

FEMINIST ETHNOGRAPHY

Research, Collaboration, and Intelligence: When Governments Take an Interest in Feminist Ethnography

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The discipline of anthropology has been wracked with controversy since the 2007 establishment of a new program within the United States military, which officially employs anthropologists and other social scientists to collect “ethnographic intelligence” on local populations in Iraq and Afghanistan. The program, the Human Terrain System (HTS), was created to help U.S. military personnel better understand local cultural contexts. As part of this program, experts throughout the academy are being contacted by State Department officials to provide information on topics of interest to those in the Pentagon. The politicization of ethnographic fieldwork has posed a series of moral dilemmas for anthropologists, particularly feminist anthropologists who work with already vulnerable populations. This article proposes to examine the question of collaboration with reference to the HTS and recent debates raging among anthropologists about whether or not to cooperate with the U.S. government or any foreign government. Drawing on the author’s own experiences conducting fieldwork among Slavic Muslims in Bulgaria, during which she was “invited” to share her findings with both the Bulgarian and American governments, the goal of the article is to openly discuss these dilemmas and offer some brief suggestions about how to navigate the murky waters of doing research in an increasingly fraught global context.

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Leyla is a young doctoral student in anthropology at a major East Coast research university. A Lebanese Christian, she speaks Arabic fluently and is writing a dissertation on gender relations and girls' education among Egyptian Copts. She spent the last two years conducting fieldwork in Alexandria, but today she is sitting in the home of two Shi'a teenage sisters in a small village in far-eastern Iraq, asking them about their dreams and aspirations for the future. One of the girls, Amira, says she wants to be a medical doctor, and the other, Sabeen, would like to become a biochemist and conduct pharmaceutical research. As a student of cultural anthropology, Leyla is well aware of feminist critiques of ethnography and is hypersensitive to the power imbalance between her and the two young women. She knows that they are eager to speak with her, because they view her as an educated and independent woman—something both girls would like to become. Her position of authority is even more palpable because of the U.S. Army uniform she wears as a member of a "Human Terrain Team" (HTT) deployed to understand local Iraqi cultures, with the hope of reducing the number of civilian casualties.

Leyla is a very good listener. She is respectful of Amira and Sabeen and open about her purpose. Both girls fear that a successful Shi'a insurgency will result in a drastic erosion of rights for Muslim women. Leyla believes that championing women's rights in this region might lead to greater support for the continuing U.S. presence in Iraq. Leyla hopes to convince the U.S. forces to help improve the material conditions of life for women in the village, and specifically to reopen an all-girls school, which has been closed for the last several years due to a lack of teachers. This is Leyla's seventh visit with Amira and Sabeen, and they have grown warmer and more trusting of her, despite her U.S. military uniform. They know she is an anthropologist and not a soldier.

Leyla asks Sabeen why she thinks the insurgents will implement Shari'a law if they come into power, and Sabeen casually answers, "Because the Iranians are funding them."

Leyla presses, "How do you know that for sure?"

Amira jumps in: "Our brother, Malik, crosses the border two or three times a week for trading. He tells us things."

"Isn't it dangerous to cross the border so frequently?" Leyla asks, concerned for their brother's safety. The border region is heavily secured by U.S. troops.

"Oh no, it is fairly easy now that they dug the tunnel. That is why there is more food available recently."

"And perfume!" Sabeen adds, smiling and holding out a wrist for Leyla to sniff.

Leyla does not ask where the tunnel is. She does not want to know. But she takes careful notes on the improved food supplies for her daily report to the HTT commander. The conversation then turns to the differences between Iraqi and Iranian pop stars, and whether or not Leyla has ever met Madonna. The girls want to know what high school is like in the United States, and if it is true that teenagers can drive their own cars. Leyla is only too happy to be asked questions by her informants; their curiosities give her insight into their worldview. She spends the rest of her

time outlining both the joys and horrors of American teenagers, with their cliques, competitions, and cars.

Three weeks later, Leyla returns to the house of Amira and Sabeen, only to find the two young women in mourning. United States troops had conducted a midnight attack on the secret border tunnel and the girls' brother was killed. He was the oldest male in their household and without him, there is no one to support them. Although both girls believe that they are too young to be married, it is the only solution, and the elder women in the household have already set about finding them suitable husbands.

No one suspects that Leyla had anything to do with the attack, though she herself thinks differently. She storms back to her HTT leader and asks, tearfully, if her report was responsible for the death of the girls' brother.

The team leader responds: "It was a military target. The Iranians were running weapons to the insurgents. We knew the tunnel existed, but could not find it. Once we knew that the brother was using it to trade, we had him followed. It is unfortunate that we lost him in the attack. But our troops and the people of this village will be safer now that the tunnel is destroyed. I am sorry."

Stunned, Leyla immediately resigns her post. She returns to the United States to finish her dissertation on Egyptian Copts.

The above anecdote, thankfully, is a fictional imagining to illustrate the burgeoning array of ethical dilemmas facing anthropologists, and particularly feminist anthropologists, in the profession today. The discipline of anthropology has been wracked with controversy since the 2007 establishment of a new program within the U.S. military, which officially employs anthropologists and other social scientists to collect "ethnographic intelligence" on local populations in Iraq and Afghanistan. The program, the Human Terrain System (HTS), was ostensibly created to reduce both U.S. and Iraqi-Afghani casualties by helping U.S. military personnel better understand local cultural "terrains" and attitudes toward American operations. Willing anthropologists can earn up to \$300,000 a year—at least five times the salary of most first-year assistant professors—to help the United States win what military publications simply refer to as GWOT (Global War on Terror).

It is not only those willing to directly collaborate with the military who are involved; experts throughout the academy are also being contacted by State Department officials to provide information on topics of interest to those in the Pentagon. This article proposes to examine the question of such collaboration, with reference to HTS and recent debates raging among anthropologists about whether or not to cooperate with the U.S. government or the governments of foreign countries. Indeed, the governments of the countries wherein anthropologists do their fieldwork can also demand to be informed of an ethnographer's ongoing research, particularly if the research is about contested ethnic-, religious-, or cultural-minority populations. Thus, the politicization of ethnographic fieldwork poses a series of moral dilemmas for anthropologists,

particularly feminist anthropologists, who often work with already vulnerable populations of women.

