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Master Narratives, Retrospective Attribution, and Ritual Pollution in Anthropology's Engagements With the Military

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In this concluding chapter Robert Rubinstein explores some of the disciplinary barriers that anthropologists studying or working for the military have encountered. He applies an anthropological perspective to this analysis to show how these barriers are created and can be overcome. He identifies four processes that create these barriers—the reproduction of master narratives that oversimplify anthropological practice, retrospective attribution, metonymic reduction, and ritual pollution. Rubinstein suggests that anthropologists can neither understand nor change military institutions so long as the discipline does not take the military as an object of serious study or does not treat working responsibly for military institutions as a legitimate professional career path.

In March 2011 Melissa Matthes, a civilian assistant professor at the US Coast Guard Academy, posted the following query to a Listserv focusing on the sociology of Islam: “Who are the Libyan rebels?”¹ Matthes reported, “Within hours a stream of e-mails began—not answering my question, but debating what the relationship should be between the list’s members and the U.S. military. . . . Indignant members of the e-mail list were suspicious that their knowledge, if shared with faculty members at the service academies, would be distorted, appropriated, or in some way used to promote agendas to which

they are not sympathetic. Indeed, some junior scholars worried in offline correspondence that communications with me could jeopardize their careers” (Matthes 2011, B4).

Matthes’s experience is remarkably similar to our experiences in anthropology, like the 2007 incident where anthropologists working with military education institutions were called war criminals during the annual business meeting of the American Anthropological Association (AAA), mentioned in the preface of this volume and discussed more fully in this chapter. She concluded, as did we, that part of the reason for this kind of reaction is a lack of grounded understanding on what military anthropologists actually do. Our desire to begin to fill in this picture led us to assemble the chapters in this volume. All the contributors to this book describe what they do on a day-to-day basis, the personal and professional trajectories that led them to this work, and they comment on some of the challenges and rewards they meet as a result.

Providing this kind of grounded and forthright information about what it is like to work as a professional anthropologist in military settings is instructive on many levels. We hope the chapters will serve as a source of data that will mitigate otherwise free-floating and often erroneous assumptions. The contributions to this book may also show students contemplating anthropological careers something more about what they might expect to encounter if they were to follow working with the military as a professional path.

We are cognizant, however, that the kinds of ideologically driven responses we encountered in 2007 and that Matthes reports (see also McNamara and Rubinstein 2011; Lucas 2009) continue to dominate anthropological discussions of and reactions to the idea of anthropologists working with the military. That this framing of discussions of anthropology and the military persists among anthropologists, who in theory are engaged in exploring deeply and fairly ideas and practices they themselves may not want to do or share, suggests a need for an anthropologically reflexive commentary on why this is so.²

This chapter is a reflection on conversations about whether and how anthropology and anthropologists ought to engage with the military, security, and intelligence communities in the United States. It serves as a commentary on some of the characteristics of these discussions. My observations are based on my own extensive ethnographic work with the military and security community, especially relating to peacekeeping, on my experiences talking about this work within anthropology, and they also derive from published literature. It is useful to highlight at the outset the four main points of this chapter.

1. Master narratives have emerged in anthropology that give license to treating the military and military anthropologists in a totalizing fashion that our discipline would never sanction were they to be applied to other peoples.
2. These master narratives are constructed through a process of retrospective attribution in which the accuracy of a particular narrative account depends on discursive repetition rather than on empirical analysis.
3. These narratives create a kind of metonymic reduction in which particular, often extreme, aspects of some of the empirical phenomena do not just stand for the whole but are used to elide the heterogeneities and complexities that exist in the military and among military anthropologists.
4. Finally, I contend that all these processes lead many anthropologists to see engagement with the military as deeply polluting in ways that Mary Douglas's (1966) work helps render understandable.

I want to emphasize that I recognize there are many voices in this conversation and that they have a wide variety of perspectives and positions. Those I address here seem to me the normatively dominant ones, however. Without taking a position about the ultimate outcome of these conversations, it is my view that for anthropology a discussion of the meaning of engaging the military requires confronting these issues.

