

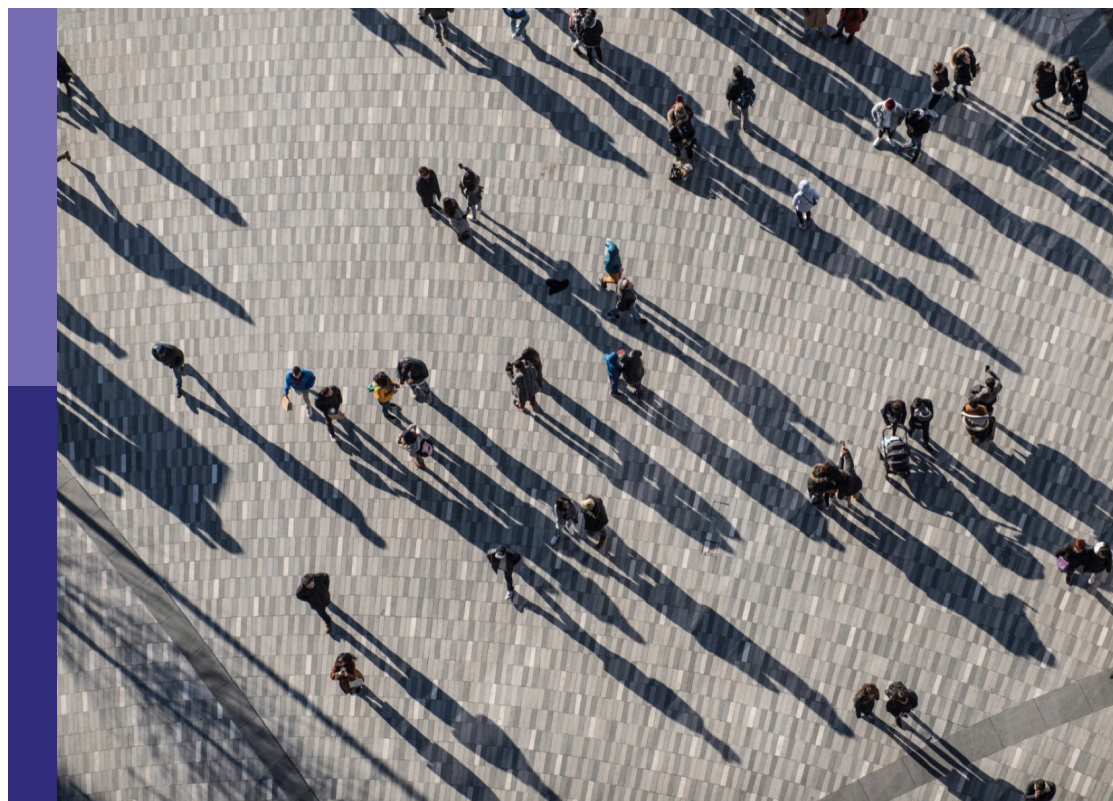
Ethnography in the open science and digital age: New debates, dilemmas, and issues

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Published in

Frontiers in Sociology



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ISSN 1664-8714
ISBN 978-2-8325-4680-2
DOI 10.3389/978-2-8325-4680-2

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Ethnography in the open science and digital age: New debates, dilemmas, and issues

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Citation

Jerolmack, C., Murphy, A., Reyes, V., eds. (2024). *Ethnography in the open science and digital age: New debates, dilemmas, and issues*. Lausanne: Frontiers Media SA. doi: 10.3389/978-2-8325-4680-2

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RECEIVED 26 February 2024
ACCEPTED 05 March 2024
PUBLISHED 15 March 2024

CITATION
Jerolmack C, Murphy AK and Reyes V (2024)
Editorial: Ethnography in the open science
and digital age: new debates, dilemmas,
and issues. *Front. Sociol.* 9:1392012.
doi: 10.3389/fsoc.2024.1392012

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Editorial: Ethnography in the open science and digital age: new debates, dilemmas, and issues

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KEYWORDS

ethnography, data transparency, open science, digital ethnography, research methods, reflexivity

Editorial on the Research Topic

[Ethnography in the open science and digital age: new debates, dilemmas, and issues](#)

Ethnographers—and qualitative researchers more broadly—arguably face unprecedented challenges in carrying out their work today. Academic gatekeepers are increasingly demanding that fieldnotes, interview transcripts, and other data be shared in the name of “open science.” Popular ethnographies have had their scholarly rigor impugned by journalists and the public. Scrutiny has also come from within: As practitioners of the craft of ethnography come from more diverse backgrounds, some question longstanding conventions of writing, representation, and ethics. What’s more, the digital and surveillance age poses novel challenges to how ethnographers study social life and protect the privacy of their participants.

While the “replication crisis” in social science has catalyzed a movement for transparency (e.g., registering hypotheses in advance; sharing data), it is not clear that positivistic standards of verification translate to the interpretive enterprise of ethnography (Jerolmack, 2023). Some, like Lubet (2018), contend that ethnographers must name sources, fact-check, and perhaps even share raw data to secure readers’ trust and facilitate falsifiability; it has also been suggested that ethnographers who spurn open science may be left behind as funders and academic journals increasingly require data transparency (Jerolmack and Murphy, 2019). Others, like Burawoy (2017, p. 269), worry that the fetishization of “factual details” conveys a “false sense of objectivity” that elides a reflexive reckoning with how our interpretations are shaped by our social position in the field; moreover, some (e.g., Reyes, 2018; Stuart, 2020) warn that demands for open science may further marginalize scholars who study vulnerable populations (where data transparency is dangerous) or who lack the resources that facilitate data transparency (e.g., hiring fact checkers). Between these poles, others have floated flexible “standards for transparency that are consistent” with ethnographers’ “commitment to their subjects and interpretive scholarship” (Murphy et al., 2021, p. 41)—e.g., partial disclosure of people or places, sharing the coding scheme, or online appendices with supplementary data

(Lee, 2016; Tsai et al., 2016; Reyes, 2018; Contreras, 2019)—and suggested criteria for evaluating scholarly rigor attuned to the *verstehen* spirit of qualitative methods (Small and Calarco, 2022).

As social life is increasingly lived online, it becomes unclear where the boundaries of the “field site” should be drawn and whether ethnographic conventions—methodological *and* ethical—are directly transferrable to the study of digital spaces (Lane, 2018; Stuart, 2020). Yet many contemporary ethnographies still read almost as if they were set in the prior millennium, barely acknowledging, much less theorizing, how much people have folded smart phones, social media, online gaming, virtual reality, and AI into their lives. As more researchers venture into digital spaces, they force us to grapple with questions like whether an online platform is a “community”—or even a “place”—and whether exchanging DMs or commenting on someone’s post “counts” as ethnography.

With a growing chorus of social critics calling ethnography “extractive” and demanding that it be “decolonized,” also at issue is whom has license to write about whom, and what we owe our research participants (Rios, 2015; Miller, 2021). Relatedly, feminist ethnographers are calling for open, critical discussions about the embodied dimensions of fieldwork (a historically androcentric enterprise), including not only emotions but also issues like sexual intimacy and harassment (Hoang, 2015; Hanson and Richards, 2019; Reyes, 2020).

There can be no “one size fits all” answer to these developments and debates. This Research Topic therefore embraces a pluralistic view, curating a collection of methodological reflections that represent varying—even conflicting—perspectives on how ethnographers are engaging (or should engage) with the three pressing issues intimated above: the movement for open science; the migration of social life into digital spaces; and the moment of reckoning with the racialized and gendered history of fieldwork and knowledge production.

On the question of how ethnographers should respond to open science, two articles reject blanket demands for data transparency and question its value. Khan et al. contend that the college students whose sexual practices they studied would be less likely to disclose personal details, and that so much information would have to be masked to maintain confidentiality that the remaining data would be meaningless. Pugh and Mosseri contend that reflexivity is a better path to scholarly credibility and reliability than data transparency, and that unmasking participants’ identities would pressure them into inauthentic performances of “narrative and emotional coherence.” (However, we note that one exemplar of “excavating ambivalence, plurality and complexity” *does* name—see Duneier, 1999). Taking a more meta critical approach, Goldensher makes the case for ethnographically studying open science as a contested field where gatekeepers (journal editors, grantors) privilege and legitimize certain forms of knowledge. Enriquez, a practitioner of open science, uses her experience making interviews with gig workers publicly available online to illustrate the kinds of ethical and practical issues involved with data sharing. As a journalist slightly removed from the open science debates, Conover puzzles over some of ethnography’s conventions around confidentiality and data verification while appreciating that ethnographers have different commitments and face different pressures than journalists.

Regarding the study of digital spaces, two articles provide practical takeaways from observing the online world of adherents to the far-right conspiracy theory QAnon. By comparing Forberg’s “digital ethnography” of QAnon to Schilt’s “analog ethnography” of a different group, the authors conclude that the two modalities “share a common epistemology” and that the former can be as “thick” as the latter if the researcher commits to reflexive immersion (rather than just lurking). Regarding ethics, Cera argues that not all social media data should be treated as public and explores how to protect privacy while still making raw data accessible. The article by Owens unpacks the problem, exacerbated in online research, of how to deal with subjects who deceive us about their identity, experiences, or relationship to the field of research.

Becker (1967) long ago urged ethnographers to discard the myth of value neutrality. This imperative has taken on heightened urgency given the resurgence of nativism and racism. Ince rejects a “spectatorship” orientation to fieldwork in favor of the ethnographer as what James Baldwin called the *witness*, which requires “using one’s status position to publicly unveil” structural inequality and advocate for change. In turn, Su and Su offer an *inward* perspective on reflexivity and the project of challenging social marginalization. The sisters reflect on how they responded to a shared experience of being sexually harassed in the field and suggest that we consider how such traumatic episodes shape the way we interpret the field—and ourselves.

The articles herein grapple with some of the most important dilemmas facing ethnographers today. These issues demand our scholarly attention, and the range of perspectives brought to bear upon them by the Research Topic’s authors promise to bolster the craft of qualitative inquiry.

Author contributions

CJ: Writing – review & editing, Writing – original draft. AM: Writing – review & editing. VR: Writing – review & editing.