The militarization of academic knowledge is not a new phenomenon, nor is it limited to the field of anthropology. In her book *The Age of the World Target*, Rey Chow (2006) examines the history of area-studies programs within the American university system. These academic units were created following a Department of Defense (DOD) initiative, which divided the world into neat geographic regions that became potential targets for future U.S. interventions. Many universities also rely heavily on federal grants, with implicit or explicit links to U.S. foreign-policy or military agendas.¹ HTS is just another instance of the militarization of the academy, but it goes one crucial step further: scholars collecting ethnographic intelligence are now wearing military uniforms and are embedded in military units deployed in war zones. How is working for HTS different from accepting a National Security Education Program grant? Does this type of fieldwork violate ethical standards that enjoin anthropologists to “do no harm” to their informants? If so, what other types of collaboration will also be deemed ethical violations? On the continuum of different types of cooperation, at what point should the potential for harm to one’s research subjects justify the abandonment of a project? And who gets to make this decision?

These are questions that are of particular salience to me. Drawing on my own experience of conducting fieldwork among Slavic Muslims in Bulgaria, after which I was invited to share my findings with both the Bulgarian and U.S. governments, my goal in this article is to discuss these dilemmas through a feminist lens. Although these moral dilemmas lack easy solutions, it is necessary to have an open debate about the use of scholarly knowledge in the context of war. These are pressing ethical issues, especially for feminist anthropologists who have a history of deconstructing the legitimacy of ethnographic knowledge and the conditions of its production. I begin this article with a brief survey of the feminist literature on the perils of ethnography, and then discuss recent debates surrounding the U.S. government’s controversial HTS program. The latter half of the article briefly explores my own experiences working in Bulgaria and the challenges I faced as I tried to ethically conduct my research among a community of Muslims that both the U.S. and Bulgarian governments suspected of “extremism.” It is my hope that these reflections will contribute to a broader discussion about feminist research ethics in an increasingly fraught global context.

Participant Observation and Feminist Critiques of Ethnography

Ethnographic participant observation is a methodological approach in which researchers meet and interact with their research subjects for extended periods of time in the field. The repeated contact and familiarity that is fostered by participant observation theoretically allows researchers insight into the social

construction of meanings and identities outside of a formal research setting. The key to participant observation is the extent to which the scholar is submerged within the culture she is studying. Ethnography requires trust, and trust is gained by spending months or years living together and sharing the day-to-day experiences of those one studies. Over time, relationships are built and friendships formed, and through these, successful researchers hope to understand the host culture on its own terms. Ethnographic data consists of field notes, which are recorded by the researcher as she reflects on her daily activities in critical detail. When the time in the field is finished, these notes theoretically become the primary text from which the scholar produces an ethnography, which is usually a book or series of articles that synthesizes the cultural data around a central theme or key argument, furthering our collective understanding of social processes and identity formations in other cultures or subcultures (Emerson, Fretz, and Shaw 1995; Reinharz 1992).

Because anthropology and sociology are considered social sciences, there is a disciplinary desire to maintain the presumed objectivity of the researcher. Feminist philosophers have heavily criticized this lens of objectivity, arguing that scientific objectivity is merely the scholarly institutionalization of a distinctly male subjectivity, the social privileging of a masculine ontology (Alcoff and Porter 1992; Belenky, Clinchy, Goldberger, and Tarule 1986; Haraway 1988, 1989, 1991; Harding 1986a, 1986b, 1990, 1991; Longino 1989, 1990). The focus on objectivity led to the privileging of supposedly positivist research methods, which required a strict separation of the social scientist from the objects of study (Reinharz 1992). This critique of positivism led to the promotion of alternative, more interpretive methods, such as ethnography, as more suitable for feminist research (Reinharz 1990). Feminist ethnographers initially thought that they could break down the barriers between anthropologists and their informants by focusing on intimacy, empathy, and transparency in the field (Reinharz 1984; Sprague and Zimmerman 1989; Stacey and Thorne 1985; Warren 1988). Despite some early attempts to make ethnography more subjective, professional standards in anthropology still largely revolved around the maintenance of a presumed objective lens through which cultural knowledges are created. While feminist epistemological critiques of science have tempered claims to the absolute objectivity of ethnographic “truth,” and ethnographies are now written with careful self-reflexive attention to the operation of power in the field, the basic *modus operandi* of participant observation has essentially remained unchanged.

Within this purportedly objective model of cultural anthropology, the worst sin that an anthropologist can commit is to “go native”—that is, to completely adopt the worldview of the studied population. But this is easy to do, since many researchers who spend extended time in the field will develop friendships and perhaps romantic relationships with locals. In some cases, scholars marry into or adopt children from the cultures they study, creating personal bonds and commitments to a society that make objectivity more difficult. Even without these

personal connections, the researcher will ultimately write both the field notes and the subsequent ethnography about a group of people who have generally been kind and welcoming to her for an extended period of time. Indeed, the entire process of conducting ethnographic research is fraught with uncertainties and serendipities, and even the best-trained researchers will face a wide range of seemingly random choices about where to go, with whom to speak, and how to interpret words and behaviors that may have multiple meanings. Despite this, anthropology as a discipline has historically made definitive truth-claims about certain peoples and cultures, and in many cases, these truth claims directly aided and/or justified the imperialist designs of the researchers' home countries. Indeed, Talal Asad (1973) has argued that professional anthropology has, from its inception, had links with the colonial ambitions of European powers.

Over time, feminists have been actively involved in the critique of anthropology and its imperial entanglements. One of the most influential early articles attacking the essentializing sex bias in Western anthropology was Michelle Rosaldo's (1980) "The Use and Abuse of Anthropology: Reflections on Feminism and Cross-Cultural Understanding," wherein sex differences once universally viewed through a Western anthropological lens as biologically based and unchanging across cultures were recognized to be socially constructed. Shortly thereafter, the whole practice of "writing culture" and the entire ethnographic project came under intense scrutiny in the works of scholars like Jim Clifford, George Marcus, and Michael Fisher (Clifford 1983; Clifford and Marcus 1986; Marcus and Fisher 1986). Once again, the presumed objectivity of the Western anthropological gaze was called into question, and ethnographic knowledge produced "under Western eyes" (Mohanty 1988) was suspect, particularly if it justified the so-called civilizing mission of the West.

With ethnography under attack, Judith Stacey (1988) and Lila Abu-Lughod (1990) both asked: "Can there be a feminist ethnography?" Both warned that the presumed shared oppression of women around the world did not mitigate power imbalances between Western women anthropologists and the populations of women they studied. Self-reflexivity about research positionality in the field was encouraged, and feminist ethnographers were increasingly adulatory of the inherent subjectivity of their discipline. Feminist ethnography became a favorite research method in the quest to discover new feminisms and to give voice to the subaltern, marginalized populations that had been unfairly subsumed within the global sisterhood rhetoric that had characterized mainstream Western feminism in the United States and western Europe. But Stacey also warned that feminist ethnography might end up being more exploitative than traditional positivist methods, since the guise of friendship and shared experiences between women possibly rendered women more trusting and subsequently more vulnerable than they might otherwise be in a research relationship that was less personal.