Master Narratives in Anthropology

Many of the shared conceptions of their discipline held by anthropologists often turn the just-so stories we tell one another into master narratives that organize, generate, and define the history and purposes of anthropology (Rubinstein 2002). These narratives use "a single voice that does not problematize diversity . . . [and] speaks unconsciously from the presumed center of things" (Star 1999, 384). Many of these master narratives often turn out to be partial or false when they are evaluated in the context of detailed historical accounts. Nevertheless, these narratives underwrite and shape the central conceptions of the discipline by organizing thinking about what anthropology is and what it does. By being reproduced in history texts and in our introductory textbooks they form for contemporary anthropology what Thomas Kuhn (1970, 182) referred to as the "disciplinary matrix." As described by Kuhn, the disciplinary

matrix has four elements: (1) symbolic generalizations, which are the theories or laws propounded by scientific disciplines (182); (2) metaphysical models, which Kuhn describes as beliefs in heuristic models (184); (3) values that include articulating the preferred disciplinary approach (184–85); and (4) exemplars, concrete problem solutions students study in their texts and which teach them how to define and approach problems (186–87).

These narratives provide an uncluttered view of anthropology's history. The same applies in anthropological encounters with the military, security, and intelligence communities. But in so doing they allow for and even encourage thinking that is essentializing and totalizing in a way that most anthropologists would reject when speaking about any other people (for example, see the presentation in Gusterson 2007).

Discussions of the challenges (and dangers) for anthropology in engaging with the military are made by reference to narratives of earlier encounters, and these narratives include similar political shadings. One such episode, the US Department of Defense's efforts to involve social science in the service of its work in Latin America, Project Camelot, plays an outsized role in these anthropological narratives. It is, for instance, referred to by the AAA's Commission on the Engagement of Anthropology with the US Security and Intelligence Communities as a kind of totemic fetish: "scandals like Project Camelot still loom enormously in the collective anthropological memory" (Peacock et al. 2007, 22; see also Wakin 1992), they say. Yet, with a single exception, anthropologists had no role in the project (Horowitz 1967).

How does a complex project, with exceptionally limited anthropological involvement, get converted into an exemplar of anthropology's moral failings? George Lucas (2009) asks this question and answers it by considering it in terms of a broader discussion of ethics in anthropology's encounter with the military. It is also instructive to see this development in anthropological perspective, as part of the ordinary, day-to-day (some would call them mundane or prosaic) sense-making activities of a discipline creating its own image.

I note that one of the master narratives current in contemporary anthropology is that anthropologists work with those "at the margins," following Malinowski's (1954, 92) legacy of giving up the comfortable "chair on the verandah of the missionary compound, Government station, or planter's bungalow." In the context of the discussion of anthropology and the military, this narrative is transformed into an ideological position that anthropologists have a special bond to those affected by state power. Historical analysis shows that the story is much more complicated; indeed, this master narrative transforms a methodological advance into an ideological program (Rubinstein 2011), a

course of action that threatens the integrity of the discipline (see, e.g., Tomforde 2011).

Retrospective Attribution

Writing about the development of scientific disciplines, Bruno Latour (1987) noted that the accepted truth of some claims often depends on what later observers say about them. He calls this process *retrospective attribution* and says: “The status of a statement depends on later statements. It is made more or less of a certainty depending on the next sentence that takes it up; this retrospective attribution is repeated for the next new sentence, which in turn might be made more of a fact or of a fiction by a third and so on” (27–28).

As I noted earlier, the master narratives in anthropology not only establish what is taken as conventionally true but can also introduce an ideological shading to these conversations. We can trace how some accounts get repeated, privileged, and naturalized in anthropology’s disciplinary discourse with careful historical analysis. One can see how claims about a particular work may become detached from the substance of that work. In so doing the later claims present an incomplete or incorrect version of the original, which then becomes part of the received view in a discipline (see, for example, the discussion in Daubenmier 2008).³

Historical analysis can be a corrective. But in the era of Internet publication and blogging, it is also possible to observe the beginnings of this process. The following is an example relating the work of one of the coeditors of this volume, Kerry Fosher. (I choose this particular case mainly because it is so clear, and because it also helps later to illustrate the operation of metonymic reduction and ritual pollution in anthropology’s engagement with the military.)

The University of Chicago Press published Kerry Fosher’s (2009a) ethnographic account of how first responders in Boston construct conceptions of security. The book, *Under Construction: Making Homeland Security at the Local Level*, employs an ethnographic framework to trace and describe the day-to-day activities of this community and interprets them using anthropological theories.

Whether this book is a good ethnography or not is beside the point for my purposes here. What is important are the following seven points of background information:

1. *Under Construction* is a revision and expansion of Fosher’s doctoral dissertation.