Funding

The author(s) declare that no financial support was received for the research, authorship, and/or publication of this article.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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SPECIALTY SECTION

This article was submitted to
Sociological Theory,
a section of the journal
Frontiers in Sociology

RECEIVED 13 October 2022

ACCEPTED 30 March 2023

PUBLISHED 24 April 2023

CITATION

Pugh AJ and Mosseri S (2023) Trust-building vs.
“just trust me”: reflexivity and resonance in
ethnography. *Front. Sociol.* 8:1069305.
doi: 10.3389/fsoc.2023.1069305

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Trust-building vs. “just trust me”: reflexivity and resonance in ethnography

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Amidst a perceived credibility crisis, recent scholarship has challenged basic norms of how ethnographies are conducted. This article identifies, underlying these critiques, a “trust me” fallacy that misunderstands ethnography as requiring blind trust in the researcher, leading to proposed reforms that promote extractive research practices by treating truths as raw commodities to be traded in for credibility. We argue such practices are unlikely to resolve critics’ concerns, and at the same time, they challenge the ethnographic capacity for resonance. Building on recent work in cultural sociology, we elaborate and refine a “textured model of resonance” to capture one of ethnography’s unique contributions: excavating ambivalence, plurality and complexity. We conclude by noting how time-honored practices of reflexivity, honed through productive dialogue among practitioners, address issues of trust and reliability without threatening what ethnography does well.

KEYWORDS

resonance, reflexivity, ethnographic accountability, culture, trust

Introduction

A flurry of scholarship has arisen around contemporary practices of ethnography, suggesting reforms designed to address a perceived credibility crisis. Yet we argue that most of these proposals reflect a core misunderstanding of what is valuable about ethnography, particularly its potential for resonance. This article makes two core contributions: first, we identify a “trust me” fallacy, through which challengers read ethnographers as enjoying undue benefits of trust, a fallacy underlying current critiques of ethnographic research. The “trust me” fallacy misses how credibility is painstakingly layered into historically grounded ethnographic praxis and is also highly visible within the text. Second, after reviewing problems with recent reforms coming out of this misplaced critique, we then explore a vital dimension of ethnographic value: the capacity for resonance. Building on recent work in cultural sociology, we elaborate and refine the concept of resonance in ethnography, proposing a “textured” model. We illustrate how talking about resonance helps clarify some of the more elusive qualities that constitute good ethnographies. We conclude by noting how time-honored practices of reflexivity, honed through productive dialogue among practitioners, do not pose the same threats to resonance as the proposed reforms.

The “trust me” fallacy

Numerous papers have been published in the past few years, each offering proposals for improving transparency in qualitative research, and ethnography in particular. We see these papers as attempts to address a perceived credibility crisis in ethnography. The starting point for this crisis traces back to an uproar in 2015 over the non-replicability of some survey research (begun in psychology ([Open Science Collaboration, 2015](#)) but extending to political science (e.g., [Broockman et al., 2015](#)) and other fields [e.g., economics ([Camerer et al., 2016](#))]). The turn to ethnography occurred amid debate over [Goffman's \(2014\) *On the Run*](#), an account of young Black men in Philadelphia and how over-policing made them a fugitive class. New calls for ethnographic reform arose as a result, some from outside the discipline ([Lubet, 2018](#)), but others from practitioners themselves ([Pool, 2017](#); [Jerolmack and Murphy, 2019](#); [Murphy et al., 2021](#)).

Implicit within the discourse on reforming ethnographic practice is an anxiety about trust. [Hancock et al. \(2018\)](#) clearly articulate this apprehension while reviewing challenges to validity in ethnographic research, writing, “the reader is (often implicitly) expected to trust the accuracy of the observer and trust that he or she rendered or translated experiences faithfully” (321). While weighing the benefits and risks of identifying participants, [Contreras' \(2019\)](#) reveals how the impact of the discourse, whether intended or not, brings ethnographers' trust into question. Of his ultimate decision to show images of his participants in an academic presentation, a move that compromises their confidentiality, he says, “I refused to be called a charlatan, a cheat, an imposter, or a liar. I wanted to be regarded as a scholar with integrity.” In a recent article published in the *Annual Review of Sociology*, [Murphy et al. \(2021\)](#) suggest that “ethnographic conventions (e.g., deidentifying people and places and shielding or destroying fieldnotes) *arouse suspicion* that the researcher may have something to hide ([Kaminer, 2012](#); [Singal, 2015](#))” (42) and that “ethnographers who insist that standards of replication and verification cannot and should not apply to their work ([Tsai et al., 2016](#)) and that ethics prevents them from sharing their data or disclosing the names of the people and places they study, are being *greeted with greater skepticism*” (42, *emphasis added*).

These statements reflect what we call the “trust me” fallacy within calls for reform, or the assumption that readers of ethnographic texts are being asked to blindly trust the individuals that produce them, the inherent risk being that the ethnographer, willfully or not, will somehow dupe the naïve audience. To meet this alleged threat, reformers offer verification solutions. And the stakes are high: [Jerolmack and Murphy \(2019, p. 819\)](#) warn “ethnography, we believe, risks marginalization if it continues to ignore scholarly and public demands for greater transparency”. We contend, however, that reformers have misdiagnosed the issue. Concern regarding individual researchers' trustworthiness belies the more fundamental point of contention: how do we evaluate interpretive work, especially given its focus on truths that are relational, partial, multiple and contradictory?

We identify two blind spots within existing reforms: (1) an undersocialized understanding of facts; and (2) an uncritical approach to transparency. Consequently, these reforms promote what we consider to be, in some cases, ethically dubious practice

and risk undermining the value of ethnographic research. We contend it is crucial to confront these erroneous conceptions before they become standard practice, particularly as pressure to conform will likely be inversely felt by those with the least power, posing a disproportional burden for graduate students, junior scholars and those who have been historically marginalized within the academy.¹

The problem with facts

“We may call truth [that which] we cannot change,” wrote [Arendt \(2005, p. 313\)](#), arguing that factual truths were those “related to other people...even if [they] occur in privacy” (301), the “other people” recalling Durkheim's notion of “social facts,” which he considered collective forces or entities both external to individuals but also constraining them (e.g., social norms, values and institutions). Thus, facts—or factual truths—are deeply embedded in social contexts. The emphasis on research transparency and replication, however, rests upon an underlying assumption that data are self-evident, able to meaningfully stand apart from the contexts in which they were gathered. This view, of course, contradicts the very strength of ethnographic data, in which realities are multiple, contradictory and contextually bound [see [Pratt et al. \(2020\)](#) for a similar point].

To be sure, reformers—especially experienced ethnographers (e.g., [Duneier, 1999](#); [Desmond's, 2016](#); [Jerolmack and Murphy, 2019](#); [Murphy et al., 2021](#))—wrestle with the important nuances of this point, acknowledging challenges to reproducibility and reducibility in interpretive work. And indeed, methodological debate and technological innovations (e.g., machine learning) have begun to chip away at the hegemony of traditional positivist assumptions and practices across the discipline, with even quantitative researchers starting to openly adopt more idiographic, inductive, and interpretive approaches ([Babones, 2016](#); [Nelson, 2020](#)).² Nevertheless, recent ethnographic reform proposals rely on an underlying assumption of data extractability, and we argue, if enacted as standard practice, they pose the risk of overstepping the reformers' more carefully crafted intentions.

To take one popular proposal, fact-checking, in which a third party vouchsafes the accuracy of one's data ([Desmond's, 2016](#)), what are the sorts of “facts” that these external arbiters are checking, and to what end? We argue that there are multiple kinds of facts, and that among these, at least three kinds—*contradictory facts*, *sedimentary facts* and *motivational facts*—are both essential to the knowledge produced *via* ethnography, and yet not very well suited to a fact-checking paradigm.

1 This task becomes urgent when university press boards and journal referees start to call for the widespread use of fact-checking, heedless of the problems such tactics incur; some editors have noted privately to us, and with perturbation, that they are starting to hear more of these calls.

2 While quantitative researchers have long incorporated inductive and interpretive elements into their practice, this work has often been obscured by writing formats and publication structures that ascribe to canonical social science's narrow ideal of objectivity.

The first kind of facts particularly troublesome for fact-checkers are *contradictory facts*, which arise when what people say is contradicted by what they do, or when data collected *via* one method contradicts data collected *via* another. We consider these contradictory facts findings, as opposed to evidence of poor data quality or a failed attempt at corroboration, because what people say and what people do can both be “factual truths”, but also in opposition to each other. For example, in Pugh’s (2009) ethnography of parents and children wrestling with consumer culture, she found that low-income parents bought very little for their children, but often claimed they bought a lot, while affluent parents talked about how little they bought for their children, who nonetheless had very well-stocked bedrooms. From these facts, she made these interpretations: she dubbed these twin practices “symbolic indulgence” and “symbolic deprivation,” and she argued that each reflected what the parents were positioning themselves against—the incompetent parent who couldn’t provide, or the materialistic parent who couldn’t say no. It is a classic move in ethnography, relying on both interviews and fieldwork observations to juxtapose talk and action while trying to make sense of both, treating subjects with what Fine (2019, p. 831) called “skepticism without derision” (see also Jerolmack and Khan, 2014).

Verifying this contradictory fact would be close to impossible, because to do so would be essentially the same as conducting the ethnography, requiring interviewing and observing in the homes without disclosing the contradiction sought, so as not to inspire parents to run around the home hiding toys or to defensively change their claim. But equally important, checking contradictory facts would be quite discourteous, scorning rather than honoring the gift of participation. Recall in Hochschild’s (1989) *The Second Shift*, when we heard the click, click, click of Nancy Holt’s footsteps going to the laundry after dinner, exposing the inequality behind her egalitarian claim to split the chores with her husband Evan between the “upstairs” and “downstairs.” A fact-checker could perhaps get Nancy to admit that actually, she does most of the chores, and yes, she had said they split it, but there would be some pain in forcing that admission.³ These contradictions are often doing something for the informant—they represent a discursive stance that papers over the irreconcilable cultural conflicts they straddle—and a direct confrontation is intrusive, not to mention obnoxious and even threatening, all to meet the researcher’s goal rather than the informant’s.

