wells. A hard knock to the head as I stumbled into the latrine at an incorrect angle and made contact with a length of metal supporting the roof added a concussion to the mix, and it was time to travel to a city hospital.

Upon my return, an entire landscape of power in the village unfolded before my eyes. In my absence, a set of stories had emerged about what had happened to me: what I had eaten, where I had eaten it, who had prepared it, whether I was really sick, where I was being treated, whether I had gone to the city hospital, which of the folk medicine practitioners had visited me and what they had prescribed, and whether I was ever coming back. I heard some of these narratives directly, as people described what they had heard and asked me about it. I heard others indirectly, through gossip networks in the village. For the first time since I had arrived in the village, I alone knew what had occurred, if not all of the social implications of the event. This allowed me to map out who had told what to whom and to identify some of the loci of discursive and material power in the village.

The woman who prepared meals in the small kindergarten kitchen where I ate had been placed in the most awkward position. The kindergarten was believed to possess the cleanest kitchen in the village, and all guests of the farm were required to eat there. In the neighbors' view, it was she who had "shamed" me (*pozorili Dzhesiku*), so she instructed me with a counter-lie, "Don't tell them you ate in the kindergarten." Otherwise, she warned, the place would be closed down for days as a sanitary crew worked, and she would lose her job. The person who had served the guilty portion, meanwhile, denied ever having fed me when she spoke with a neighbor who was close to the farm chairman. In this case, my temporary vulnerability laid bare not only the ways people in the village understood their own vulnerability, but also how others responded to it.

Without this genre of knowledge, it would have been difficult fully to understand key components of village social relations that shaped the priva-

tization process at the local level. The nature of the farm director's power over certain members of the community, as well as the complex webs of reciprocity that tempered that power, became clear only after this episode. Here, a new social position not only revealed a different type of information, but also brought to my analysis of property relations a more sensitive instrument for measuring power and negotiation in the village.¹⁹

In other words, participant observation research made possible analysis that could not have been achieved in any other way. While this research revealed a shifting terrain of power relationships, it did more than show me how people understood their social positions. It also showed how specific power relationships structured performance, illuminating villagers' precise points of strength and vulnerability: the pensioner who had to remain in the chairman's good graces in order to have coal for the winter; the schoolteacher who could afford to give me a 5-liter jar of milk for making *prostokvasha*, a sour milk product useful for recovering from food poisoning; and the drinking, singing *babushki* who after long, hard lives formed a tight, mutually supportive social network that allowed them the freedom to worry about pleasing no one but each other.

Yevhen

A realist approach to ethnography, even as it may be principally concerned with causal mechanisms and truth claims, nonetheless requires that the ethnographer be equipped with the type of knowledge that characterizes interpretivist approaches. In particular, in order to make reliable observations, the ethnographer must be able to distinguish ritual speech from original narrative, as well as to interpret the social meaning of texts.

In July 2006 I interviewed Yevhen, a sixty-year-old man selling a pile of what looked to be the previous year's root vegetable harvest. Yevhen had a vegetable stand—two wooden boxes covered by a dishtowel—alongside a road that runs through his village in eastern Ukraine. Yevhen had come to eastern Ukraine from the Crimean peninsula on the Black Sea. He worked in the village for years, and then commuted to a nearby city to work in a bicycle factory. After the Soviet collapse, in his words, "the state seized" his savings. His wife had worked in the local collective farm kindergarten after suffering an injury to her leg, but many kindergartens were closed during *perestroika* and she lost her job: "She was let go [*sokratili ee*] and that was it." During the ensuing period, land shares were distributed, but there was no information available to her about it at the time, so she did not receive land.

In 2006 there was no collective agricultural enterprise in the village, but there was a farm with "one boss" (*odin khoziain*), who was "some kind of deputy" head of state administration in the provincial capital. Somehow, after some manipulation of farm finances, the boss had managed to pay off farm debts. Yevhen had a land share: at first, he had 8 hectares of land, but later he lost 2 hectares. He received 700 to 800 *hriven'* (about 150 dollars) in land rents for his 6 hectares, "not in cash but in food [*produktami*]." At home, his wife's "tears fall because there's no [land] share." It cost him 700 to 800 *hriven'* each year to have his household garden plowed.

As Yevhen described it, people had "already thoroughly robbed [*perevorovali*] the collective farm," and "there are none of our people there. They are people who were brought in [*oni privoznye liudi*]." Even though "we built the *kolkhoz*," Yevhen and his coworkers received nothing from it. When asked what prevented him from starting his own farm, he replied, "Fuel prevents me. Machinery prevents me. Before, there was interest" in private farming, but now "we're not building but destroying. Everything is destroyed like after the war." In 2006 there were no lights on the roads and "there's no gas." There was, however, a "good school" in the village through eleventh grade, the last year of secondary school in Ukraine.