2. Her research was funded by the National Science Foundation and a variety of graduate research support grants offered by her university.
3. During her research, Fosher was affiliated with Syracuse University and a research center at Harvard University.
4. Her dissertation received an Outstanding Dissertation Award from the Syracuse University Graduate School. That award was made in open competition and determined through an interdisciplinary selection process.
5. Subsequently, Fosher worked for the New England Center for Emergency Preparedness at Dartmouth Medical School.
6. She then became the first command social scientist at the Marine Corp Intelligence Activity, which is the affiliation she gives in the book.
7. Fosher served on the AAA Commission on the Engagement of Anthropology with the US Security and Intelligence Communities.

With this background in mind, here are some excerpts from a review of this book by Jeremy Keenan (2009b), School of Oriental and African Studies, University of London. It is worth quoting the review at some length, so one can make a fair assessment of its tone and substance.

Keenan writes, “What is a ‘security anthropologist’? I am not sure I know, except that Kerry Fosher, author of *Under Construction*, calls herself one. Fosher is the US Marine Corps’ command social scientist at the Marine Corps Base at Quantico, Virginia. What, one might ask, is a ‘command’ social scientist? One who salutes before interviewing, or lets the interviewee ‘stand easy.’”

In a paragraph largely reworked from the book’s back cover, he goes on to say:

Fosher began an anthropological study of counterterrorism [*sic*] in Boston shortly before the 9/11 attacks. She thus found herself in the unique position of being able to observe the “construction” of “homeland security” in a major US metropolitan area in the immediate aftermath of 9/11, when security became the paramount concern of virtually everyone involved in governing the US. Her study of the development of the homeland security “community” in the Boston area is therefore unique.

But the bulk of his review offers the following:

Imagine my surprise, on checking with the publisher, to learn that the book is an extensive revision, with new fieldwork, of the author's prize-winning dissertation. *While the American military-intelligence security establishment may well afford Fosher's work iconic status* [italics added], there are no such accolades from this reviewer: *Under Construction* is the epitome of all that anthropology should not be.

The fundamental problem with her position, however, is that she seems to think that working in and for the military (albeit in a "corrective" position) can somehow be separated from its larger mission and project.

That larger mission and project is one that has carried the greatest nation on earth (in the eyes of many) through the most shameful period in its history. It is the project of illegal invasion and occupation; of rendition and torture; of Abu Ghraib and Guantanamo; of complete disregard for international law, fundamental human rights and freedoms; of deception, disinformation, lies and dismemberment; of fabricated false flag terrorism incidents; and of killing and bringing suffering to tens of thousands of innocent peoples in the name of the global war on terror and in the cause of US imperialism.

I now turn to look critically at the substance of Keenan's review. There is no reasonable way to connect *Under Construction* with the work Fosher was doing at the time of publication; her dissertation research was carried out and written up before Fosher held the position that identified her in the book. If there is such a connection, rigorous scholarship would demand the link be demonstrated.⁴

Fosher's is not a study in counterterrorism. It is a study of the organization and conceptualization of disaster preparedness by people whose purpose is to aid and assist members of their community when a calamity—human or otherwise—puts them in mortal peril. Conflating emergency preparedness and counterterrorism as equivalents reveals an unconscionable lack of knowledge or a willful act of polemic on Keenan's part. Indeed, it would be like reviewing a book on aspects of life in a village whose members practice Sunni Islam and applying to it the theological and clerical structures of Shia Islam.

Keenan's assertion that Fosher's dissertation (and thus the subsequent book) received iconic status from the military intelligence security establishment is false. It creates the impression that the book and her Boston research is something other than what it was.

Finally, Keenan's long invective concerning U.S. actions and policies calls into question whether he actually read the book he was reviewing. In my opinion, Keenan's review reflects, at best, lazy scholarship by someone who didn't make the effort for honest intellectual engagement.

In a sense Keenan's review is similar to many others in which the reviewer does not seem to apprehend what the book is about, and one should not take it especially seriously. Yet, that is precisely what the process of retrospective attribution depends on. Indeed, that process started almost immediately where Keenan's review is concerned. Returning to the *Times Higher Education* website where Keenan's review first appeared, we find a comment by Maximilian Forte (2009) that reads in part: "The much bigger problem is this, and the book seems to fall to pieces before it was even begun. . . . Such authors distort the conception of power to suit their career goals—they want to suck up to power, while pretending to speak truth to power. If you want to speak truth to power, position yourself in an Afghan wedding party during an American air raid, and then tell me about how power is managed and exercised. Thanks for the timely and well written warning about this book."

