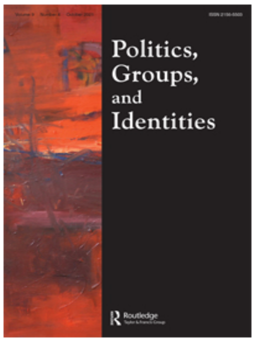

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Sources of evidence and openness in field-intensive research on violent conflict

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ABSTRACT

This article engages with Steven Lubet's arguments in *Interrogating Ethnography* on reliability of evidence and replication of findings in ethnographic research. It draws on eight months of immersive fieldwork on Abkhaz mobilization in the Georgian-Abkhaz war of 1992–1993 to show that field-intensive researchers who work on sensitive political topics leverage multiple sources to develop their insights and engage in reflexivity while prioritizing the safety of their research participants. It is these practices that underlie the trustworthiness of research and form the basis for the evaluation of research results rather than verification standards proposed by Lubet that do not, and cannot, apply to this kind of research.

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Ethnographic; political knowledge; methods; political violence

Interrogating Ethnography: Why Evidence Matters, Steven Lubet (2018, 7) writes, is "an extended essay on the nature of proof in qualitative social science, and the use of trial techniques to test, and thereby strengthen, ethnographic studies." These techniques include "fact- and citation-checking, revisits and re-interviews, and examination of field notes" and would allow for replication and verification of ethnographers' accounts (128).

I address two aspects of Lubet's argument: first, the proposed hierarchy of evidence in ethnography and respective reliability of sources; and, second, the call for replication and verification of results, particularly through data sharing. I draw on eight months of immersive fieldwork on Abkhaz mobilization in the Georgian-Abkhaz war of 1992–1993 to illustrate my arguments.

While Lubet establishes a hierarchy of evidence from documentation to first-hand observations and second-hand accounts, I argue that studies of complex political processes, such as mobilization, require leveraging multiple sources of evidence. The combination of in-depth interviewing and participation in present-day social interactions, together with analysis of other researchers' interview archives, official documents, and media transcripts, and extended reflection on how my and my research participants' emotions shaped the process of research, all provided me with an understanding of Abkhaz mobilization in its broader socio-structural context, both at the time of the

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war and during my research. The resulting richly textured account would not have been possible had I focused on one source of evidence or prioritized some sources (e.g., documentation) over others (e.g., second-hand accounts), as Lubet suggests.

Leveraging different kinds of sources helped me increase the confidence in my interpretation of the mobilization process, and my reflexivity on how research materials were produced and analyzed enabled what Büthe and Jacobs (2015, 57) call "replication-in-thought" by providing sufficient information for a reader to trace my interpretations. This stands in contrast to the notions of replication and verification through fact-checking, re-interviews, and reviews of data from field research, as proposed in *Interrogating Ethnography*. These practices would have been neither desirable nor possible in the challenging conditions of my field research, which was characterized by the isolation of the *de facto* Abkhaz state, ongoing violence in the Georgian-Abkhaz border area, and surveillance. Under such conditions, sharing the data with other researchers, journal editors, reviewers, or the public could have compromised the safety of research participants. Moreover, no other researcher could replicate my immersive experience as the particular constellation of research conditions at the time of my research, my own background, and the knowledge of the case that I had developed could not be repeated.

From hierarchy of evidence to leveraging multiple sources

According to Lubet (2018, 21), ethnographic data falls into three broad categories: researchers' observations, informants' accounts, and "rumors, folklore, and popular beliefs." Lubet argues that problems of memory and bias make all three categories unreliable, but that second-hand reports are particularly prone to these problems. Therefore, Lubet prioritizes first-hand observations in ethnography, "not least because they may sometimes be fact-checked" (13), and he calls for greater use of documentary evidence, which is "frozen in time, unlike fragile human memories," and "inherently reliable" (23).

This establishes a hierarchy of evidence from documentation to first-hand observations and second-hand accounts. Elevating documents over lived experience misunderstands the underlying "sensibility" of ethnography as "an approach that cares … to glean the meanings that the people under study attribute to their social and political reality" (Schatz 2009, 5). Evidence in this kind of research comes "in a variety of forms, with no single form pre-judged as superior to another" (Schwartz-Shea and Yanow 2012, 78).

In my research, I relied on multiple tools of inquiry to grasp the meanings Abkhaz men and women attributed to their participation in the Georgian-Abkhaz war of 1992–1993. Semi-structured life history interviews with 150 participants and non-participants in the war helped me understand how everyday social interactions and participation in collective action before the war shaped what I call "collective conflict identities," or shared understandings of conflict and one's role in it (Shesterinina 2021, 2). These shared views were then invoked across the social structures in which potential participants were embedded to enable individuals to make sense of the violence in the uncertainty of the war's onset. As in other interview-based studies, this body of evidence allowed me to "pursue the meanings of specific statements by locating them within a broader web of narratives, explanations, telling omissions, and nonverbal cues" (Soss 2006, 128). Observation of war-related events, such as war medal awards and celebrations of the anniversary of the war, meetings of mothers' and veterans' organizations, and informal gatherings and celebrations went hand-in-hand with the interviews and helped me identify inconsistencies in what I was told, develop informed follow-up interview probes, and situate respondents in their present-day social settings.