The service that contradictory facts are doing for informants—that cultural reconciliation—is, we would argue, sociological gold, one of the most valuable findings ethnographers have to offer (see Vaughan, 1996; Fields, 2008; Van Cleve’s, 2016; Clair, 2020 for other examples among many). Contradictory facts demonstrate not just informants’ acts but the meaning they make from them; they allow us to see the pressures people feel from the colliding demands of their social world, and how they manage that collision. All in one paradox, we can see both yoke and yearning, and how people bend to one without relinquishing the other, a process rendered the more powerful because of people’s reluctance to admit it

(Pugh, 2013). There are circumstances under which the researcher might probe such contradictions without undue harm. But in many cases, forcing informants to acknowledge inconsistencies explicitly, in addition to being difficult and insensitive, would threaten the ability to achieve an important signifier of value in qualitative research: namely, the demonstration of what Small and Calarco (2022) describe as “cognitive empathy”—one of their five indicators of good qualitative research (alongside heterogeneity, palpability, follow-up, and self-awareness). Cognitive empathy, they argue, is about understanding participants’ beliefs holistically, from origination to their ongoing pragmatic value, and in the case of contradictory facts, they help ethnographers understand the work people do to cope with or resolve everyday tensions produced by a group’s structure and culture.

The truth of *sedimentary facts* is more verifiable, but what makes such facts sedimentary is they are individually very small, yet when combined they provide evidence for a particular claim; sedimentary facts also offer the details and texture of lived experience—what Small and Calarco call “palpability”—that lift the narrative off the page, creating, for the reader, a connective virtual space for orienting themselves to or within the story. A source of not only cognitive empathy but also heterogeneous understanding, sedimentary facts are the building blocks of good ethnographies.

The value of sedimentary facts is illustrated in Mosseri’s (2019) ethnography investigating the intersections of intimacy, insecurity and inequality in contemporary work. Detailing life inside a busy Manhattan restaurant she dubs The Jones, Mosseri describes how Ken, a magnetic and longstanding bartender, served as a backbone of the restaurant’s familial culture. Over a period of a few months, however, Ken joined other bartenders in expressing frustration with management over their decision to add additional bartenders to every shift in order to improve service. Because bartenders pooled the money they earned from their own tips and the “tip out” money they received from servers, the managers’ decision undercut their individual earnings. Given Ken’s influence among staff, and even some managers, his dissatisfaction posed a risk to the restaurant’s internal climate.

The tension came to a head one night when Ken was serving a regular guest: a bartender from a popular staff hangout down the street. It is customary within the industry to give a few freebies to valued guests, usually in hope that the act will be reciprocated through higher tips. At The Jones, this practice, known colloquially as “comping,” was officially accepted as a means of cultivating customer loyalty, although each case was ultimately up to managerial discretion. DJ, the manager on duty that night, had not approved any comping for Ken’s guests, yet he noticed that “\$70 worth” of food and drinks had been taken off the regular’s check. He confronted Ken and was appalled by his response: “he basically said that he was comping all sorts of checks because he was angry at us for how we were scheduling, and he wanted to make more money!” Ken later clarified to other staff members that he said something along the lines of “the way you guys are staffing, if I don’t take care of my regulars, I won’t make enough money to live.” Nevertheless, in what another bartender described as “corporate deductive reasoning,” Ken’s actions were interpreted as insubordination and theft, and he was promptly fired.

Shockwaves rippled through the restaurant. Over the next week, staff members made a highly visible display of collecting funds to

³ Some might argue that informants face that pain later, when they read about the contradictory fact in the published work, but as we argue below, they are able to do so privately and on their own terms.

support Ken's "livelihood." When one staff member worried aloud that Ken might think the collection was "pity" money, another quickly rebuffed the idea, saying, "no, tell him we are mad, and we think this [incident] is stupid! This is our way of showing our support and standing up for him."

In reconstructing the story for readers, the ethnographer accumulates multiple sedimentary facts that, individually, are not very material. A fact-checker may try to investigate the cash value of Ken's comped items, confirm the details of the exchange between Ken and DJ or seek to uncover both the formal records and common understandings of the comping policy at The Jones. Doing so, however, would be largely inconsequential to Mosseri's finding that people use money, as a cultural resource, to create boundaries and advocate for their own interests within workplace cultures that emphasize interdependence and community.

Through their public collection for Ken, for example, The Jones' staff drew upon the normative perception of money as depersonalized and fungible to reduce their ties with management to economic terms, countering the restaurant's family ideal and demonstrating that—as solely a source of income—employers, like workers, could also easily be replaced. Ken and DJ's altercation further shows how each deployed distinct social meanings of money to reframe the tumultuous intimate relations of conflicted workplace interests, with DJ viewing wages as a reward, earned through hard work, and Ken viewing wages as his livelihood and thus a right not to be denied. In the former view, "comping" is a practice that has the potential to align worker and customer in a way that threatens meritocracy and consequently workplace hierarchies, while in the latter, the practice serves the needs of all parties: workers through better tips, customers through freebies and the company through improved loyalty. Fact-checking the sedimentary facts of this case loses sight of the larger point that emerges from careful, contextual analysis, akin to debating the brush strokes in an Impressionist painting.

The last kind of fact that challenges the checker but is common to ethnography is what we might call a *motivational fact*. Motivational facts do not necessarily matter if they are correct, because what is important is that the informant believes they are; their inclusion builds the evidentiary base for richly textured multiple meanings generated by cognitive empathy. When Utrata (2015) talked to fathers in Russia for her book *Women Without Men*, for example, she noticed that they swore by a very low bar for what constituted an adequate family man. "As long as his children don't feel like orphans," one informant told her (201); others insisted that all men are unfaithful (195). Men's low bar for fatherhood contributed to a gender crisis that has swollen the ranks of single mothers and reflected the marginalization of men in families, Utrata argues. "In the realm of family life, the negative cultural discourse on men creates a self-fulfilling prophecy," she concluded (229). Whether or not all Russian men are unfaithful is immaterial; what matters is that Russian men believe it to be true. As Wedeen wrote, "When Scott (1985) analyzed how poor peasants and landlords recounted events, for example, he was less interested in whether their narratives were true than in how the disagreement worked to constitute a moral economy of village life" (Wedeen, 2010, p. 267).

Yet what matters with the motivational fact, the reformer might protest, is not the accuracy of the statement but whether

the person said it, which could be verified by reviewing interview transcripts. This poses an enormous logistical problem, of course. If we are supposed to be worried about an ethnographer lying in their manuscript, where they include an abundance of data and information on methodological considerations, why should they stop there? What would prevent them from fabricating transcripts or field notes or creating "deep fake" recordings? More involved fact-checkers could also contact some sample of informants who—assuming they remember and will own up to it—could confirm that they said what the transcripts said they said (e.g., Desmond's, 2016). Yet these steps are once again not just prohibitive, but presume that the situation in which such statements arose—the relationship between ethnographer and informant, the relationships between participants, the context for their speaking—is immaterial.⁴

In actuality, the shaping power of researcher and context is considerable, and this is even more critical in the case of fieldnotes. These ethnographic artifacts are neither self-explanatory nor complete, far from flat documents easily transferred from one researcher to the next, but rather notes that come to life in relation to the researcher (Reyes, 2020). The valuable physiological knowledge produced through ethnographic immersion is rarely explicit within the ethnographers' fieldnotes. This tacit and embodied knowledge may not even be fully accessible to the researcher at the time but instead is made available later through self-reflective analysis and writing.⁵

Most important, the facts outlined above—those that pose such challenges for fact-checking—are exactly those that speak to ethnography's multi-vocality. The conflict, the irony, the wrenching emotion of the contradictory fact, the richly detailed puzzle pieces of the sedimentary fact, the motivational fact's claim to representing particular voices—these are foundational to ethnographic claims and to the value of qualitative work (Small and Calarco, 2022).

To be sure, fact-checkers by themselves do not eradicate these contradictions, and it is of course possible to produce multi-vocal accounts while also—we would argue despite—using fact-checking. Desmond's (2016) *Evicted* being a paradigmatic example. Nonetheless, the calls for fact-checking participate in an overall fetishization of individual "facts" as if they were static, immutable, singular and inalienable, ignoring their complexity, flexibility and polysemy, not to mention the connection between ethnographer and participants that helped to produce them. As we argue below, that complexity is part and parcel of resonance, and one of ethnography's core contributions.

⁴ It is also worth noting that fact-checkers and the tools they use for fact-checking also exist within specific relationships and contexts. Relevant for this discussion is Tripodi's (2022) research into how conservative evangelicals do a lot of their "own research" to confirm discursive claims that exist in the public realm and the ways that seemingly objective search engines, like Google, echo back what these fact-checkers already believe based on the particular keywords used in their search.

⁵ This point about analysis also raises the question: when does data sharing in the name of transparency begin to impede upon researchers' intellectual property rights, increasingly a matter of concern in a post-COVID era when faculty are being asked to generate materials that universities then commandeer?

Given the various ethnographic facts reviewed here, the use of fact-checkers is not very feasible, not very consequential, not very warranted or not very kind. Indeed, their use can seem more like a talisman adopted to reassure gatekeepers (mostly funders or editors) who do not understand or trust qualitative research methods. But by deferring to their suspicions rather than educating them out of them—increasing their qualitative literacy—adopting such talismans only delays the reckoning of the value of qualitative methods and their contributions, posing risks for the multi-vocal complexity that comprises ethnographies' core value and furthering rather than fighting the ethnographic marginalization reformers fear.

Transparency's myth of neutrality

In recent years, practices used to protect research participants' privacy have come under fire. Most notably, Jerolmack and Murphy (2019) argued that “disclosure, not anonymization, should be the default convention within ethnography” (802). The authors warned that “masking” provides a false sense of security given an inability to fully guarantee confidentiality, threatens participants' voice and agency in cases where they prefer to be named, and may even undermine opportunities for knowledge production. More recently, the authors toned down their argument in response to challenges raised by other ethnographers (e.g., Stuart, 2016; Reyes's, 2018; Contreras's, 2019; Seim, 2020), suggesting instead that researchers “anonymize as minimally as possible” (Murphy et al., 2021, p. 49). These arguments have gained traction, with some ethnographers appearing to provide a wholesale endorsement (e.g., Timmermans, 2019). Other ethnographers are more wary of naming practices but have nevertheless offered up concessions, e.g., Contreras's (2019) proposal for partial disclosure, Small's (2018) call for a “pragmatic approach to confidentiality” (197), and Reyes's (2018) case-by-case framework for decisions regarding transparency.

We contend that the terms of this debate are fundamentally uneven. “Masking” implies suspicious practice, while “transparency” ostensibly conveys objectivity, and we take issue with both. Ethnographers do not begin with the goal of obscuring information; their orienting framework—or baseline consideration—is to reduce participants' risk, and anonymization is one of the only and, while not foolproof, one of the most effective tools they have to do so. Moreover, transparency is far from a neutral broker of truth, and portraying it as such ignores or significantly downplays inequalities in privacy and the dangers of visibility for marginalized groups that have been chronicled by many (e.g., Lyon, 2003; Monahan, 2008). Given the unevenness in this debate, and the pressure it places on scholars to adopt reforms, we find it necessary to detail why naming practices are problematic.