As part of this introspective turn in anthropology, Diane Wolf (1996) edited a collection of essays in *Feminist Dilemmas in Fieldwork*. In her introduction,

she argued that these dilemmas “gnow at our core, challenging our integrity, our work, and at times the *raison d’être* of our projects. Feminist dilemmas in fieldwork revolve around power, often displaying contradictory, difficult and irreconcilable positions for the researcher. Indeed, the power dimension is threaded throughout the fieldwork and postfieldwork process and has created a major identity crisis for many feminist researchers” (1). In the volume, a wide cross-section of feminist ethnographers reflected on the ethical and intellectual contradictions of being feminists and studying disenfranchised populations both at home and abroad. In a sustained moment of soul-searching, these scholars wondered whether or not it was ethical to lie about one’s marital status when doing research in a rigidly patriarchal culture where single women living on their own are unacceptable; others questioned whether it made sense to hide one’s sexuality from one’s informants or to be elusive about one’s religious background, class, or income status. All of these questions were precipitated by the robust feminist literature on ethnography that allowed privileged women to study those with less privilege, as long as the researchers were satisfactorily cognizant of, and willing to discuss in print, how power relations in the field shaped the construction of knowledge about the particular cultures studied. No longer could ethnographers hide behind the veil of presumed social scientific objectivity; there were several key questions that had to be answered before any cultural analysis could begin. These were questions like: How were you introduced to the community? Who were your main informants? What was your relationship to them? What did they get out of the relationship? How did you explain to them what you were doing? And, most importantly, how did you protect identities so that your research causes no harm to the community studied or to specific individuals within it?

In response to these many critiques of the discipline, the American Anthropological Association (AAA) decided to revisit and revise its professional Code of Ethics in the mid-1990s. The final document, which was approved in June 1998, clearly spelled out the moral obligations of individual researchers to their informants. The code states:

Anthropological researchers have primary ethical obligations to the people, species, and materials they study and to the people with whom they work. These obligations can supersede the goal of seeking new knowledge, and can lead to decisions not to undertake or to discontinue a research project when the primary obligation conflicts with other responsibilities, such as those owed to sponsors or clients. These ethical obligations include . . . [t]o avoid harm or wrong, understanding that the development of knowledge can lead to change which may be positive or negative for the people or animals worked with or studied.

Following these developments, a focus on the production of social theory and postmodern critique of previously written ethnographic texts became a

significant trend in anthropological scholarship. This analytical inward turn in the discipline led many cultural anthropologists down a more theoretical and observational path. Many young scholars abandoned the idea of fieldwork for fear of reproducing the same hegemonic Western-bias inevitably produced by Western scholars trained at Western universities and ultimately produced for Western academic audiences. A new generation of “armchair” anthropologists were bred who were mindful of the power imbalances and colonial history of the discipline. In large part, they preferred to theorize about forms of Western cultural imperialism. Certainly, there were still many anthropologists who continued to conduct fieldwork and write traditional ethnographies, but there was a parallel trend of a momentarily more fashionable anthropology, which focused on genealogical excavations of particular universalisms or on the deconstruction of historical narratives. This trend produced much valuable work, but led to a relative abandonment of the field and an aversion to direct encounters with potentially vulnerable informants. Today, some anthropologists have begun to call for renewed attention to the importance of fieldwork and the intellectual value of face-to-face interaction with informants in the context of their daily lives.

In 2007, Princeton anthropologist John Borneman (2007) published *Syrian Episodes*, in which he calls for anthropologists to get back in the field and start experiencing difference once again, despite all of the complicated relationships and power incongruities that inevitably result from such encounters. Borneman honestly describes his sexual flirtations and relations with some informants, and reflects openly about how other informants were later questioned by the Syrian police and possibly put at risk because of their contact with him. Borneman himself becomes the object of investigation by the Mukhabarrat—the Syrian secret police—and worries about how to conduct research under surveillance. He writes:

They make [an informant] Ziyad promise to write detailed reports about our meetings, about whom I meet, what I want to know, and if I am corrupting the youth. . . . I tell him to write the truth about what I say; I am doing nothing wrong or illegal. . . . But at all costs, I admonish him, do not reveal the identities of other people, which is now my major concern. How do I learn anything without getting people in trouble for merely talking to me? (250)

This passage reminds us that ethnographic fieldwork can be dangerous for both the anthropologist and the informants who speak to him, and that any fieldwork encounter will always raise new ethical questions about whether the risks are worth the rewards. Borneman’s book may be fraught with uncomfortable power relations (and indeed may provoke the ire of many a feminist), but it supports the idea that intercultural contact between researcher and informant can still produce valuable ethnographic insights despite the complicated and perhaps exploitative conditions of its production. As a Fulbright scholar funded

by the U.S. government after 9/11 and as someone who was also being followed by the Mukhabarrat, any knowledge produced through his ethnographic work had the potential to be used for purposes with which Borneman may not have agreed. But does this mean that he should not have done the research at all? And upon what ground can anthropologists maintain ethical “purity” in the field?

In a disciplinary moment when there is a renewed attention to the importance of the field, it is troubling that this new trend coincides with increasing military interest in ethnographic intelligence. For those who believe that understanding daily practice and exploring the quotidian within its own cultural context makes for the production of richer and more informed social theory, new projects such as HTS necessarily throw up a crop of disturbing ethical issues, to which feminist scholars must pay attention. If fieldwork is predicated on power imbalances and inevitably facilitates some form of intellectual exploitation of the researched, it may still be justifiable if the product of that research can ultimately benefit the community studied. But what if that product does harm rather than good, either directly or indirectly? And how are individual ethnographers to decide which is the more likely outcome?