As Yevhen spoke, a couple in their early twenties drove up to the small store behind his stand. Observing their Cabriolet convertible, Yevhen compared their situation to that of the men down the road who work twelve hours a day and are paid 260 *hriven'*, or about fifty dollars, each month. Yevhen remarked that the heads of people like him are filled with concerns about money: "you sit at home and think about how to get money to buy pants for your grandchildren so they can go to school." As I departed, Yevhen presented me with two apples from his stand and instructed, "Tell them there's nothing good here, nothing good."

I reconstructed the previous narrative from my notes, after I had asked Yevhen to tell me about his experience of village life and land reform in Ukraine. I have reported the elements of Yevhen's story in the order in which he gave them. This interview was unusually short, lasting only about twenty minutes, and I have not spoken with Yevhen before or since then. Yevhen declined to be recorded as we spoke, a preference that did not distinguish him from hundreds of others whom I have interviewed in this region of rural Ukraine and in other postsocialist societies. I have thus included only those fragments that I was able to transcribe verbatim, and so I have reported his speech in the third person. I do not know Yevhen's real name. Most people in low-status positions in post-Soviet Ukraine are reluctant to identify themselves, even with the assurance that their statements

would be reported anonymously. For them, danger resides not so much in the publication of their statements abroad as in their local circulation: Yevhen's wife might not like to be seen as weepy, his grandchildren as poor, his neighbors as thieving, or his local boss as shady or stingy.

Several other people were present as Yevhen spoke. It was easy to remove these individuals from my report of Yevhen's narrative, erasing them like Soviet leaders photographed on Lenin's tomb or, in Kundera's formulation, Clementis on the balcony of the Prague palace (Kundera 1979, 9).²⁰ The person who drove me out to the village was the regional head of a national voters' rights organization. In recent years, he had spent time on the road "giving lectures" to rural people about land reform. At several junctures, he interrupted Yevhen to challenge him on his interpretation of one point or another—usually just when I wanted to ask Yevhen what I saw as an important question of clarification. Other people, primarily urbanites who had stopped to inspect Yevhen's onions, carrots, and apples on their way to collect water from a natural spring down the road, moved in and out of the conversation.

Yevhen's story appears to be a personal narrative, a radically abbreviated version of a life history filtered through the voice of a political ethnographer. However, certain elements of his story are not personal at all. Instead, they are part of both broader social narratives and a liturgy of lamentation that is shared, above all, with outsiders.²¹ In Hayden White's terms, Yevhen's trope in this interview is tragedy (White 1978). Viewed through the lens of White's philosophy of history, and presuming a degree of conscious storytelling on Yevhen's part, Yevhen selected elements of his past to support this mode of narration. In other words, the sequence of detail is crafted for the purpose of singing a tale of woe in the face of economic liberalism.

I suggest this may be the case because every element of Yevhen's story saturates post-Soviet social space and is reproduced in conversation repeatedly, often using the very same words. Over a decade and more of studying rural politics in Russia and Ukraine, and over the seventeen-year period during which I have conducted research in post-Soviet countries, I have come across the statement, "everything is destroyed, like after the war" hundreds of times. It may very well be true that some individuals in rural eastern Ukraine look around them at wasted farmland, ruined buildings, and broken people, and conclude that their landscape indeed resembles a world of sixty years before, when Hitler's armies and Soviet forces had only recently vacated land where they had lingered in front-line battle and occupation for over a year. But it is also true that this trope has become part of the post-Soviet cultural landscape and can be repeated without thinking, sub-

stituting, in Arendt's (1994) formulation, cliché for thought. In Yevhen's case, he was an infant and a toddler in the years following what came to be known as the Great Patriotic War. It seems unlikely that his description proceeds directly from personal observation and experience: rather, he is drawing upon a shared social metaphor.

Conversely, Yevhen's description of his wife crying over her loss of land seemed, to my mind, implausible. It is, of course, possible that she did mourn the absence of a land share, and that the additional land rent income would have made all the difference for the couple. But in years of conversations with land shareholders in the region, I have never come across such open regret in the face of dispossession. It was the exceptional quality of this statement that raised my suspicions about the character of Yevhen's words as a performance for a foreigner. Against the background of other statements that are *too often* uttered, the question arises: how familiar or unfamiliar must an element of narrative be in order to have the ring of truth?