Advocates of naming practices argue that ethnographers can never guarantee participants' confidentiality, especially in the Google era (Scheper-Hughes, 2016; Lubet, 2018; Jerolmack and Murphy, 2019). This risk of unintended disclosure is real, and it is something that ethnographers explicitly consider and take steps to minimize, as codified during the IRB approval process. The risk of disclosure should also be, and typically

is, discussed with participants during the consent process to quell a false sense of security. We agree that none of these steps guarantee participants' protection from harm, but we argue that that does not make the overall effort unworthy. Declining to try to conceal identities because to do so has become too challenging in the information age punts the responsibility of protecting participants to participants themselves. Moreover, it denies participants the potential for plausible deniability. As Reyes's (2018, p. 212) writes, “it is one thing to guess at someone's identity and another to know for certain who those people are.” Plausible deniability provides even known participants with some insulation from the potential consequences stemming from findings disclosed.

Reformers also question the very premise that confidentiality is desirable for participants, noting cases where research participants may want to be named and may, in some situations, enjoy material benefits due to their heightened visibility (Duneier, 1999; Jerolmack, 2013; Broughton, 2015). Masking thus undermines such participants' agency and voice. This argument, however, oversimplifies agency and consent. For one, it overlooks the interconnectedness of participants, how one individual's request to be named forces the hand of other participants, some of whom may have more to lose. Moreover, unmasking does not allow participants to change their minds about being known.

Lareau's (2011) revisit to the families that participated in her influential study, *Unequal Childhoods*, provides an illustrative example of how participants' feelings about their association with a study may evolve over time. The families expressed displeasure with the book, but as Lareau notes, “accuracy was not the crux of the problem. The problem was how the families felt about the way they were portrayed” (326). For example, the mother featured in the chapter entitled, “Beating with a Belt, Fearing ‘the School’: Little Billy Yanelli,” thought that the book made her family seem like child abusers. Originally, the Yanellis were excited about the study, anticipating that the book was “going to be like the book Oprah had”; little Billy “had been looking forward to showing people about the book but now he felt he couldn't show it to anyone” (323). These reactions suggest that this family might have sought for Lareau to use their real names if she had made that option available to them, a decision their later comments suggest they would have greatly regretted. The change in their views is a crucial point. Importantly, that change is possible not only prior to publication but also once the research is out in the world, taking on new life as the surrounding social context evolves. Masking makes it possible for informants to act in accordance with their revised views. They retain the ability to show other people the book, or not, and their confidentiality enables them to keep an arms-length distance to any public dialogue surrounding its findings. Disclosure, in contrast, would make informants' felt regrets more common and more acute, largely, we would add, in service to future researchers' potential convenience.

Unequal Childhoods offers rich ethnographic evidence for a set of powerful theoretical findings about how parenting contributes to class reproduction. Social science (and society, we would argue), is surely better off for this book having been written. Yet the costs to the families involved appear to be in some cases fairly high, particularly in their embarrassment and chagrin at what they look like in the book [a point noted by Jerolmack and Murphy (2019)].

As it stands, they are able to wrestle with these emotions on their own terms, out of the spotlight and without potentially significant or long-term consequences associated with public condemnation. Had their identity been known to readers, those costs would undoubtedly be higher and more prolonged.⁶ Neither researcher nor participant know how a book's portrayal will be received; that very uncertainty means it is difficult to control when transparency in research might turn into surveillance of the researched. Public recognition can incur real risks, particularly in an era of trolling and doxing and particularly for communities susceptible to social policing, such as women and people of color (Gosse et al., 2021). These risks are borne largely, but not solely, by participants, who are much more vulnerable than researchers, and for whom the individual benefits are often less. Their vulnerability, and the gift they offer in their engagement with our research, obligates us in the strongest terms to protect them.

Lastly, reformers argue that anonymization practices sacrifice opportunities for scholarly reanalysis, which Murphy et al. (2021, p. 4) define as the marshaling of “any and all available data to independently evaluate an ethnographer's interpretations and consider alternative explanations,” and which can take the form of ethnographic revisits, comparisons to large n data sets and other primary sources, or secondary analysis of field notes. Without the transparency provided *via* naming, they ask, how can scholars pull out sociologically relevant details, determine how to generalize or develop comparison studies? To take the case of revisits, this practice certainly offers intellectual value (Burawoy, 2003). Yet, we urge caution for reasons of both ethics and scholarship. In ethical terms, revisits might veer into exploitative if people feel compelled, because of their (or their predecessor's) involvement in past research, to participate in future studies seeking to build upon the original. Prolonged research increases the costs of participation, requiring continued attentiveness to the (already uneven) distribution of risk and benefit between researcher and researched. As a collective, we need to make sure that the places and people from which we draw knowledge are not being unnecessarily tapped over and over.

Regarding the value of revisits for scholarship, any revisit to a site involves a new historical moment, and often a new researcher, which means they do not generally provide opportunities to “double-check” the original ethnographers' empirical observations, especially given the hard-to-replicate path dependencies created through the qualitative research process⁷; indeed, we echo Pratt et al. (2020) in reminding scholars not to “conflate replication with

trustworthiness” (1). Furthermore, unmasking may not do much to reveal that which was missed by the original observer and of interest to the new ethnographer. We argue parallel studies in a new location or among new subjects may prove equally valuable in searching for negative cases or exceptions, leading to further theory development when found or greater generalizability of the theory when not. For example, despite masking, Kanter's (1977) study of “Indsco” generated numerous subsequent studies, such as those by Williams (1995) and Wingfield (2009), which made valuable revisions to her original theory of tokenism at work. Indeed, we note that Wingfield's study introduced a racial analysis to productively modify the original theory, despite the limited information provided by Kanter on the race of Indsco workers and managers.

Ultimately, we argue, calls for reform seem to ignore the politics of transparency and fetishize ethnographic fieldnotes as “facts,” easily adopted for alternative use outside of the context in which they were produced. Moreover, this discussion fails to acknowledge how ethnographies already prioritize data elaboration *within* the text, a stark contrast with quantitative, hypothesis-testing research, where data reduction is pursued to reduce confounding noise and enable standardized comparisons. Indeed, we contend that unmasking impedes this more robust transparency within ethnographic work, as it treats identities as largely fixed, not shaped in tandem with their social environments, bearing implications for multivocality and the ability to honor the fluidity and complexity of human life and emotion. The pitiless glare of notoriety is not conducive to the nuance, flexibility, and ambivalence of ethnography, which we consider its greatest strengths. Unmasking exposes informants to demands for narrative and emotional coherence, and shames those who are forced to bear witness to their own compromises. Voices would be less likely to haunt an unmasked ethnography, and instead simply ring forth with positions people are not afraid to espouse under the gaze of others. Instead of unmasking, we encourage ethnographic readers to pay attention to “follow-up” (Small and Calarco, 2022): is there evidence that the researcher probed on statements and events that were confusing? This real-time practice honors the emergent aspects of ethnographic data that enrich the account.

Reforms stemming from the perceived credibility crisis have a number of different problems, but most critically, they reduce ethnography to its component parts—facts and names and typologies—and interrogate each piece for some inner truth. To give too much weight to these verifiable pieces, especially over and above the more holistic narrative presented by the ethnographer, would be akin to claiming that “a birth certificate is a birth, or a script is a performance, or a map is a journey,” an error shrewdly highlighted by British author Mantel (2020) in a Reith Lecture. In short, these reforms challenge the complexity that makes ethnography valuable, and of note for this paper, their capacity for resonance.

Resonance and ethnography

If proposed reforms threaten what we think is most valuable about ethnography—its capacity for resonance—our discussion about the reforms would not be complete without outlining the

6 To their credit, Jerolmack and Murphy discuss the Lareau case in their 2019 article urging ethnographers to reconsider masking as default practice. Citing ethical concerns, they argue that the fact that the families were unhappy suggests that masking did not protect them emotionally. Yet ethical pursuits are not limited to the eradication of harm but also to its reduction. As the sex columnist Dan Savage once said (speaking about monogamy), ethical practices are more like sobriety than they are like virginity – something to keep striving for, rather than something that once lost is gone forever.

7 See Burawoy, 2003 for a useful discussion of two such attempts at refutation and how they were met with defenses based on the subjectivity of data: Freeman's (1983) revisit of Mead (1929) and Boelen's (1992) critique of Whyte (1943).

concept of resonance, how ethnographies accomplish it, and how the reforms impede it. In what follows, we build upon recent scholarship in cultural sociology to elaborate and refine a “textured” model of resonance as an example of what ethnography does well. By outlining this concept of resonance, we seek not just to demonstrate how we evaluate better or worse ethnographies but also to illuminate the broader value of good ethnographic research for social scientists, and ultimately, for society.

The history of the concept of resonance has largely been situated within social movement scholarship (e.g., [Snow and Benford, 1992](#)) and in research focusing on the cultural reception of music or art (e.g., [Binder, 1993](#)), but has been plagued by the lack of a shared definition ([McDonnell, 2014](#)). Moreover, some of this earlier work seemed to conflate resonance with relevance: resonance reflected a connection between a cultural message or symbol and an audience with interests that were socially constituted beforehand (e.g., [Schudson, 1989](#)). For Schudson, for example, cultural objects obtained resonance in part from how their audience was able to put them to use, as informed by how these objects interacted with prior traditions. “In this view,” write [Hallett et al. \(2019, p. 548\)](#), “a social science idea would have ‘resonance’ with the public to the extent that it fits their worldview, experiences, and expectations.”

More recent scholarship has tackled some of these limitations. In a series of publications, [McDonnell \(2014; 2016; McDonnell et al., 2017\)](#) offer what we might term a “pragmatic” model of resonance, through which they usefully add a needed dynamism to the model, resolve the tautological quality of earlier definitions, and suggest ways to measure it [see also [Glaeser’s \(2011\)](#) discussion of “resonance in pursuit”]. The “pragmatism” of the model is one that locates the “point” of culture in helping people solve problems, broadly construed. A resonant object, they contend, “may crystallize a previously unarticulated experience, provide a novel way to approach a problem [that] actors routinely encounter, or actually problematize something previously taken for granted in a way that sheds new light on an old pragmatic problem” (2017, p. 4). When people have emotions they do not know what to do with, for example, a well-timed ethnography can help to “solve” that conundrum by offering clear reasons for those sentiments, for example (e.g., [Bonikowski, 2017](#)).