Armed Ethnographers: The Human Terrain System (HTS)

Although the military was increasingly aware of its own shortcomings in understanding different cultural contexts after September 11th, 2001, it was not until it became clear that the insurgency in Iraq would not easily be defeated that the military began to realize the need for detailed knowledge about local populations. In particular, the idea that the “liberated” Iraqis would welcome U.S. troops with open arms turned out to be a terrible farce. More embarrassing was the failure of the United States in understanding preexisting ethnic and religious divides that would complicate the transfer of power to a new Iraqi government. Similar mistakes were made in Afghanistan, such as failing to realize the long-term consequences of allying with the Northern Alliance. As the “kinetic operations” of both wars wound down, U.S. soldiers were being asked to “win the peace” as occupying armies against scattered and relatively disorganized insurgent forces.² Although there were civilian specialists who had warned about the potential failures in Iraq and Afghanistan, the military ignored their advice, perhaps because those who had provided this advice were overwhelmingly opposed to the war. Recognizing this problem, senior officials in the military began calling for more “culture-centric warfare” (Scales 2004), and some proponents of the HTS asserted that “[c]onducting military operations in a low-intensity conflict without ethnographic and cultural intelligence is like building a house without using your thumbs” (Kipp, Grau, Prinslow, and Smith 2006). The military wanted insiders committed to the U.S. foreign-policy objectives to gather its own cultural intelligence.

The HTS program was the brainchild of anthropologist Montgomery McFate and her colleague Andrea Jackson, who wrote the 2005 article “An Organizational Solution for DoD’s Cultural Knowledge Needs.” From the initial seed of an idea for the HTS, the military began the practical implementation of the program over the next two years. The concept of the “human terrain” is defined by the military as “the human population and society in the operational environment (area of operations) as defined and characterized by sociocultural, anthropologic, and ethnographic data and other non-geophysical information about that human population and society. . . . It includes the situational roles, goals, relationships, and rules of behavior of an operationally relevant group or individual” (Kipp, Grau, Prinslow, and Smith 2006). The purpose of HTS is to help the U.S. military to “make sense of personal interactions, to trace the connections between people, to determine what is important to people, and to anticipate how they could react to certain events” (Renzi 2006). This institution supports U.S. foreign-policy objectives in areas considered to be of strategic importance, which, according to McFate and Jackson (2005), include “Eastern Europe, the Maghreb, Sub-Saharan Africa, the Middle East, Southwest Asia, Central Asia, and Southeast Asia” (20).

Once the military was convinced of the necessity of the HTS, a no-bid contract for \$40 million was handed over to British Aerospace Electronic Systems to hire and train the required anthropologists (Ephron and Spring 2008). The HTS program consists of small, brigade-level Human Terrain Teams (HTTs) that include at least two civilian and at least three military personnel each. These are: A military team leader, a cultural analyst (anthropologist), a regional specialist (with academic qualifications), a human-terrain research manager, and a military human-terrain analyst. The civilian members of the team are uniformed contractors to the military and can choose to be armed while in the field. An initial period of training at Fort Leavenworth in Kansas is followed by deployment to either Iraq or Afghanistan.

The HTTs are supported by a variety of forms of what the army calls “reachback.” Principally, the Foreign Military Studies Office at Fort Leavenworth has created a Reachback Research Center (RRC), which also employs social scientists as cultural analysts based in the United States. A second RRC is based at Oyster Point, Virginia. The RRCs synthesize and make accessible the data coming back from the HTTs, and also make contact with “a network of researchers throughout the government and academia to conduct research and get answers” (Kipp, Grau, Prinslow, and Smith 2006). To support its research function, the military is building a database of Subject Matter Experts (SMEs), who have specific regional or cultural knowledge about populations of interest. Many of these SMEs are likely to be academics, and specifically anthropologists, prompting some concerned American professors to accuse the U.S. military of trying to “weaponize anthropology” (Price 2006).

The American Anthropological Association Strikes Back

The establishment of the HTS and the deployment of professional anthropologists to the field to gather “ethnographic intelligence” prompted a fierce debate among anthropologists, culminating in an official statement (2007) of the executive board of AAA strongly disapproving of the HTS program, giving five specific reasons for its decision. First, it claimed that working as military contractors in war zones might make the anthropologists indistinguishable from other military personnel and therefore unable to fulfill their ethical responsibility to tell people who they are and what they are doing. Second, as military contractors, anthropologists may have conflicts of interest and may not be able to fulfill their responsibility to ensure that no harm comes to those studied. A third issue is the compromised ability of military contractors to obtain voluntary, informed consent from their informants in a war zone under potential conditions of distress and in the context of the vast imbalances of power between researcher and researched. In its fourth reason, the board reiterated its concern about doing no harm, stating that

[a]s members of HTS teams, anthropologists provide information and counsel to U.S. military field commanders. This poses a risk that information provided by HTS anthropologists could be used to make decisions about identifying and selecting specific populations as targets of U.S. military operations either in the short or long term. Any such use of fieldwork-derived information would violate the stipulations in the AAA Code of Ethics that those studied not be harmed.

Finally, there was a worry that “[b]ecause HTS identifies anthropology and anthropologists with U.S. military operations, this identification—given the existing range of globally dispersed understandings of U.S. militarism—may create serious difficulties for, including grave risks to the personal safety of, many non-HTS anthropologists and the people they study.”

The AAA’s concluding statement also included very political language, which placed its disapproval in a specific historical context and seemingly limited the context of the statement to the Iraq war. The AAA board opined: “In the context of a war that is widely recognized as a denial of human rights and based on faulty intelligence and undemocratic principles, the Executive Board sees the HTS project as a problematic application of anthropological expertise, most specifically on ethical grounds.” The inclusion of this one sentence set off a firestorm of impassioned support and criticism of AAA, as scholars and students across the country examined the various arguments for and against collaboration with the military in its quest for greater cultural knowledge.

Senior anthropologists in the field immediately came out in favor of the statement. Representative of this support was a posting on the AAA blog (2007)

hosting a discussion of the statement against the HTS by James Ferguson, the chair of Stanford University's Department of Anthropology. He stated that

I think the Executive Board's statement is excellent, and something we will be proud of when we look back at this sorry period many years from now. I understand the wish to separate the concerns about anthropologists in the military from views on this particular war. But I think there's no escaping the fact that anthropological complicity with a military operation (no doubt problematic in the best of cases) is much more troubling when that operation is an imperial war of occupation, initiated through an unprovoked invasion and carried out using methods that include the systematic use of illegal detention and torture.

Also in support of the AAA statement was a group of anthropologists that founded the Network of Concerned Anthropologists.³ In a December 2007 radio interview with Amy Goodman, David Price, a founding member of the network, worried that the HTS was merely a repeat of a very controversial Vietnam War-era program that also used anthropologists to provide cultural intelligence on the Vietnamese:

The claim by Human Terrain is that they can reduce casualties by giving more nuanced information to people in battle situations. But there's a lot more to it than that, especially in that people in the Pentagon see this as being linked to the CORDS program. [The] CORDS program in Vietnam was used to map human terrain, to identify suspected individuals and groups that the military believed were sympathizers for the Viet Cong, who were, in the Vietnam era, targeted for assassination.