Although Keenan's presentation of the book he reviewed is largely detached from the substance of the book, the commentator accepts Keenan's inaccurate account without bothering to consult the text itself. Indeed, one can sense in the comment relief at having escaped the onus of actually looking at the book and making an informed judgment. We are witnessing the process of retrospective attribution in the making.

It might be objected that I am making too much of this review. It may be that Keenan's review is simply one of too many such examples of shoddy scholarship, written by an ideologue. Indeed that is one possibility. Even should that be the case, the process is still an invidious one that has damaging effects on anthropology as a discipline and on individual anthropologists. Let me illustrate.

Metonymic Reduction

To show why the process of retrospective attribution is so damaging, I return to the 2007 AAA annual meeting for a fuller account of the event that was one of the motivations for this book. Rubinstein and Fosher participated in

the panel “The Empire Speaks Back: U.S. Military and Intelligence Organizations’ Perspectives on Engagement with Anthropology” (Glenn 2007). I spoke about my work studying peacekeeping and how the obligations of reciprocity established with my informants led me to work with some of them to improve their practice in peacekeeping operations. I reported on my efforts in working with the United Nations, the US Army Peacekeeping Institute, the US Army War Colleges, the Joint Forces Staff College, predeployment training in negotiation at Fort Drum, and about a variety of encounters I had in classrooms and lecture halls. Others on the panel spoke about their efforts to introduce cultural understanding into the curricula of various professional military education settings.

The following day at the annual business meeting, a person rose from the floor to speak passionately against anthropology’s engagement with the military. He said, “Yesterday there was a panel called ‘The Empire Speaks Back.’ Everyone on that panel is a war criminal, and should be banned from the association.” (This quote is reconstructed, but the use of the term *war criminal* and the proposed banning of the panel participants are the speaker’s own words.) There was applause at this remark. Charging anyone with being a war criminal, and proposing sanctions as a result, is a serious act. Yet no objections to this comment were made from the floor or from those presiding over the meeting.

When I raised the remark with colleagues after the meetings, the replies I got had two troubling themes.⁵ The first, akin to the possible dismissing of the Keenan (2009b) review as simply poor scholarship that slipped by, was that the remark had been made by someone whom no one took seriously; “Oh, that was crazy old Harry; everybody knows he’s way out there.”⁶ I for one did not know this, and so presumably neither did others in the room, the comment had been made publicly in a professional meeting attended by several hundred of our professional colleagues, and the meeting was public and open to the press. The second most common response was an equally troubling studied ignorance of the remark: “Oh, did someone say that? I didn’t hear it; I must have been in a side conversation at the time.”

After the meetings I learned of a conversation between two colleagues in which an interesting double standard emerged. The first colleague offered that the person making the war criminal comment had been behaving like a fascist. To this the second colleague quickly responded that we must be very careful about the accusations we make and the words we use; calling the speaker a fascist was going too far. Yet when then asked about the war criminal charge, the second colleague demurred, saying he would have to know more about it.

It is worth noting that none of the anthropologists on the panel had been involved in any kind of military operational role, nor had any of them supported combat operations. Rather they were speaking about acting as anthropologists educating the communities with which they work on questions of culture or conducting research on them. Some reported that in this context they had raised tough questions about how the military organizations they work with or study act. Nonetheless, “Harry’s” remark glossed their work to a subset of military activity. At a minimum it creates a professional environment that is hostile and unwelcoming to the people involved in this work. It can also mark as out-of-bounds whole areas of investigation.

We know from studies of human cognition that people often think by analogy and employ conceptual metaphors to help them understand one domain of social life in terms of others they have experienced (Lakoff and Johnson 2003; Rubinstein, Laughlin, and McManus 1984). One of the strategies people use to do this is metonymy, where a part stands for the whole. Metonymy can be an expansive cognitive act that can structure a systematic and broader understanding of the target area of social life (Lakoff and Johnson 2003, 35–40).

In contrast, metonymy can also be used to reduce and misleadingly simplify the phenomenon under consideration. When a subset of the activity is taken as the whole (not just an avenue of cognitive access to the broader whole), or taken to be a much more substantial part of the activity than it actually is, the result limits rather than expands opportunities for investigation. Doing this leaves things out. This kind of elision is a metonymic reduction. Keenan’s (2009b) review and “Harry’s” accusations are examples of metonymic reduction.