The benefits of the model are several. First, [McDonnell et al. \(2017\)](#) urge a particularly dynamic approach, arguing that resonance is not a fixed trait that cultural objects have or do not have, but rather it is an attribute-in-relation that emerges in a given cultural context and can later subside. Their model places a resonant cultural artifact not only *within* the specific relationships between author, object and consumer but also *at* a particular time and place. Second, they argue that resonance is about more than just an echo of what we know already, but rather a means of connecting what we know to what we do not. Cultural objects become resonant as they help audiences make sense of their experiences and interactions, and so they feel like an “aha” moment, “heightening emotions and enabling actors to transcend what was previously taken for granted” ([McDonnell et al., 2017, p. 4](#)). Finally, [McDonnell \(2014\)](#) adds some helpful means of operationalizing resonance—in the heightened state of emotions with which people greet or absorb the idea. These

contributions have spawned a renewed interest in resonance, and scholars have found the approach fruitful, applying it to a range of studies such as how social science contributions become “public ideas” ([Hallett et al., 2019](#)), how organizations appeal to volunteers ([Paxton et al., 2020](#)); and how radical right politics mobilize collective resentment ([Bonikowski, 2017](#)). Interest in resonance spans multiple sociological subfields.

Understood in this way, it becomes clear that providing resonance is also at the heart of what ethnographers seek to do. Ethnographies reflect the social world, but through the analysis, reassemble it in a new way, making the strange feel familiar or the familiar feel strange. [Hochschild’s \(2016\) Strangers in their Own Land](#) is a good example: it offered an illuminating metaphor for the seemingly irrational contempt of government among those with the greatest need for its help as akin to the everyday, shared frustration of waiting in a line that, for various reasons, fails to progress. Ethnographers use familiar chords to bring new sense-making tools to readers, and through both alignment *and* transcendence, they achieve resonance with readers. As the anthropologist, [Messeri \(2017\)](#) describes it, resonance is how “the knowing and sensing subject”—whether that be the ethnographer or the reader—“detects and amplifies connections between discrete, distant objects and worlds” (132). She explains that resonance “brings closer the conceptually distant worlds that culture tends to reify” (140) and “allows humans to know one another” (133). This result reflects the more dynamic understanding of resonance that [McDonnell et al. \(2017\)](#) suggest, in which the cultural object not just echoes but reconfigures or expands.

At the same time, however, there are limits to what the pragmatic model can explain about resonance in ethnographies. First, its insistence on the practical utility of resonant cultural ideas or objects shares a generative tension with the robust finding of the ambivalence or multi-vocality of compelling cultural objects ([Reed, 2011](#)). It is the very flexibility of meanings that allows certain ideas to speak to large audiences, since, as [Schudson \(1989, p. 159\)](#) noted long ago, “no cultural objects work with everyone, none of them affects even the people they do affect in the same way.” It is not that the pragmatist emphasis on problem-solving and this kind of flexibility inherent in resonance are contradictory exactly—we can imagine that rich, complex cultural ideas might allow their audiences to express their own ambivalence, which also “solves a problem” of sorts. In addition, the ability to re-apply a cultural idea to a new situation is part of the “interpretive flexibility” of particularly “public” ideas, according to [Hallett et al. \(2019\)](#); [see also [Vaughan \(2006\)](#) re: the ethnography as “boundary object”]; [McDonnell et al. \(2017\)](#) make room for this kind of periodic renewal with their dynamic approach to resonance-as-process.⁸ Yet viewing cultural objects as “solutions” suggests a certain fixity to their meaning-making, and rather less flexibility than more.

This points to a larger flaw regarding the pragmatic approach: problem-solving, even when broadly construed, takes as its focus

⁸ The Salem witch trials, for example, achieved a certain resonance in their day. According to [Reed \(2015, p. 87\)](#), they managed to corral public opinion through a process he dubbed “resignification,” which “binds consensus by suppressing alternate interpretations of the course of events.”

“problems,” and intimates that they are overcome; it creates a dyad between problem and solution. Yet, the social world is much more multidimensional and complex—we might sit with problems or worry them like a bone; dilemmas can create motive or structure; we may be anxious or unmoored by them or even enjoy them. Of course, pragmatists might counter this point with the notion that solutions also are plural and not one-dimensional. Nonetheless we maintain that the issue here is not how complex the solutions are, it is that the very definition of problem-solving seems to set the world into two binary categories and thus threatens the heterogeneity of the study’s findings.

Second, we would argue that the pragmatic model offers a fairly limited role for emotions. The model seems to look to emotions as a stimulus for resonance, but emotions are also a medium for resonance, which we consider a crucial distinction, albeit challenging to parse operationally. The former notion of emotions-as-stimulus prompts resonance, but resonance remains largely cognitive. The latter emotions-as-medium conceptualization argues that resonance is an emotional process, at least in part. In addition, while the pragmatic model usefully invokes an emotional dimension to resonance, its authors seem to insist on only a positive view. In his earlier article, [McDonnell \(2014\)](#) asserts “a strong connection to positive affect,” arguing that the content of the “aha” moment “matters tremendously,” and that resonance means not fear, horror or shock, but ebullience (262). Later, [McDonnell et al. \(2017\)](#) distinguish between salience (“when an object or idea becomes a social problem”) and resonance (“when objects and ideas *solve* practical problems”) (9). Yet not all examples of resonance invoke solely positive emotion. If we agree that [Bonikowski \(2017\)](#) is analyzing the resonance of radical right ideas, for example, the collective resentment that they harness is very related to fear, horror *and* perhaps a certain ebullience (see also [Lamont et al., 2017](#)).

These differences, while partial, are important; they also center on the unique potential for resonance in ethnography. One of the primary strengths of ethnography is in its capacity to convey and elicit emotional ambivalence, contradiction, and the multiple meanings of many voices. In this way, resonance is both a tool for researchers in their work and a product of their work ([Messeri, 2017](#)). To adequately center these contributions, we develop what we term a “textured” model of resonance.

In this model, resonance is, as the literary historian Stephen Greenblatt (2018 [1990]) argues, “the power of the displayed object to reach out beyond its formal boundaries to a larger world, to evoke in the viewer the complex, dynamic cultural forces from which it has emerged and for which it may be taken by a viewer to stand.” Note that this definition does not differentiate between personal resonance and what we might consider collective resonance, or appeal to a large audience; we share this agnosticism about resonance’s scale. More important, resonant objects are multidimensional, containing diverse voices and complex histories, often excavating difficult emotions while also potentially moving audiences toward a longing to overcome what society is, has been, or could become.

As an example, Greenblatt describes an exhibit of Judaica from communities across Moravia and Bohemia, housed in the Prague State Jewish museum, which was distributed across

several area synagogues, including the “Old-New synagogue” from the 13th century. Contrasting resonance with wonder, which he defined as “the power of the displayed object to stop the viewer in his or her tracks, to convey an arresting sense of uniqueness, to evoke an exalted attention” (Greenblatt, 2018 [1990], p. 265), Greenblatt notes that the objects themselves are rather ordinary and not particularly arresting aesthetically. Their resonance, he argues, “depends not upon visual stimulation but upon a felt intensity of names, and behind the names, as the very term resonance suggests, of voices: the voices of those who chanted, studied, muttered their prayers, wept, and then were forever silenced” (268). The voices belong to Jews murdered in World War II, Greenblatt writes, but also to those massacred while seeking refuge in the Old-New synagogue in 1389.

Yet the complexity does not stop there. The museum’s “ultimate source of resonance,” Greenblatt (268) argues, is that it was the Nazis who amassed the bulk of the collection. “Most of the objects are located in the museum—were displaced, preserved, and transformed categorically into works of art—because the Nazis stored the articles they confiscated in the Prague synagogues that they chose to preserve for this very purpose.” Abused and malnourished curators were tasked with organizing and displaying these objects for SS officers’ private viewing, until they themselves were rounded up and sent to the camps to die. After the war, the Jewish community donated the objects to the state for their preservation, Greenblatt reports (269), creating the “resonant, impure ‘memorial complex’ they are—a cultural machine that generates an uncontrollable oscillation between homage and desecration, longing and hopelessness, the voices of the dead and silence.” Resonance is not about consensus nor does it resolve; its captivation relies, in part, on the tension—the constant oscillation between competing ideas and emotions—that it evokes.

Thus the conventional definition of resonance that prevailed for decades—as that which confirms what audiences think already—fails to capture the complexity that makes resonant ethnographies so powerful. The pragmatic model improves upon this original idea with a hybrid vision of new and old—the aha! epiphany. But even so, as noted above, scholars intimate that resonance comes with a sense of new clarity. Ultimately, the pragmatist priority—culture must solve problems for its users—seems a bit awry here, if not actually wrong. Resonant ethnographies do more than explain, they raise curiosity, leading us to ask particular questions, look for particular clues, notice particular details. When we move beyond a problem-solution binary, we find ourselves able to hear a bit more from a scene: the sometimes many contradictory voices, the irony, the complexity, the multiple layers, aspects which deepen and enrich our experience and understanding.

These aspects also happen to be exactly what ethnography brings to the social science table. The textured model of resonance argues that sometimes resonance is not about the reach for clarity, suggesting instead that multiple layers of meaning—not all of which agree, or point in the same direction, or tell us to feel the same thing—contribute to resonance. To achieve resonance, ethnographies unearth contradiction, irony, poignance, or paradox ([Vaughan, 2004](#)); they exhibit multi-vocality. As Greenblatt writes:

“the key [to resonance] is the intimation of a larger community of voices and skills,” what he calls (269) “an imagined ethnographic thickness.” Second, the emotions that resonance invokes, indeed relies on, are far from only positive, but instead a complex welter. Often the voices that lead to resonance are powerful not because they resolve a persistent concern, but because they haunt a given cultural object, akin to what O’Brien (2009) called “an inescapable scratchiness.” It is that haunting that makes for the persistent thrum, the reverberations that create resonance.