Once again, the injunction to "do no harm" was stated as a key ethical consideration when deciding whether or not to work with the military. Network activists started their own Web site and actively disseminate information about military and State Department programs that are trying to recruit PhD students and junior scholars in anthropology.

Those associated with the program or who are in the military took the opposing position, claiming that anthropologists had an ethical responsibility to try and do good with their cultural expertise. Writing in *The Chronicle of Higher Education*, Marcus Griffin (2007), an assistant professor of anthropology and sociology at Christopher Newport University who was serving with the Second Brigade of the First Infantry Division in Baghdad, defended the HTS program:

Whether you think the United States should have entered Iraq by force (which I don't) and toppled Saddam Hussein, the inescapable fact is that we are here. Now academics have a choice: We can apply our specialized skills in the field to ameliorate the horrors of war, stem the loss of both American

and Iraqi lives, and improve living conditions for Iraqis, or we can complain from the comfort and safety of the faculty lounge. (Bio)

Other academics criticized the AAA for trying to hold anthropologists to a “priestly standard of ethical purity.” Noah Feldman, a Harvard law professor, was quoted in the *Wall Street Journal*, addressing the question of whether any form of collaboration compromises academic integrity: “Are academics so much purer than anybody else that we can’t ever be in situations where we are confronting tough ethical choices? . . . If academics didn’t get involved with these kinds of difficult questions, maybe all that would be left is a department of Kantian philosophy . . . Then we would be pure, but we would be irrelevant” (Goldstein 2007).

What was fascinating about the ongoing debate was the dominance of male voices arguing either for or against the program. On the AAA blog (2007) there were only a handful of women who posted comments, and those who did seemed inclined to stake out some kind of middle ground in the debate. In particular, feminists worried about the precedent set if the AAA used its political opposition to the war as the justification for its ethical judgments. In one comment, anthropologist Elizabeth Dunn opined:

I am in complete agreement about the Board’s statement upholding the principles of informed consent and doing no harm to the people we study. . . . [But] I’m concerned about the Board’s statements of disapproval about the war. While I, too, disapprove of the war, I’m very concerned about the precedent being set by having AAA dictate political views to its members. As a scholarly association, we have to preserve our members’ rights to a diverse range of opinion, and we have an obligation to foster discussion rather than stifling it. Even if the majority of us hate these viewpoints (and I do), there still needs to be room for anthropologists who are politically conservative, who approve of Bush, and who believe that the war is being conducted for the right reasons. We need to do so even if we disdain these views, because the very existence of dissent and minority views is the lifeblood of any scholarly organization.

In a later posting on the AAA blog, Saida Hodzic, a professor of gender and women’s studies at George Mason University, asked even more challenging questions about anthropological cooperation with governments, corporations, or institutions that occupy positions of power over vulnerable populations. She pinpointed the political nature of the AAA’s decision to condemn collaboration with the military in the context of the Iraqi war, but not to openly condemn similar ethical violations committed by anthropologists, writing that

I am opposed to the HTS, but I’d like to have a wider set of debates about it. . . . I’d like to expand the existing debate on ethics and move beyond it. The question, for me, is not whether HTS violates the anthropological Code of Ethics. I believe that it does. But so do many other anthropological

engagements the AAA does not oppose. And this is where politics enters the game. Is the HTS involvement an absolute violation of AAA code of ethics or is it somewhere on the continuum of violations? What is the difference between an HTS ethnographer and, say, an ethnographer analyzing health-seeking behavior in [the] service [of] an insurance company that's trying to cut its cost at the expense of quality of care? Or a consultant to the World Bank hired to facilitate a resistance-free displacement of peoples for the sake of building a dam? Or, on the other end of the political spectrum, an anthropologist who studies up, but due to his/her allegiance to the "people" writes against the interests of the main informants? To me, the difference seems that the question of survival is more immediate in war, but the code of ethics is compromised in all these cases. Hence, the decision to condemn this, but not other violations, is a political decision.

Both Dunn and Hodzic pointed to the uncomfortable and untenable dichotomy being proposed by both supporters and opponents of the HTS: On the one hand, supporters argued that anthropologists unwilling to apply their knowledge to real-world problems were hiding in their ivory towers and were therefore irrelevant and out of touch with reality; on the other, anthropologists opposed to the HTS felt that any work that helped the U.S. government win an unjust war, and that could also potentially harm vulnerable informants, was an unforgivable ethical violation. Although feminist voices were able to temper the Manichaean nature of the debate, many questions for practicing anthropologists remained fundamentally unanswered. Given that the United States was already in Iraq and Afghanistan, and given that some anthropologists truly believed that they could provide valuable cultural information that would save civilian lives and therefore freely chose to cooperate with the military based on their own convictions, did the AAA have the right to issue a statement that defined this type of cooperation with the government as an ethical violation, while it simultaneously ignored other, perhaps equally egregious cases of collaboration that "did harm"? Who, ultimately, is to judge whether or not ethnographic knowledge in any particular context does harm? Given that there is no international, professional body that will adjudicate the final outcome of every ethnographic project, whether undertaken in conjunction with the U.S. military or as individual academic research for scholarly publication, the decision on whether or not to collaborate with governments (either officially or unofficially) must reside within the individual anthropologist. Considering that sometimes one's ability to do research in the first place is predicated on some level of cooperation (either for federal funding or in obtaining visas or residency permits), is taking a federal grant with a reporting requirement to the U.S. Department of State any less collaborative than conducting ethnographic fieldwork with the HTS program?

These are intensely personal questions that every ethnographer must face and for which there is little guidance. Do no harm is an easy enough charge in theory, but in practice, ethnographers in the field always run the risk of doing harm, especially if that harm is defined by any level of cooperation with governments. Furthermore, being in the field can also mean personal risks for the ethnographer if she chooses not to cooperate, especially in the context of war, when anthropologists might be subject to military law if they are accused of withholding valuable cultural intelligence and therefore of collaborating with the enemy. More specifically, given the backdrop of GWOT (Global War on Terror), how can one produce knowledge about Muslim communities that one day may become military targets, even if that knowledge is only being produced for academic publication? Once published, academic knowledge can be put to a multiplicity of uses, including those with which its authors do not agree. In the final pages of this article, I reflect on my own experiences doing ethnographic fieldwork among Muslims in Bulgaria, and the various dilemmas I faced when asked to cooperate with Bulgarian and U.S. government requests for cultural intelligence.