These latter observations are what Fujii (2010, 232) called "meta-data": "the spoken and unspoken expressions about people's interior thoughts and feelings, which they do not always articulate in their stories or responses to interview questions." Lubet (2018, 12) views "emotional involvement" as a source of researchers' bias, but ethnographers have drawn on their own and their research participants' emotions as sources of data. My emotional responses to participants in my interviews pointed to how my and my participants' empathies and fears during the fieldwork shaped what questions I asked, what respondents told me, and how I interpreted the interviews (Shesterinina 2019). Reflecting on the emotional dynamics of the research during and after the fieldwork helped me understand why some respondents reproduced dominant conflict narratives during the interviews, whereas others transcended the official line and were sometimes critical of the war effort and the post-war, *de facto* Abkhaz state.

Instead of diminishing the significance of the interviews due to the fading of memory and bias, as Lubet suggests, I followed the practices of other researchers of war-time mobilization (Wood 2003). I focused on research participants' memory and addressed how the war processes, post-war affiliations, and the retelling of war stories in post-war life shaped those memories. That was the point of the research – not to establish a factual account of events that would be verifiable, let alone replicable, but rather to grasp how participants made sense of and acted upon these events. This required a more complex combination of methods than what Lubet calls for. I interviewed individuals across a broad range of pre-war, war, and post-war backgrounds to understand whether and how different loyalties across the stages of conflict affected what people told me, asked event and narrative questions to compare sequences of actions from respondents' recollections with their views of the conflict, and cross-checked responses within and across my interviews and informal conversations. Triangulation of these accounts with other researchers' interviews, often with the same research participants, combined with 30 additional interviews with witnesses and experts in Georgia and Russia, extensive analysis of official documents, newspaper reports and video footage, and secondary literature, increased my confidence in these materials – because multiple sources of data helped me contextualize and check what I heard, saw, and read in one source against others.

Moreover, the analysis of multiple sources of evidence allowed me to understand how evidence was generated – a process that shapes data quality (Herrera and Kapur 2007; Cheesman 2021), but that Lubet (2018, 26) brushes aside, particularly when it comes to official documents. Each of those sources focused on a distinct aspect of the phenomenon, something that Davenport and Ball (2002) also found in their analysis of source selection in the case of Guatemalan State Terror of 1977–1995. The interviews I conducted provided detailed accounts of individual mobilization trajectories in the context of the conflict in Abkhazia; state documents presented the events described by research participants from the perspective of the Soviet state; news and videos demonstrated how official narratives were transmitted across the society; and secondary sources helped substantiate these materials. The different perspectives that each source brought to the analysis enabled me to arrive at a nuanced interpretation of Abkhaz mobilization which otherwise would not have been possible. This suggests the need to move away from the evaluative hierarchy of evidence proposed by Lubet to a focus on how researchers leverage multiple sources to gain a deep understanding of complex political processes in ethnographic research.

From replication and verification through data sharing to "reflexive openness"

To evaluate evidence, Lubet (2018) calls for replication and verification. Field notes, he says, "typically include confidences that cannot easily be anonymized … [which] inevitably impedes verification" (132). Therefore, Lubet suggests, "[e]thnographers should routinely fact-check one another's work" (137) and "review ethnography field notes" (134). This, he argues, would ensure that research findings are supported by the notes. Similar calls lie at the heart of ongoing debates about research openness in political science (Jacobs et al. 2021).

Field-intensive research on sensitive political topics, in general, and on violent conflict, in particular, presents serious challenges to the calls for replication and verification through data sharing. First, revisits and reviews of field notes and interview transcripts cannot "replicate" the immersive experience that informs research outcomes. For example, a revisit of my research site or review of my interview transcripts or field notes would not necessarily enable another researcher to feel the empathy and fear that played a crucial role in helping me understand the context of violence in post-war Abkhazia. "This is because the research is based not just on what people say," Tripp (2018, 730) explains, "but also on the context, the individual's relationship with their research subjects, and the multitude of ethnographic impressions that are accumulated in the process." From this perspective, "there is no prior non-relational, non-interpretive moment of raw information or data to reference back to" (Pachirat 2015, 30). As Cramer (2015, 20) notes, "The raw data exist in the act of spending time with and listening to people. That cannot be archived" – or shared, or replicated by another researcher.