As an example, consider *Crook County: Racism and Injustice in America’s Largest Criminal Court*, Van Cleve’s (2016) ethnographic account of the Cook County criminal court.⁹ In accordance with the pragmatic model, *Crook County* became resonant within a particular cultural context. The book was published in 2016, at a time when white Americans were faced with a puzzle: evidence of enduring racism in police shootings and right-wing resurgence despite a colorblind ideology that peaked in the early years of the Obama presidency. In this perplexing moment, Van Cleve connected what was already known to relatively new insights: racism persists without racists; racism is not a pathology within institutions, it often serves a function for those institutions; the criminal justice system does not simply produce racist outcomes, it operates through racist processes. The narrative in *Crook County* offers some explanation of the persistence of American racism to a growing audience interested in understanding it.

Yet, the book’s value involves more than explanation. The power of the narrative is driven by its complexity and multidimensionality. Van Cleve juxtaposes maddening and heart-wrenching stories of “racial degradation ceremonies” that take place within the court system with a patient immersion into the professional culture that fosters them. The reader bears witness to court professionals mocking, ridiculing, and verbally abusing defendants, especially poor people of color, many of whom entered the criminal justice system *via* false allegations or minor infractions. The narrative condemns the professionals’ behavior and makes visible the intense suffering it produces, but simultaneously salvages a piece of their humanity. Van Cleve describes how professionals’ devotion to justice is subverted within a cultural logic that renders the targets of their abuse as morally worthless “mopes,” distracting them from the more rewarding pursuit of prosecuting violent “monsters.” We learn how professionals rationalize their behavior within the broader system of justice and how most view themselves as neutral participants—if not allies—in the fight for racial equality.

Crook County resonates, not solely because it solves a problem, but because it excavates the complexity, the messiness, the irreducibility of life. The social world entails conflict, compromise, and an enduring lack of resolution, and ethnographies that resonate are like a prism, parsing the sunlight to reveal the multitude of color hidden within. Contradictions are not beside the point, they are the point.

9 Crook County was also the recipient of many accolades: among these, the American Sociological Association’s 2019 Distinguished Scholarly Book Award, the recipient of the Society for the Study of Social Problems’ (SSSP) Eduardo Bonilla-Silva Outstanding Book Award and a finalist for the SSSP’s C. Wright Mills Book Award.

The power of reflexivity

We have argued that recent ethnographic reforms demand narrative coherence, impede flexibility and polysemy, and fetishize “facts” and “fieldnotes” as if they were static and immutable. These reforms confuse transparency with authenticity, constructing ethnography as a window instead of a prism, pursuing verification over interaction and achieving clarity at the cost of complexity. In sum, they sacrifice ethnography’s resonance. However, there are already time-honored practices, honed through productive dialogue among practitioners, that address issues of credibility and trust in ethnography without trading off its central contributions. While reformers dangle such a costly path to legitimacy for ethnographers, we contend that if readers do not trust the ethnography by the time they finish reading, fact-checking or exposing names and places will not fix that problem. Instead, by that point and for those readers, the ethnographer has already failed in their task. Trust in ethnography is built incrementally, through practices of reflexivity.¹⁰

The three most important approaches to reflexive practice, as we consider them, are *pursuing radical self-consciousness*, *interrogating consensus* and *exploring inconvenient data*. There are other reflexive practices that are worthwhile, such as checking back with informants or listing anonymized participants and their relevant characteristics in the text; we view the three approaches as broader and worth discussing because many reflexive practices are encompassed within them. As they have been the subject of extensive scholarly conversation and in many cases reflect longstanding practice, our discussion here is necessarily abbreviated. Our point, however, is twofold: that ethnographers build trust bit by bit rather than simply rely on readers’ faith, and that these practices do not generally risk other dimensions of value, such as resonance.

Radical self-consciousness, or the ongoing consideration of how one’s identities, relationships, expressions and resources shape the research process, is replete in ethnographic texts.¹¹ This practice focuses on the complexity, contradictions and sometimes changing perspectives that ethnographers can inhabit. Because ethnographers are themselves the instrument of data collection and analysis, they more continually confront the opportunities both blocked and made possible through their social location, and they are attuned to how their subjectivities and theoretical commitments shape the phenomena they notice and the insights they glean (e.g., Duck, 2015; Reyes, 2020). As established practice, ethnographers “constantly ask ourselves about our research design,

10 While the new urgency to reanalysis usefully highlights the tensions of authorship (Murphy et al., 2021). Ultimately, we think, an ethnography represents one person’s take, and these existing standards of reflexivity allow readers to see the author’s positionality. We do not need a reanalysis of their ethnographic site to evaluate that.

11 This concept is loosely akin to what Small and Calarco (2022) call “self-awareness.” As the authors note in their book, the concept of reflexivity varies substantially across the discipline and the social sciences more broadly. Our goal in providing this three-pronged overview is to add detail to discussions of reflexivity that might help to alleviate confusion and conflation within a complex and nuanced domain of academic writing.

our relationship with our research participants, the labels we give them, and the way we write about them" (Rios, 2015, p. 260). This practice is not extraneous to ethnography's conduct, "extras" that are somehow icing to the ethnographic cake; instead they are the cake, ethnography's widely shared norms, albeit imperfectly followed.

Interrogating consensus is when ethnographers check common phrases and practices for assumptions that erase other viewpoints, often centering the perspective of their site participants. A recent example of this strategy in action is Altomonte's (2020) research within post-acute care units that serve elderly patients recovering from hospital stays of three or more days. Altomonte finds that care staff are morally committed to the goal of "independent aging," defined broadly as patients' return to autonomous life within their own homes. However, care staff toggle between different meanings of independence as they negotiate specific patient orientations within the competing mandates of safe and fast patient discharge that define their organizations. When trying to ensure safe discharge for patients perceived as being too hasty to return to their previous routines, for example, staff emphasize how *independence entails acknowledging one's limitations* and need for specific accommodations (e.g., walkers, sliding shower seats, at-home caregiver). In contrast, staff emphasize *independence as taking personal responsibility and achieving self-reliance* to prompt timely discharge when working with slowly progressing patients. By grounding the analysis in the lives of her participants—as opposed to existing social categories—Altomonte uncovers a complex and nuanced story that demonstrates how the ambiguity of moral concepts enables care staff to invoke seemingly contradictory logics at different points in time. Ethnographers interrogate abstract concepts, keenly aware of how universal language can erase participants' lived experience and agency, and they display skepticism toward systems of classification, known to be a source of symbolic violence (Bourdieu and Wacquant, 1992). In the field, they bring the taken-for-granted under scrutiny, questioning the "obvious" meanings of familiar vocabularies and practices (see Vaughan, 1996 for a paradigmatic example). This interrogative, skeptical curiosity is part of the ethnographer's arsenal in the bid for credibility.

A third approach to reflexive practice is the constant exploration of what Duneier (2011) calls "inconvenient" data. Inconvenient data are made up of examples—those negative or exceptional cases—that throw a wrench in the theories or arguments that an ethnographer might deploy. These anomalies are useful in that they can expose blind spots in the ethnographer's thinking, and they may prompt a productive reconfiguration of the analysis; they also add—rather than necessarily solve—contradictions or deviations in patterns that can enhance resonance.

The search for inconvenient data sometimes starts with research design. In *Pricing Beauty*, for example, Mears (2011) analyzes fashion modeling as a "deviant case" in which women outearn men. Traditionally, men working in feminized fields experience a "glass escalator" effect (Williams, 1992), in which they quickly move up within organizational ranks. In the case of fashion modeling, however, precarious, short and non-linear

careers disrupt this process (Mears and Connell, 2016).¹² The anomaly of the inverted wage gap also sheds light on how the objectification of women's bodies is culturally celebrated, while men's sexualized bodies are devalued. Women earn more in fashion modeling, Mears shows, but at the cost of reproducing pernicious cultural beliefs about gendered bodies. The pursuit of inconvenient data also occurs during data analysis (e.g., Thorne, 1993; Khan, 2011), making conclusions at once more refined and more nuanced as a result of the consideration of exceptions within the data. Like pursuing radical self-consciousness and interrogating consensus, exploring inconvenient data introduces multi-vocality and complexity to ethnography.

These widely practiced strategies of reflexivity bolster the credibility of ethnographic research and do so not by reducing but by maintaining complexity. Ethnographers use these and other approaches as an opportunity to explore and bring attention to multiple, coexisting realities, many of which are concealed by the processes of standardization and generalization common within other research methods (Collins, 1990; Harding, 1992). As Bourdieu and Wacquant (1992, p. 236) argue, "a scientific practice that fails to question itself does not, properly speaking, know what it does...it records itself without recognizing itself". The upshot of this reflexive undertaking, however, is not to superimpose the standards of other methods onto ethnography; rather, we urge practitioners to pursue the kind of reflexivity that does not harm its capacity for resonance.

Conclusion

Concern over ethnographic methods is burgeoning, propelled not only by controversies bedeviling recent examples of the trade, but by the credibility crisis roiling psychology and calls for greater transparency, replicability and access to quantitative data from within academia as well as the lay public. Reformers express worry about the marginal status of ethnography in a positivist discipline. Yet in seeking out a reluctant anointing from suspicious others, many of whom already view ethnography as not-quite-social-science, these reforms increase the vulnerability of ethnographic participants, and further, threaten to undermine what we consider what ethnography does well.

Instead, we join Small and Calarco (2022) in calling on practitioners to articulate and embrace ethnographic best practices, strengthening and developing them from within an interpretivist perspective. Ethnographic research enables rich accounts of social worlds and the perspectives of their inhabitants, helping to dislodge social life and reveal its dynamism. Crucially, ethnographic best practice includes striving for resonance by offering up concepts and practices that invoke multi-vocal, flexible meanings with interpretive depth and complexity, developing insights with "chimes that people feel down to their feet" (Lepselter, 2012, p. 101). These strengths are at stake in proposals that strive to nail down "facts" or name informants.

¹² The authors also note similar dynamics among porn workers and strippers.

Ultimately, we also question whether the reforms would do much to shore up the legitimacy of ethnography among its critics. At best, it seems ethnographers would be settling for ill-fitting, yet universal standards of evaluation, akin to judging all movies—not just comedies—on whether or not they are funny. At worst, fact-checking and naming practices may provide critics with resources to further scrutinize ethnographic work that they do not like. Rather than depending on outside experts to establish veracity or expertise after the research is completed, however, ethnographers carefully bid for the confidence of their readers by adhering to common strategies of research conduct from beginning to end. Their efforts will not convince every reader. Yet these strategies do not depend on *ex post facto* stamps of verification from some external source, but instead work to establish trust in layer after incremental layer, through practices of reflexivity.