“Indiana Joanna,” or the Perils of Doing Fieldwork

In the summer of 2004, just as my first book was heading off into production, I was traveling around Bulgaria and thinking about my next project. My previous research had been on women working in resort tourism and one of my field sites had been in the central Rhodopi Mountain range, where there is a high concentration of Bulgarian Muslims (Pomaks). Over the years that I had traveled to this region (1998–2004), I noticed a steady proliferation of new mosques being built in what was otherwise one of the most impoverished areas of Bulgaria, with unemployment rates above 40 percent.

Although my initial intention was to study the role of women in the development of rural tourism, I eventually became curious about where the money for the mosques was coming from and whether or not it was related to the appearance of new Islamic practices in the region. The form of Islam traditionally embraced by the Pomaks were being challenged by new ideas about what it meant to be a “proper” Muslim. I was fascinated by the intergenerational conflicts emerging between the older women raised under communism, who considered the new forms of Islam a threat to their rights and freedoms, and the younger Pomak women, who actively embraced the new beliefs.

Despite the academic fashion for Islamic studies after 9/11, I viewed my own project as very specific and limited to the Bulgarian context, a country about which few Americans cared and where there had been no Christian–Muslim conflicts since 1989. Furthermore, I was still hoping to focus my analysis on the development of rural tourism, which some Pomaks feared would be undermined by the proliferation of new mosques. I am well aware that there are many scholars

who work on research topics of interest to their host governments, but these scholars usually go into their projects cognizant of the potential ethical issues that will be raised. Perhaps it was my own naiveté, but for the previous six years I had been left alone to toil in relative obscurity on Bulgarian women's issues. In retrospect, I foolishly underestimated how interesting both the Bulgarian and U.S. governments would find my new project and the ensuing troubles and moral dilemmas I would face as I conducted my fieldwork over the next several years, including visa troubles, my detention by the Bulgarian police, and an invitation from the U.S. Department of State to discuss my research in an "off-the-record" session in 2008.

The problems began in early 2006, when I had to apply for a long-term visa for Bulgaria. Although I had lived in the country for over a year as a Fulbright scholar during 1999–2000, had been married to a Bulgarian, and had a five-year-old daughter who was a dual U.S.–Bulgarian citizen, they initially refused to issue what should have been a routine visa. I later learned that the Bulgarian government was suspicious of my wish to live in the Rhodope among the Pomaks. I wanted to live in a town with a high concentration of Pomaks who were embracing more devout forms of Islam. Although I was initially attracted to this town because of its plans to develop mining tourism, I eventually became more interested in questions of religion, and how these newly devout Muslims fit within the larger, more secular Bulgarian Muslim community. Specifically, I had questions about the changing gender roles of newly pious women and the question of the headscarf. Some Muslim women felt that head covering was mandatory, while other Bulgarian Muslim women did not. After the 2004 French law banning headscarves in public schools, I was fascinated to see how this issue would play out in Bulgaria in the lead up to its accession to the European Union.

After several delays and logistical obstacles thrown in my path, the Bulgarian government allowed me to begin my research in the country while I simultaneously continued the legal process necessary to obtain a residency permit. I had already been living in the Rhodope for four months before I was finally going to be granted official permission to stay in the country. During this time, I had forged close relationships with several of my informants, including some women who were adherents to what was locally called "Arab Islam" and who were never seen in public without their headscarves. Two of these women had become my close friends, and my daughter often played with their children on the playground.

Thus, after months of waiting, I was invited back to Sofia to pick up my personal identity document (*lichna carta*). It was, to my great surprise, that on the day I was to receive my residency permit, I was told that I would have to come back the following day to be interviewed by a "boss" (*nachalnik*) in the Ministry of Interior. The following day I was escorted into a windowless, bare-walled room with a desk and two chairs. The chair that I was supposed to sit in

was considerably lower than the *nachalnik's* chair behind the desk. I felt small as I sunk down into it, examining what I believed to be recording equipment in the light fixtures in the room. The room screamed "communism" to me and I expected some old, pot-bellied *apparatchik* to stroll into the room in a badly cut East German suit from 1984.

I was quite surprised when a young and very handsome man in his late 30s sauntered in with a clipboard and sat on the "tall" chair behind the communist-era desk. He was soft-spoken and polite and asked me in impeccable English a series of personal questions. Then he turned to my research. Of particular concern was a recent national furor created by two Bulgarian Muslim teenagers who wanted to wear headscarves in a high school in the city of Smolyan, just thirty kilometers west of my own field site. The case had been reported widely in the Bulgarian press and was the first time that a group of Muslims had nationally advocated for recognition of their "religious rights." I told him that I was aware of the case. He then asked me:

"Are you responsible for this?"

"Excuse me?" I said, not quite understanding his implication.

"Is your purpose in Bulgaria to encourage these girls to assert their human rights?"

"No," I stammered, "I've been doing this research since 2004, long before this summer."

"But you know the girls?"

"Some of them."

"And the people who are teaching them?"

"They are all the subject of my ongoing research. An academic research project."

"Good," he said. He nodded and jotted something down on his clipboard. He finally asked me if I had any questions for him.

"Is this interview a normal procedure for Americans applying for long-term residency?"

"No," he said, matter-of-factly, "It is only for you."

"Why me?"

"Your topic is interesting to us."

I nodded stupidly. Someone knocked on the door. The *nachalnik* stood and extended his hand to me. "Thank you for your time. Your permit is approved." Then he jotted something down on his clipboard and tore off a corner of the paper. He handed me the sliver of paper with a cell-phone number on it. "Call me when you are in Sofia," he said. "I would like to speak with you again. And not in such . . . formal circumstances."

I shoved the paper into my pocket, worrying about how much my ability to stay in the country was linked to my future cooperation. I became suspicious when, a week later, my backpack was stolen as soon as I returned to the

Rhodope region. All of my documents, including my passport, *lichma carta*, driver's license, and credit cards were taken, as well as a notebook filled with recent field notes. When I filed a report with the local police, I was subjected to two days of police questioning. During that time a full investigation of what I was doing in the region was conducted, ostensibly to "help find my bag." I sat for hours on end, alone with five male police officers in a small room, answering questions in Bulgarian about what I was doing in the area. At some point, the questions slowly switched over from what I was doing to what I had learned. Given that I was still in the midst of fieldwork and had not really had time to synthesize my field notes, I made a joke: "How to make a *patanik* [a local kind of potato pancake]," I said.