The thorny methodological and epistemological issues Yevhen's story raises are part of a broader problematic. If the purpose of political ethnography is not only to offer alternative epistemologies in the form of shared understandings, but also to gain an understanding of social reality (the ultimate goal, as I understand it, of realist social science) and some purchase on causality, what can this brief exchange with Yevhen reveal? Is there anything about his narrative that would show "what happened and why"? Can his individual account be aggregated in any meaningful way, if the story he tells about his life partakes in established social narratives? What can a practitioner of realist ethnography learn from this?

Some truth about power relationships can be read between the lines of his story.²² For example, Yevhen seems to have reason to believe that he should not identify the characters in his drama. With the exception of one mention of "the state," Yevhen does not name the agents of change in his experience. Instead, he uses passive or impersonal constructions to describe action: "she was let go," "thoroughly robbed," "people were brought in." He notes that he lost land but does not specify how or why, or who now owns it. In one instance, the subjects of action are inanimate objects: "Fuel prevents me. Machinery prevents me." The practice of avoiding attribution of responsibility is a Soviet discursive tradition, but it is also characteristic of subordinate populations more generally.²³ That Yevhen speaks in this way strongly suggests a lack of social power—a hypothesis corroborated by the condition of his carrots.

Key parts of Yevhen's story not only echo conventional narratives of post-Soviet life, but also suggest individual variations of historical experience

already established through the accumulation of ethnographic and documentary evidence.²⁴ Dispossession through exclusion from the community of beneficiaries of postsocialist distribution and privatization, consolidation of managerial power in the hands of outside figures, the reduction of land share holdings at the margins, cashlessness and labor migration, disintegration and dismantling of rural factor endowments, income inequality, and the destruction of socialist-era property are by now uncontested phenomena in postsocialist rural politics (Allina-Pisano 2008; Creed 1995; Humphrey 2001; Verdery 2003).

Clearly, the researcher must be aware of the existence of broad social narratives in order to interpret individual statements intelligently and situate them within a field of social-scientific knowledge. For this reason, it may not always be possible to interpret meaningfully the texts of open-ended interviews without the type of knowledge that ethnographic research can provide. Because of the existence of social narratives, the texts that result from open-ended interviews have a form that precedes their content (White 1987). A social scientist seeking to discern individual experience or counter-hegemonic discourse must know that form well enough to distinguish ritual speech that captures *Zeitgeist* from individual speech that means to convey information about specific experience.

To put it a different way, ritual speech tends to be marked by repetition across individual narratives. The *gute Mensch* Stasi agent character in Henckel von Donnersmarck's screenplay *Das Leben der Anderen* (The Lives of Others) provides a useful description of a related phenomenon when he observes that persons telling the truth under interrogation reformulate their story each time they tell it, producing slight variations in each repetition. In contrast, persons who are lying (for whatever reason) do not: they tell precisely the same story each time, often using the same words.²⁵ In the face of an ethnographer's request for information, repetition may mark the telling of a shared social narrative, while departures from such a script may signal an account intended to describe individual experience.²⁶

Yevhen's narrative also points to the necessity for ethnographers to understand the world of politics and policy in the metropole in order to interpret their interlocutors' statements. Yevhen's words taken at face value, in isolation from knowledge of formal institutions, convey only partial knowledge. For example, the state did not, technically, "seize" his savings. Rather, amidst the hyperinflation and currency devaluation following the Soviet collapse and the implementation of structural adjustment policies, state banks temporarily froze assets as the currency plunged, leaving the population with savings that lost all value. Yevhen, like most others of his genera-

tion, experienced this policy as seizure—a characterization that is not only a representation of his worldview, but also an expression, couched in metaphor, of a reality shared by all, as seen from below.

Conclusion

This chapter has suggested that a realist approach to ethnography can illuminate truth claims and identify causality more reliably than some other types of qualitative social science inquiry—whether in the world of individual action (here, murder) or economy (informal petroleum trade). In addition to enriching social research with the elaboration of alternative epistemologies, ethnography can contribute in important ways to debates about causality within positivist social science traditions. Realist ethnography also can provide contextual detail to illuminate the informal power relationships that underlie formal politics, as in the village networks that shaped privatization. Finally, armed with knowledge of social narratives, the realist ethnographer can extract truth claims from field notes and interview texts in much the same way that practitioners of other approaches use documentary evidence to build and test arguments.²⁷

Ethnography and ethnographically informed qualitative research provide windows onto hidden politics and an opportunity to recalibrate the vision of histories and explanations as told from above. Work that is close to the source, whether in villages or the halls of legislatures, can reveal incentive structures, causal mechanisms, and patterns of action that drive formal politics, telling us not only how their meaning is understood, but also how they work. In this sense, ethnography can provide not only a different view, but also a view that more fully and accurately expresses the content and meaning of politics.