We are struck, in this debate about research credibility, by the tacit focus on individual researchers, as if they alone are the problem-maker or savant. We view this approach as asociological, ignoring how perceptions of trustworthiness are shaped by relations of power and inequality (i.e., Cook, 2005; Gambetta and Hamill, 2005; Ridgeway, 2009) and potentially leading some researchers—namely, early career researchers and those underrepresented in the most secure positions of academia—to be more vulnerable to scrutiny than others. Suggested ethnographic reforms, such as attempts at replication or calls for fact-checking, may as a result disproportionately target less powerful or historically marginalized scholars. Due to their social position, some scholars may therefore feel greater pressure than others to adopt practices like fact-checking, unmasking and sharing fieldnotes that are costly in terms of money, time and even physical safety (e.g., Reyes's, 2018; Contreras', 2019).

Alternatively, we encourage a more collective conceptualization of the problem, as well as its solutions. In an era in which many social science disciplines are slouching toward a methodological uniformity, shunning or defunding non-statistical approaches, one of sociology's strengths is its unique commitment to methodological omnivorousness. We need to match that commitment with institutional changes in training. Hancock et al. (2018) report that only 20% of top-20 sociology departments require a qualitative methods course of its graduate students—of which ethnography might occupy 1–3 weeks—while all of them require a quantitative course. Improved qualitative training would better enable fruitful methodological debate and strengthen peer review processes.

Anticipating that some may interpret our discussion of resonance as an appeal to popularity, we point out instead that the textured model of resonance is not fueled by commonality or even *a priori* alignment; rather, the heart of resonance within the textured model is the establishment of a meaningful connection. This connection, or relationship, can just as easily be derived from difference and disruption as from consensus. In this way, resonant texts respond to Abbott's (2007) call, within his “lyrical sociology” manifesto, for texts that confront us, as readers (and as authors), with “the radical chasm between our own here and now that of its subjects.” In revealing this difference, argues Abbott's (2007), “the chasm itself is crossed by our moral recognition of the common humanity we share with those we read about” (95). Resonant

texts do not bow to popular morality, but by unearthing conflict and contradiction, they can—and frequently do—spark a moral consciousness that can bring us together.

Others may argue that resonance privileges style and form over substance and veracity—that captivation is in tension with truth. Yet, ethnographers' respect for inconvenient data, as highlighted above, belies this notion. Like some of the world's most celebrated artists, the best ethnographers, we contend, view dissonance as a resource, adding complexity, depth and drama to the work, not as an impediment to its beauty or even its coherence.

This critique also echoes longstanding (gendered) debates about emotion and rationality, subtly implying that the emotional dimension of textured resonance—the haunting, the “thrum” of feeling—can shape how an audience evaluates an ethnography, inciting passion and overcoming uncertainty. We have neither the space nor desire to rehash these debates here; suffice it to say, one is no easier misguided by a text that moves them than by one that relies on clinical but faulty or homogenous evidence. Indeed, we encourage greater attention within the discipline to what elements of truth may be lost with the latter.

A final potential limitation or downside of textured resonance as a feature of ethnography is that the fluidity and multivocality of resonant ethnographies may make them vulnerable to cooptation by politically motivated actors. Resonant ethnographies are complex and nuanced, stitched together to produce a *sui generis* patchwork. Purposefully fragmented findings from ethnographies risk misrepresentation when depicted in isolation within citations, media coverage or everyday talk. We would argue, however, that the fetishization of facts within proposed reforms are likely to promote—not prevent—the fragmentation and cooptation of ethnographic findings.

While we have focused on staving off reforms that we believe address a credibility crisis that ethnography does not have to own, we want to conclude by making a claim for the sheer value of trust as a practice. The merit of preserving trust in academic work seems particularly relevant in an era when many Americans feel that their confidence in social institutions has been betrayed. There are worthy practices and activities that enrich our world but that fundamentally at their core depend on a modicum of trust. There is a kind of leap of faith that is necessary to bring this sort of work into the world, and that faith is worth defending for the work and insight it makes possible. This is not to say that ethnographers should not work hard to demonstrate that their conclusions are sound—they should, and as we have shown, they do. At some point, however, the risk of deception is turtles all the way down, with manipulated records supporting manipulated texts. Transparency does not eliminate deception, and in fact, it can legitimate it by giving a false perception of disclosure. Ultimately, ethnography's distinctive contributions, as well as the substrate of trust on which all academic work ultimately depends, show us the importance of trust and trustworthiness for the sociological enterprise.

Data availability statement

The data analyzed in this study is subject to the following licenses/restrictions: interview transcripts and fieldnotes are not

publicly available. Requests to access these datasets should be directed to sarah.mosseri@sydney.edu.au.

Ethics statement

The studies involving human participants were reviewed and approved by University of Virginia's Institutional Review Board for the Social and Behavioral Sciences (IRB-SBS). The patients/participants provided their written informed consent to participate in this study. Written informed consent was obtained from the individual(s) for the publication of any potentially identifiable images or data included in this article.

Author contributions

AP developed the first iteration of the conceptual framework and the first draft of the paper. SM substantially revised the draft for critically important content. All authors have continued to revise and refine the draft.

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Funding

AP's work on this article was funded by the Berggruen Institute.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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OPEN ACCESS

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RECEIVED 09 January 2023

ACCEPTED 03 April 2023

PUBLISHED 04 May 2023

CITATION

Conover T (2023) Slow journalism: a letter to
ethnographers. *Front. Sociol.* 8:1141033.
doi: 10.3389/fsoc.2023.1141033

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Slow journalism: a letter to ethnographers

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KEYWORDS

ethnography, journalism, participant observation method, anonymity, privacy

At New York University I teach a graduate seminar called Ethnography for Journalists. The aim of the course, I tell my students, is not to learn how to write ethnography. Rather, I say, nobody has invested more energy thinking about humans as social beings than social scientists. What have they learned that we as journalists can borrow? I explain that it's a course in longform narrative writing and ethnographic thinking. It aims to teach students to be not an ethnographer but rather a journalist who can perceive the world like one.

The first year I taught this class, I assigned work by great anthropologists I'd read as an undergraduate, including Radcliffe-Brown, Malinowski, Geertz, and Benedict. Big mistake: I could see students' eyes glaze over as they failed to connect with the prose of titans. Soon I found a better text, Mitchell Duneier's *The Urban Ethnography Reader*, starting with its introduction, where Duneier cites "the old canard that ethnography is merely 'slow journalism.'" Students of journalism quickly grasped what he meant when he explained that ethnography "seeks to go beyond what people might say in interviews and to reveal understandings that emerge only after countless interactions over the course of time." That made sense, as did letting them choose their own readings from the dozens collected by Duneier.

I consider this essay to be a sort of letter to the land of bona fide ethnographers from an admirer on the outskirts—a letter of appreciation and self-explanation that might inform the space between journalism and ethnography, where I spend a lot of time. I am a working journalist, and professor of journalism, whose exposure to ethnography suggested paths toward a deeper journalism that I have pursued, and tried to teach, for many years. I am flattered when real ethnographers point to me as a fellow traveler. Yet at the same time, I know that our projects are different. Most of what follows is an accounting of the ways ethnography has empowered my journalism, though I will also speak to differences in our pursuits and the ways in which I do *not* emulate traditional ethnography. My goal here is not to critique current practice (I am not really qualified nor especially interested in doing so) but rather to describe how I do things and speak to this the space between our two traditions, which I believe contain ways to enrich each other.

The number of journalists interested in ethnography is not large. Conducting ethnographic field work takes time, and journalists (probably like most ethnographers) seem to have less of it than ever before: the rise of digital publishing and advertising have ravaged newspapers and magazines and pushed thousands of journalists out of work. Those who remain feel under constant pressure to produce not just timely content (aka, news) but clicks and page views. Then there are the things ethnographers typically do that journalists normally don't, including seeking approval from an institutional review board, masking the identities of people and places, and aiming for an academic audience rather than a general one.¹

Still, I tell my journalism students, there is much we can learn from social scientists. I try to contextualize the various insights, lessons, and research strategies I learned from

¹ A work of literary journalism that draws more heavily on the work of ethnographers than any I know (and cites them all in extensive source notes) is Anne Fadiman's *The Spirit Catches You and You Fall Down: A Hmong Child, Her American Doctors, and the Collision of Two Cultures* (1997).

ethnographic studies of social worlds in stories I tell them from my own research. For example,

- Social worlds can be understood in terms of shared meanings, which often correlate to personal qualities the culture admires. Corrections officers, I learned at the New York State training academy, say their work is about the Care, Custody and Control of inmates. Above all, in my experience, they admire an officer's ability to *control* prisoners, especially their movements into and out of controlled spaces inside the prison.
- Social worlds can be understood in terms of status hierarchies. The railroad tramps I traveled with often shared the following ranking with me, once they learned I was new to the rails: The most admirable person on the rails is the tramp, who travels and works. Next best is the hobo, who travels but doesn't work. Least prestigious is the bum, who neither travels nor works.
- Social worlds can be understood in terms of material artifacts. Prison officers care greatly about keys (Why? Because they are instruments of control). USDA inspectors and other meat workers devote a huge amount of attention to the knives they use in a slaughterhouse (Why? Because the sharpness of a knife correlates to the effort required to cut into meat, and how much pain one feels in one's hands and arms at the end of the day).²
- Social worlds can be understood in terms of the meanings ascribed to certain spaces. Railroad tramps have a shared ranking of *places to ride on a train* (Protection from the wind and from the view of railroad employees are key factors). Undocumented migrants quickly acquire a keen understanding of the kinds of places they might be hassled (by police or by racists) and the kinds of places they'll be left alone.
- I talk about the tensions between etic and emic understandings. I say that the fullest understanding of a social world or subculture might come from appreciating both kinds of meaning, and the tensions between them (The general public believes that corrections officers are often brutal, for example. That explains why a corrections officer, making a casual acquaintance outside the world of prisons, might tell the new friend they work in a different occupation than they in fact do—they might say they're a contractor, in other words, or that they work in private security).
- I talk about participant-observation. I suggest that students view it as a line with two endpoints: researchers starting work on a story about a new social world begin as observers. But their goal should be to participate: to talk to people, of course, but also insofar as possible to share food with them, travel with them, work with them, or simply "hang out" with them in the spaces they inhabit. The goal is to move along the line from being an observer (who might, say, learn about football from watching TV) to being more of a participant (who might interview players in their locker room or broadcast interviews from the sidelines of a game). The more you participate the closer you come to having an insider's perspective. But of course, while you may aspire to have a

participant's understanding, you never will completely succeed (and then we discuss why you won't, and how that matters).