The police were not amused. They wanted to know with whom I had spoken and why I was spending so much time in the mosques. Were there people who had been particularly helpful to me? When I finally asked what any of this had to do with finding my documents, one policeman merely said something about finding "a bird under any stone." Until that point, I believed that the police were trying to help me, but when one of the policemen said that my car had been seen parked outside of a restaurant in Smolyan at a time when I knew I was in a different city, I started to worry. The policeman insisted that I had met someone in a restaurant in Smolyan. That my car had been seen parked outside of it. I insisted that I had been in my field site some thirty kilometers away. That I had witnesses. He did not believe me. He asked the same questions repeatedly, When did I arrive at the restaurant? "I was not at the restaurant." Why was your car seen? "I don't know." Are you certain you were not at the restaurant? "Yes." It is very important that you tell us the truth. "I was not at the restaurant." I slowly realized that I was in a very vulnerable position. I had no money, no legal representation, and no identifying documents. They could hold me for as long as they wanted. They could ask me the same questions all day.

In the meantime, the police had gone to my field site and questioned all of my friends and informants about my activities over the past several years. They also started asking me personal questions about whom I knew and how I knew them. Also: How did I learn to speak Bulgarian? Was I married? Did I have a lover? I repeated again and again that I was an ethnographer writing a book about the Bulgarian Muslims. They asked about my field notes, and I told them that I typed them up daily in an Internet café and sent them to myself via e-mail. They asked if I kept hard copies. I said that I printed them all out when I got back to the States so I did not have to carry them around. I reiterated that I was a *pisatel* (a writer), a word that carries particular cultural cache in Bulgaria, and that I was doing research for a book about the region. At that point, I had the idea to tell them to type my last name into the www.amazon.de Web site, where my first book on Bulgaria popped up in German.

They finally let me go. After being questioned all day, I was too tired to drive back to Sofia, which was almost a five-hour trip. My nerves were a mess,

so I decided to go back to my field site and stay the night with friends. As I was about to leave Smolyan, I called a friend who warned me that he had orders to call the local police as soon as I arrived in the town. Apparently the police there also wanted to speak with me about my research. I took the hint and drove directly back to Sofia.

Once back in the capital, I immediately cancelled my passport and began the process of applying for a series of new documents. Two days later, a policeman from Smolyan called me. They had found my passport and wanted me to come back to retrieve it in person. I asked him where they had found it. He said he could not tell me. I told him that I had already canceled it. He said that I should come to pick it up anyway, that there was some procedure they needed to follow. I called the U.S. embassy. They told me there was no reason to pick up a passport that had already been invalidated. I was issued a new passport and left the country three weeks later.

Back home, I read through the hundreds of pages of my field notes, wondering if I had written anything in my notes that might cause trouble for my informants. Once out of Bulgaria, I thought I might be able to sink back down into academic obscurity. To my dismay, it was not long before someone claiming to be a Bulgarian PhD student interested in Islamic fundamentalism contacted me via e-mail. He asked me for a copy of my book manuscript, which was then in the very early stages of preparation. I was able to apologize and truthfully say that it was not ready. When he contacted me a second time, the manuscript was under review and I was able to say something about not wanting it circulated until it was under contract. At that point, he asked me to send him copies of everything that I had already published on my research (two articles and a working paper), which I did, figuring that he could have obtained these on his own (Ghodsee 2005, 2007a, 2007b). A month later, the senior political officer at the U.S. embassy in Sofia also contacted me and specifically asked for copies of my published articles. The similarity of the request unnerved me; I became increasingly worried that something I had written might do harm to someone who had helped me with my research. But since the articles had already been published, there was nothing I could do.

At that point, I went back to the book manuscript and starting obfuscating more and more details about my informants. I had already changed their names, family relationships, and descriptions of their appearances, but now I felt compelled to change identifying details about their education, professions, and personal histories. I considered changing the name of the town, but there were too many secondary sources cited in my footnotes that referred to the place by name. I cut out sections of the text and long quotes from my informants that provided too much specific information about individual people, ultimately diluting the ethnographic details and forcing me to rely more on secondary sources, such as newspapers, where I could be sure that the information was already in the public domain. I also felt compelled to provide fewer specifics

about the ideas and opinions of my newly devout local informants, refocusing my research instead on the public figures who were already being quoted in the press and on television.

As the manuscript worked its way through the painfully slow process of academic book publishing, I was able to get some distance from the project and work on other things. Then one day in April 2008, I received the following e-mail message from the president of one of the foundations that had given me money for the research. It read:

Dear Colleagues,

In an effort to maximize the dissemination of research, which you have conducted under . . . grants funded by the Department of State, this note is to let you know that from time to time, you may be contacted by State Department officials. . . . A number of our grantees have already been contacted and have been happy to discuss their research. [Foundation] . . . encourages direct contacts of this kind as part of our ongoing partnership with the Department. . . . Thank you for your cooperation in this manner, and we wish you every success in your research activities.

One week later, I was invited by the State Department to brief a new American ambassador to Bulgaria. At first, I was flattered by the invitation, but I also worried that by agreeing, I would somehow be placed in a category with HTS anthropologists, against which AAA had taken such a firm stand. I worried that my knowledge of Bulgarian culture, and particularly Bulgarian Muslim culture, would somehow be deployed against my friends and informants. How could I even know what would be done with the knowledge I produced? How could I ever know? Perhaps it was really better to stay home and deconstruct other people's ethnographies instead of venturing forth and trying to write my own. Perhaps I should have just stuck to tourism. But since I had already done the research, I did not know what to do.

To Collaborate or Not to Collaborate?