One effect of these processes is that they put military anthropologists in a double bind. If on the one hand, they accede to the insistence that to be taken seriously they must speak about controversial programs like the Human Terrain System (HTS), then they find themselves constantly repeating their views on HTS and not being able to move to discuss the ethnographic data and conceptual materials they would like to address. In doing so, they reinforce the metonymic reduction and may be scolded for not contributing anything new ethnographically, which is then taken as evidence of the bankruptcy of their endeavors. On the other hand, if military anthropologists offer analyses of the things they find interesting and important and do not take HTS as their concern, they are accused of not participating in disciplinary discourses.

Ritual Pollution

Some time ago, I wrote, “Because of the suspicion with which anthropologists view the defense community, researchers risk being stigmatized because of those exchanges” (Rubinstein 2003, 24). Why should researchers working on aspects of a prominent set of institutions in our society fear that doing so will lead to their being ostracized from their discipline? It is not hard to see that the kinds sentiments expressed by the Keenan (2009b) review, Forte’s (2009) response to it, and “Harry’s” call give force to those concerns, especially since they are not exceptions, as Matthes reports.

There is, though, a more anthropological way to account for this. Keenan’s (2009b) review and “Harry’s” call are examples of people tending the boundaries of the discipline. Kerry Foshier (2009b) points out that “boundary tending is a legitimate exercise. However, the location of these boundaries and their permeability should be the subject of informed debate rather than polemics.” In contrast, in their statements Keenan (2009b) and Forte (2009) articulate a conception of what anthropology ought to be and what it ought not be according to a particular ideological and polemic conception of the world. Both see the retrospectively, metonymically reduced acts of military anthropologists as profoundly threatening to the proper order of things anthropological. In short, they reinforce the master narratives they emerge from. And they are not alone.

Shortly after Seymour Hersh (2004) wrote in *The New Yorker* about the central role that Raphael Patai’s *The Arab Mind* played in the Abu Ghraib torture, Laura McNamara was moved to ask an empirical question: what evidence was there that *The Arab Mind*, or anthropological literature more broadly, or individual anthropologists actually played a role in that episode? After a thorough search through thousands of pages of material obtained through the Freedom of Information Act, she found no evidence for this claim. Indeed, all paths led back to Hersh’s original article (McNamara 2011). Yet her careful and transparent efforts were dismissed by the editor of *Anthropology Today*, based on nothing more than her affiliation—McNamara works for Sandia National Laboratories. In a move resonant with Keenan’s (2009b) dismissal of Foshier’s work because of her affiliation with the Marine Corps, he wrote:

But why wait half a century before we can debate what McNamara and her secretive colleagues are up to *today*? Sandia National Laboratories, her employer, is managed and operated through a wholly

owned subsidiary of Lockheed Martin Corporation. . . . As a contractor responsible for military interrogation, Lockheed Martin was deeply involved in the torture and prisoner abuse scandals at Bagram and Abu Ghraib. . . . Two former presidents of her organization went on to work for Lockheed Martin, and one of the four current deputy directors also serves on a Lockheed Martin venture. One prominent advisor to her lab is separately associated with a different company, CACI International, at the heart of this scandal. . . . Given the close relationship between her laboratory and companies implicated in torture, can we fully trust her claim that anthropologists have no involvement in such practices? (Houtman 2007, 21)

One might ask on what basis do we trust any of the ethnographic or research reports of our colleagues? Anthropologists are notoriously secretive in their treatment of field notes. Indeed, the act of making our data available to other colleagues is the exception in anthropology rather than the rule.

Part of being human is to create categories that help regulate how we live, how we ought to behave, and separate what is good and desirable from what is not (Zerubavel 1993). In this context I am struck by the sense in Keenan's (2009b), "Harry's," and Houtman's (2007) comments that association with military anthropologists is profoundly polluting, and that there should be prohibitions against anthropologists engaging with the military in this way. Surely Mary Douglas (1966) would have recognized this process of articulating prohibitions as akin to her analyses of taboo and ritual impurity. I suspect that she would have seen in their comments both themes that emerged from her study. The prohibitions on anthropological engagement with the military that emerge from Keenan's, "Harry's," Houtman's, and others' similar comments serve as "device[s] for protecting the distinctive categories of the [anthropological] universe" and also provide a way of limiting "the cognitive discomfort cause by ambiguity" (Douglas 1966, xi).