Second, sharing "raw" data in the form of field notes or interview transcripts would not help another researcher, editor, or reviewer "verify" the broad range of evidence, knowledge, and experience that informs research results. More importantly, data sharing could generate significant harm to research participants, individuals mentioned in interviews, research assistants, and even the researcher herself, especially in contexts of violent conflict. In such contexts, data "should almost always *not* be made accessible for ethical reasons" (Parkinson and Wood 2015, 24). Even de-identified field notes and interview transcripts could allow an informed observer to decipher individuals and locations in the research and result in local retaliation and state investigations that could subject those involved in the research, including the researcher, to imprisonment, torture, and even death. Sharing notes and other materials, making them public through an archive or a digital repository, could also break promises of confidentiality, which would undermine the informed consent given by research participants and the trust established between researcher and research participants that field research depends on.

Lubet (2018, 108) sees these practices as an "extreme commitment to anonymization," and he suggests relaxing it by, for example, "citing the dates and locations of interviews and observations" (135), which could be readily traced back in the field notes. Yet in sensitive political contexts where political circumstances change, often unexpectedly, such explicit information could have unintended consequences, endangering participants (Fujii 2012). As a result, the requirement of data sharing or of making these kinds of details public knowledge could prevent future work on sensitive political topics.

Research results in ethnography and other forms of qualitative studies can be evaluated in other ways, not only through replication and verification through data sharing. For instance, a researcher can provide "sufficient information to allow readers to trace the reasoning and analytic steps leading from observation to conclusions," facilitating "replication-in-thought" (Büthe and Jacobs 2015, 57). This involves what MacLean et al. (2019, 1) call "reflexive openness," or "sustained reflection on ethical research practices," which means engaging in ongoing reflexivity and providing reasoned justification of these practices.

I could not share interview transcripts or field notes, as Lubet suggests, because that would have endangered the security of my research participants and breached our agreed-upon confidentiality. Even if anonymized, publicly available field materials would have potentially compromised the safety of my research participants. An informed local observer could have parsed their identities given the relatively small size of Abkhazia and the high density of its social networks. This potential identification of research participants would be detrimental not only to the respondents who participated in the war, but also to those who fled from the violence. In the sensitive political environment complicated by the Russian presence and by tensions between Georgia and Russia, discussing the war with an international researcher could result in scrutiny – and that scrutiny could well extend to a researcher's archives and digital repositories. Making my data publicly available, therefore, could have jeopardized research participants' trust in my ability to protect what they shared with me in an ongoing way, as well as my own future security as a researcher who might want to return to do follow-up research in the area.

In order to facilitate the evaluation of my research, as an alternative to making my interview transcripts and field notes available for review or uploading them to a digital repository, I adopted various transparency practices, including providing extended interview excerpts and a detailed description of my data production and analysis processes in both published texts and methodological appendices (2021). Such extended excerpts provide contextualized data for a reader to evaluate, while protecting the identities of research participants. I also discussed the procedures involved in selecting the case and sites within Abkhazia, recruiting research participants, conducting and analyzing my interviews, including my coding strategy and how I addressed competing explanations, and generating other sources of evidence. This discussion offers a reader sufficient information to trace my *analytic* steps from observation to conclusions, which sharing data for replication and verification, as proposed in *Interrogating Ethnography*, even if possible, would not provide.

Conclusion

In the opening paragraphs of the book, Lubet (2018, 1) says that

> the ethnographer must earn the trust of the reader. Because ethnographic results cannot readily be replicated, we must ordinarily depend on the researcher's word that the sample

is sufficiently representative to tell us something meaningful about the social context and the larger population.

What I argue here is that field-intensive researchers in political science who work on sensitive political topics, such as violent conflict, can achieve the goal of trustworthiness through other means than what Lubet stipulates: leveraging multiple sources of evidence to develop in-depth understanding and to arrive at their conclusions, and engaging in a range of transparency practices to facilitate evaluation of their research while prioritizing the safety of their research participants.

The example of research on violent conflict raises important questions about "'who decides' whether data and/or details of its production should be withheld to protect human subjects" in field-intensive research in political science, more generally (MacLean et al. 2019, 15). While reflexive openness on data production and analysis in my research has been critical for my ability to provide readers with the means for evaluating my research results, these practices cannot be generically applied to other studies (Kapiszewski and Wood 2021). For example, in contexts where describing the research process is as detrimental for the safety of research participants as data sharing is, extreme care should be taken in discussing aspects of data production and analysis. It is up to researchers to decide, based on their "ethnographic sensibility," what dangers different forms of data sharing and discussion of data production and analysis processes pose to research participants (Schatz 2009, 5). The deep knowledge they develop through immersion in the contexts of their research and their openness concerning the choices they make from research design to publication should be what researchers draw on in conducting ethnographic research, more than conforming to standards of replication and verification that do not, and cannot, apply to this kind of research.

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