As I struggled with these personal dilemmas, I found myself sucked into the increasingly heated debate surrounding the HTS and the AAA's blanket condemnation regarding cooperation with the U.S. military. Then, in May 2008, it was reported that Michael Bhatia, a 30-year-old Oxford graduate student working for the HTS in Afghanistan, was killed by a roadside bomb (Glenn 2008). A month later, an explosion in Baghdad took the life of Nicole Suveges, a Johns Hopkins PhD student also working for the HTS.⁴ In an article in *The Chronicle of Higher Education* written shortly after these two deaths, reporter David Glenn (2008) quoted a question that Bhatia had asked at a March 2007 special committee meeting of the AAA—a meeting that revolved around the ethical questions raised by the HTS. Bhatia addressed his query to the anthropologists

before deciding to join the HTS, and he had clearly worried about the ethical implications of collaborating with the military. “If you are involved,” he asked,

to what extent can you dictate a freedom or an ability not to provide information? . . . If you are involved or are engaged with military actors on the ground and that space between reducing otherness and potentially being involved in targeting starts to get blurred, when can you say, I’m not simply going to provide that information to you? And whether that’s a space that can actually be negotiated. Or is that just naïve? (A9)

Although Bhatia ultimately joined and openly supported the HTS, his earlier question about the nature of the program in the context of war forced me to reflect on my own situation. Comfortably ensconced in my office in Maine, I actually did have a free choice over the extent to which I now collaborated with either the Bulgarian or the U.S. government, but it certainly might not have been the case in Bulgaria if the police had forced me to hand over all of my field notes (which they thankfully did not). Regarding the Department of State invitation, I sent them a narrated PowerPoint presentation summarizing my already published work, after carefully editing once again to ensure that I completely occluded the identities of my informants.⁵ Although I still had some qualms about how my research would ultimately be used by the department, I rationalized that this was research already in the public realm. But this was my choice; had I chosen not to cooperate, I assume there would have been no serious repercussions.

The HTS is a scholarly endeavor being carried out in the context of war, and anthropologists working with it in that context are embedded in military units where there are clear chains of command. To what extent can anthropologists judge whether their research will help or harm? And are they allowed to withhold strategic information that they suspect could be harmful to their informants? Furthermore, their safety and security are heavily dependent on the commanders to whom they report, and, as in the cases of both Bhatia and Suveges, they are working in situations where their lives are at constant risk. To what extent can ethical dilemmas be satisfactorily resolved while working in these conditions? If turning over my unedited field notes had been a condition of my release in Bulgaria, what would I have done?

Although AAA’s statement may have been too political in its condemnation of the Iraqi war, it does seem to capture something of the dilemmas of doing anthropology in the context of an international conflict zone, even if the conflict in question is a “just war” (Walzer 2000). The AAA Code of Ethics requires anthropologists to protect their informants, but this may be impossible if the decision to withhold potentially harmful information places the anthropologist at risk of being accused of collaboration with the enemy, or if the anthropologist puts her own safety at risk in order to prevent her research from falling into the wrong hands. In circumstances whereby an anthropologist has no freedom to

make an ethical decision, then it seems that knowingly entering such a situation (such as in the case of the HTS) could be deemed an ethical violation. From this point of view, it seems that the current HTS program should be dissolved and perhaps replaced with some kind of civilian program that ensures freedom of individual ethical choices.

On the other hand, fieldwork is fraught with all sorts of unknown risks, and, as in my case, it may be impossible to know whether you will find yourself in a situation where you have no choice but to cooperate with those who may do harm. In this situation, it seems that a decision to do fieldwork among any marginalized population that might potentially be interesting to your own or their government could also be construed as an ethical violation. If there is even the slightest chance of being detained or harassed by government officials, should a fieldwork project be abandoned? This dilemma appears to lead anthropologists out of the field and back into the ivory tower once again—an unfortunate outcome of AAA's increasingly less flexible Code of Ethics, not only for the discipline of cultural anthropology, but also for the many vulnerable populations that might be helped by ethnographic research (particularly that which is tied to political activism).

It seems that AAA's injunction to "do no harm" ignores the messy reality of most ethnographic research. It is virtually impossible to know with complete certainty whether any study will ultimately help or harm the subject population. In the case of war, the AAA has (controversially) made that decision for us; namely, that any anthropologist collaborating with the HTS is presumed more likely to do harm than good, and given the very real context of war, this is not an unfounded assumption. But what about the rest of us working outside of Iraq and Afghanistan, though also in circumstances where our ability to protect our informants might be compromised?

These controversies are ongoing and as I wrote this article in 2008, the AAA passed a stronger version of its Code of Ethics, reinforcing its imperative to do no harm. Despite this, many of us still have questions that remain unanswered. Although I do not have any ready solutions, it is my hope that feminist anthropologists will participate in the debates more actively, while at the same time encouraging anthropologists not to abandon the field altogether. Yes, there will always be uncomfortable power relations and messy moral dilemmas, but it would be a great shame if this latest chapter of disciplinary introspection created a sort of political and moral paralysis around the impossibility of conducting 100 percent guaranteed "safe" fieldwork. Ethnographic fieldwork will always have risks, and these should certainly be mitigated. These risks, however, are not only to the subject population, but also often to the anthropologist herself, who must navigate through a myriad of personal challenges (and possible dangers) in pursuit of knowledge and intimate cultural understanding. Thus, it should not be forgotten that there are also great rewards, and that sometimes these rewards are worth the risks associated with them. Therefore, rather than

unilaterally determining which risks are not worth the rewards (such as not working with the HTS), it would be far more productive to have an open and honest conversation about how to balance the two, both for the protection of informants and for the preservation of anthropology's long commitment to research in the field.

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Notes

1. One such grant is available through the National Security Education Program (NSEP), founded in 1991 and run by the DOD. Under the NSEP Boren Fellowship Program, PhD students receive support for their research and agree to work for the federal government for a minimum of one year, no later than three years after graduation. The service requirement openly states that NSEP grant recipients must work in "a position with national security responsibilities." According to the NSEP website, "[t]he Departments of Defense, Homeland Security, State, or any element of the Intelligence Community are priority agencies." See the NSEP Web site at www.nsep.gov.

2. An interesting military euphemism, "kinetic operations" are those requiring violent military force; they are contrasted to "information operations," which are a wide range of military activities that generally support the initial kinetic phase of military interventions.

3. See Network of Concerned Anthropologists' Web site at concerned.anthropologists.googlepages.com.

4. In November 2008, a 36-year-old HTS contractor, Paula Loyd, had gasoline hurled at her face and chest while she was visiting a market in ethnic Pashtun territory in Afghanistan. The Afghani man then set her alight and Loyd suffered massive burns over her body. Two months later, on January 7, 2009, she died of her injuries in an army hospital (Constable 2009).

5. A lack of child care prevented my traveling to Washington for this briefing. This article was originally written in 2008, and the book based on my research was finally published in 2009. After this, I decided to pursue a new line of research in Bulgaria,

one not directly involving Islam or Muslim minorities. As of May 2011, I have returned to Bulgaria over five times without incident, and my new project has not attracted interest from any government. I do worry, however, that the difficult moral dilemmas posed by fieldwork among “sensitive populations” have resulted in an unconscious form of self-censorship.

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