Further, their discourse presumes a kind of moral purity, setting them apart and above the military anthropologists they critique. Laura McNamara suggests there is a presumption that particular locations in academia accord a kind of purity. I have noted elsewhere that many of these critics work for state institutions and conduct their research using state money and that they pay their taxes, thus implicating them in support of the structures they abhor and scold others for supporting. In this connection, it is worth noting there is considerable, though unintended, irony in Keenan's (2009b) savaging of Foshier

for her institutional affiliations, as he is institutionally situated in the School of Oriental and African Studies, created and supported by British colonialism. Further complicating Keenan's own moral position, and by his own logic equally contaminating, he asserted during his talk at Syracuse University,

I sit on the Foreign Office Committee for Intelligence, I also brief the State Department, I brief the Pentagon, so I actually know what these guys are talking about. . . . But, just so you know why I get there, there's a group in the State Department, Pentagon, who will not have briefings unless I'm invited. So you have this strange ritual every six months where I get an air ticket and I'm flown over to Washington." (Keenan 2009a)

Conclusion

Military, intelligence, and security institutions and the people who participate in them are prominent parts of our society. Those institutions are a way of providing defense for the state and for its citizens. There are many examples of the military carrying out policies set by the civilian leadership in the United States that are objectionable and doing so in ways that are shameful. Yet it seems unreasonable to me to think that anthropology is better off not dealing with the military. If we don't study the military ethnographically we will not understand it as a human institution, and we cannot affect change in those institutions without interacting with the people in them just as we would any other group. Likewise, treating anthropological colleagues who interact with those institutions and individuals as a priori polluting limits severely the value of what we can learn and what we can do to affect changes in policy and actions.

The creation of master narratives about the military and about military anthropologists through the process of retrospective attribution coupled with the metonymic reduction of those activities to the most extreme and complicit forms of engagement results in the construction of stereotypes that are dangerous for the military anthropologists. Those stereotypes energize notions of anthropological impurity that are dangerous for our discipline as well, since it denies us the nuanced and empirical information about major, powerful institutions we need if we wish to control rather than be controlled by the encounter with the military.

Lest I be guilty of the kind of totalizing and essentializing I critique in this chapter, I repeat what I said earlier. Within anthropology there are people who

contribute serious, empirically based, ethnographically rich, and wide-ranging analyses while offering critical assessments of anthropology's engagement with the military. Examples of these voices include Roberto González's (2009) work on American counterinsurgency, David Price's (2008) historical analyses, Keith Brown's (Brown and Lutz 2007) work on grunt literature, and Matthew Guttman and Catherine Lutz's (2010) work on veterans of the Iraq war.

The danger for anthropology is that those who seek to make grounded, empirically informed evaluations of the merits and dangers of anthropology's engagement with the military will be shouted down by those who wish to reduce that encounter to an ideological duel.

Part of the force of culture comes from activities taking place outside our daily consciousness. When we analyze these processes and make their content evident, we can diminish their power and make alternative action possible (Foster 1990). We hope that exposing the processes I identify in this conclusion for further analysis and combining them with the ethnographically rich accounts offered by the contributors to this book will help create the context in which military anthropology can become a field of reasoned discussion and, for those whom it suits, a professionally legitimate career path.

Notes

1. An earlier version of some of the material in this chapter was presented in "Scholars, Security and Citizenship 2," (plenary session, Annual Meeting of the Society for Applied Anthropology, Santa Fe, NM, March 19 2009). I thank Kerry Fosher, Clementine Fujimura, Sandra D. Lane, and Laura McNamara for comments on this chapter.

2. I thank Jim Lance for this particular formulation of the issue.

3. Some might fault writers for relying on others' representations rather than consulting the original work. There are, however, pragmatic reasons that make such reliance on colleagues' claims necessary and even desirable. This reliance is very much a problem when representations are subject to ideological shading.

4. Another error in Keenan's review is this: after noting that Fosher is an associate of the Syracuse University Institute for National Security and Counterterrorism (INSCT; again not pertinent to the substance of *Under Construction*), Keenan says, "For readers not familiar with INSCT, check the website. After the martial music, its introductory video (www.exed.maxwell.syr.edu/exed/sites/nss) tells us that its national security studies program is open to civilian and military leaders. Yet 100 per cent of the alumni mentioned in its latest alumni newsletter hold senior positions in the US Defence Department and associated intelligence services." Even a cursory effort reveals that the website he directs his readers to is not the INSCT website at all.

Despite Keenan's swagger in this review, during Keenan's visit to speak at the Maxwell School of Syracuse University, which was attended by the INSCT director, Keenan made no mention of his objections to the institute's work.

5. Although I was in the audience, I too did not object even though I was stunned and offended by this speech. The context in which the remark provoked applause and the fact that I was among those so charged would have made voicing an objection an uncomfortably self-interested act. Perhaps others who thought to object were constrained in similar ways.

6. "Harry" is a pseudonym.