Notes

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1. It should be noted that such a move is not solely the product of right-wing politics. Here, the "hermeneutics of suspicion" (Ricoeur 1970) accommodates the hermeneutics of belief.

2. When discussing the conflict between this particularistic interpretation of a biblical text and the field of evolutionary biology, one should bear in mind that neither of these worldviews addresses the question of the origin of life. Rather, they both make claims about change in the physical world over time.
3. I do not intend to suggest any affinity between Holocaust deniers and proponents of intelligent design. However, their epistemologies have two things in common: first, an approach to knowledge that privileges *a priori* belief over evidence; second, a rhetorical strategy of using pseudo-empirical methods to convince others to adopt their beliefs.
4. *Beim Häuten der Zwiebel* (Peeling the Onion) is the title of Günter Grass's 2006 autobiography in which he reveals, among other things, his wartime participation in the Waffen SS.
5. This is the "substantivist" study of economy that Kubik contrasts with neoclassical theory in chapter 1.
6. A list of such research would be long. The work of James C. Scott provides but one example. In socialist and postsocialist societies, authors conducting ethnographic research (or research with what might be termed an ethnographic sensibility), such as Katherine Verdery, Michael Burawoy, Kate Brown, and Alena Ledeneva reveal causal mechanisms even as they examine layers of social meaning. In this volume, chapters by Elisabeth Wood and Katherine Cramer Walsh exemplify this approach.
7. However, realist ethnography need not fall into the trap of attributing false consciousness to political actors (see Scott 1990, 70–107).
8. See Wendt and Shapiro (1992) for a discussion of scientific realism in the context of political science.
9. For a discussion of this proposition in another field, see Brewer and Lambert's (2001) discussion of conditions that shape degrees of "theory ladenness" of observation in cognitive psychology.
10. On process versus product in scientific realism, see Shapiro (2005, 9).
11. It seems possible, in addition, that ethnographers may sometimes overestimate the extent to which our interlocutors care what we think.
12. For a brief explication of how ethnography differs from other tools of qualitative social science research, see Bayard de Volo and Schatz (2004).
13. "Truthiness," as used by Colbert, means the quality of being known from the gut, rather than the head (*The Colbert Report* 2005).
14. In North America, 89 octane; octane numbers are calculated differently in different countries.
15. The DT sign provides an illustration of the type of text that can be an appropriate object of realist analysis: it is an object "out there" that has a single, unambiguous meaning for the author and intended reader.
16. For a satirical exploration of this practice, see O'Brien (1996).
17. Here, I do not presume, though neither do I exclude, a binary consciousness that admits to a distinction between authentic or "true" thought or speech, and dissimulation. Works that suggest the existence of such a dichotomy include Scott (1990) and Wedeen (1998). Alexei Yurchak (2003) offers a critique of this approach.
18. On this point, see Ortner (1995).
19. Timothy Pachirat powerfully examines the effect of position on knowledge in his chapter in this volume.
20. The imagined scene on the Prague balcony may offer a more appropriate analogy

- than Soviet-era photos, for the individuals erased from Yevhen's narrative nonetheless leave traces in it, like the hat of Clementis that remains on Gottwald's head.
21. On conversational rituals in a Soviet context, see Ries (1997).
 22. See Timothy Pachirat's chapter for another example of how ethnography can allow the researcher to read power relationships between the lines.
 23. For an exploration of this subject in the Ukrainian media, see Ryabinska (2006).
 24. This instance could be interpreted as support for Schatz's contention that some ethnography serves only to correct perspectives already established by other methods. See Schatz (2007). However, the use of ethnography for this purpose in this instance does not render it useless for establishing truth claims and causality more generally, as in the following examples.
 25. The implication here—that in post-totalitarian contexts, the ethnographer and the policeman have much in common—is hardly an original suggestion. While the product of ethnographic work differs in several important ways from state surveillance—ethnographers' interlocutors are not identified, and the results of research are not secret—the close formal resemblance, *as it may be experienced by those who interact with ethnographers*, should condition our reading of ethnographic texts. The farmers with whom I interact in postsocialist societies regularly draw this connection, and as one of my Moscow colleagues in the natural sciences recently commented in reference to my research, "well, you're almost a spy [*razvedchik*]."
 26. This accounts for one of the problems encountered by ethnographers in places where ethnographic research has been done before: over time, people develop narratives that they then produce when prompted by the presence of a notebook and pen, or of a voice recorder. Elisabeth Wood further discusses issues of memory and testimony as evidence in chapter 5 of this volume.
 27. The production of knowledge through ethnography is heir not only to the interpretive tradition of recent decades, but also to the more ancient practices of observation and recording executed by more conscious servants of power: the scribes, administrators, and officials who tracked, recorded, and filed their notes about the world around them.