Next semester, I will assign students the brief prolog to my recent book *Cheap Land Colorado: Off-Gridders at America's Edge* (2022). The prolog describes a moment several weeks into my research, when I'm not yet living on the prairie but I'm making progress on gaining a participant's understanding. In it, I'm the passenger in a pickup truck driven by a rural outreach worker in Colorado's expansive San Luis Valley. The very first thing that happens is that the worker explains to me how he approaches a stranger living off-grid in the remote precincts of the valley where land is very cheap. His goal is to make a connection with the prairie dweller, who well may be hostile to his presence; high on his list is putting the resident at ease, especially important because so many of them fear strangers and have firearms ("If they have a flag then they probably have a gun," was one of his lessons). In class, I will explain how I pay lots of attention to how I present myself "in the field"—whether you're a journalist or an ethnographer, one of your key goals will be to establish rapport with people. I will say that the work of ethnographers introduced me to a research posture I have embraced to this day: *The people you're interested in know the things you want to learn; make them your teachers.*

I may also address empathy, a connection ethnographers valorize that is not always taught in journalism classrooms. Traditionally, journalism has been about objectivity (or more recently, in recognition of the biases we all bring to perception, *fairness*), about "seeing both sides." That goal may make sense when the subject is politics, local controversies, social conflict. But when our aim is to more deeply understand a particular point of view or way of life, making an empathetic connection, "seeing through their eyes," is a better research posture. Immersion, a term that I use as a shorthand for participant-observation, has empathy as its goal.

While ethnography has many lessons for journalists, as I've outlined above, in other respects it does not serve as a model. In the sections that follow, I'll refer mainly to my experience with *Cheap Land Colorado* in discussing issues where I see ethnography and journalism as being in tension. I'll also include thoughts on how social media and the rise of internet search have changed the landscape for this kind of research for me and other journalists.

Social media

The people I wanted to write about in southern Colorado were mainly poor, White, and rural—but rather than tenant farmers, as in the famous account by James Agee and Walker Percy, *Let Us Now Praise Famous Men*, they are landowners: Colorado's San Luis Valley has a large number of cheap, off-grid, 5-acre lots created by sub-dividers in the 1970s, and initially sold by mail and from newspaper ads ("Buy your Colorado ranch! \$30 down and \$30 a month!"). They cost about \$3,000 - \$5,000 at creation and that has not really changed, fifty years later, such is the oversupply. Few people lived on their lots until just a few years ago, when housing in town became prohibitively expensive, solar panels got cheaper, and Colorado began to allow the cultivation of marijuana for personal use. My sister, who visited the area in connection with her work for a foundation, sent me photos of old RVs and

² To research an article for *Harper's Magazine*, I spent two months working as a USDA inspector at a Cargill Meat Solutions plant in Nebraska.

trailers and sheds serving as residences in wide open spaces. It looked like parts of Appalachia without the trees. Donald Trump had recently been elected president and, I'd heard, was popular among many of the homesteaders. I thought that getting to know them might help me get out of my urban political silo. I began to volunteer for a local group that had begun as a rural homeless shelter; they had received money (not from my sister's employer) for a "rural outreach" initiative that aimed to help the off-gridders avoid homelessness when the weather got cold—a common fate.

I ended up buying a camper trailer and renting space for it from a family that was homeschooling five daughters on the prairie, the Grubers. After about two years of regular visits, I wrote a long article for *Harper's Magazine* about my experience. Then, feeling I was just getting started, I bought my own five acres nearby (it came with an old mobile home), and wrote a book about my four-to-five years of living part-time on the prairie.

At various points during the project I picked up *Let Us Now Praise Famous Men*, which, in terms of subject matter, intended audience, and Agee's nervousness about his position as a Harvard-educated person of privilege, seemed relevant to my project.

But Agee's three tenant farmer families seemed so different from the people I was meeting! They seemed to be largely isolated, with an experience of the world limited to their immediate surrounds. In a nutshell, they reminded me of traditional peoples from the ethnographies I had read in college, different enough from the researcher and the reader that it didn't feel like a stretch to refer to them as "subjects." By contrast, the people I was getting to know in the San Luis Valley shared a lot of the same mass culture that I grew up with. Frank Gruber, the father of the family I rented trailer space from, has his back covered with Scooby Doo cartoon figures. He grew up a juvenile delinquent in Denver, a city where I had been an honor student; he was a fan of the Denver Broncos football team and Colorado Avalanche hockey team. His wife, Stacy, had lived mostly around Casper, Wyoming. She had worked as a waitress in a diner, on a ranch, and in a traveling carnival. When they had enough money they shopped at Walmart, just as most everyone in the valley did, or stopped by Little Caesar's Pizza. Their girls, when I first met them, were passionate about My Little Pony; more recently, they were very into cosplay and posting on TikTok. We were all products of American mass culture.

I learned some of Stacy's work history from her Facebook profile. And that points to another sea change in the lives of our "subjects": many or most are on social media, as am I and my friends. Smart phones, with their cameras and constant connectivity, change everything. Social media not only lets me know about *them*—in the Gruber family, mom and dad and the two oldest sisters post regularly—but it lets them know about *me*. The ramifications are, of course, momentous. I can check in on them and follow their news when I'm away. They can do the same with me.

Social media also gives them the ability to praise or criticize me—not only to their world of acquaintances but, if I have "friended" them, to mine. This has made me more careful about what I post. Though I grew up in Colorado, I am today a New Yorker, a college professor, and a journalist, three identities that are not very popular on the prairie. I try not to fuel their preconceptions by sharing political comments or memes that I know would raise

their eyebrows. I was pleased and relieved when the editors of *Harper's* used a photograph of Stacy Gruber for their cover photo—she loved it, and bragged on Facebook about her newfound status as cover girl. She and other family members expressed no qualms to me about the content of the article; if they had any, I think that cover photo, along with photos inside of other family members, would have blunted them.

About six months prior to publication, my book publisher created a web page for the book I had finished, now titled *Cheap Land Colorado*. I learned about the page when I saw that a person I didn't know had posted the link to it on a local Facebook group. Others I didn't know commented, some in the snide, skeptical tone one might imagine local people taking about the work of an Eastern college professor who had dared to write a book about their part of the country. But there were also comments by people I had met who defended me, urging others to reserve their criticisms until they had read the book.

As I write this, my book has been available in stores for five months. It is 100,000 words long, and I'm sure that some of those words will strike people in the valley the wrong way, and that they'll post about it to social media. That's life, and as a journalist in this millennium, I fully expect online criticism. What will likely matter more to me, though, is the feedback I receive from those whom I wrote about in the book. I gave free pre-publication copies to almost all of them, both to stanch the currents of rumor and gossip and as thanks for the hours and days they spent talking to me. Unlike many of the people in *Let Us Now Praise Famous Men*, my neighbors are literate. Some didn't graduate from high school, and I don't think I know any who graduated from a four-year college. But they're plenty smart and I'm sure I'll be hearing from them.

Do today's authors of academic ethnographies also expect feedback on social media? I expect that the answer is not so much, especially if they have masked the details of people and places to the point where people don't recognize themselves. Also, books from university presses are often quite expensive, and the delay between research and publication is usually longer than that in journalism or trade nonfiction, due in part to the time needed for peer review. My book, granted, lists for thirty dollars. But the audiobook costs less; a paperback will follow at perhaps half the price of the hardcover; and, most relevant, copies of unauthorized pdf's will likely become available on pirate sites within weeks of publication—if they aren't already! (I don't have the heart to look).

Privacy and anonymity

Ethnographers and journalists think differently about privacy and anonymity. Ethnographers speak of protecting the people they write about by changing their names and, often, masking other identifying characteristics, such as where they live and the names of people around them. Sometimes they go so far as to create composite characters—two or more people wrapped into one. I have heard that some institutional review boards (IRBs) may *require* that professors to anonymize in order to protect vulnerable subjects. Per federal regulation, students and professors of Journalism are exempt from IRB review (One reason I have heard for this is the delays inherent in the IRB process—most topics in

journalism are timely, and reporting can't wait weeks or months for approval).

For journalists, by contrast, accurate information about “sources,” including their real names, is expected practice. Exceptions can be made for people who are vulnerable or at risk (say, children, or dissidents living under a repressive regime). In the subcategory of investigative journalism, anonymity might be offered to people sharing sensitive information that could, for example, get them fired. But because accuracy is of paramount importance in journalism, granting pseudonyms or other forms of anonymity is discouraged, because not only does it release the person quoted from accountability for what they've said, it means there's no way to factcheck the journalist. A famous example of a journalist hiding falsity by depending on pseudonyms is Janice Cooke's article for *The Washington Post*, “Jimmy's World,” about a supposed eight-year-old heroin addict. It won a Pulitzer Prize but afterwards, when the deception was uncovered, it ended Cooke's journalism career. A freelance writer for the *New York Times Magazine*, Michael Finkel, sparked a firestorm when it was discovered he had combined interviews with several boys from Mali into a composite character for his article, “Is Youssouf Malé a Slave?” Rearranging chronology or making up quotations are similarly verboten.

The best magazines all do factchecking of their articles. Social scientists who write for them seem to adopt the practice: Matthew Desmond, whose stellar *Evicted* (2016) was excerpted before publication in *The New Yorker*, at the end of the book thanks his “obsessive and tireless fact-checker, [who] made this book better.”

Publishers of trade nonfiction books do more limited factchecking, and often it is focused on legal liability—but increasingly, authors of nonfiction books hire their own factcheckers, as Desmond did. Still, they are more accepting of pseudonyms than some magazines. I made up names for several corrections officers and prisoners in my book *Newjack: Guarding Sing Sing*, generally those I did not portray in a positive light; at the book's beginning is a list of all of those pseudonyms. In *Let Us Now Praise Famous Men*, James Agee and Walker Evans used pseudonyms for every person depicted, perhaps to protect their privacy (Evans used their real names with the same photographs that are archived in the Library of Congress).

I appreciate that the ethnographic tradition has embraced a sort of fuzziness around particular details of people and place, in the name of foregrounding ideas and analysis; the idea is that advances in social science are predicated on ethnographers' ability to typify and abstract. But I'm also aware of pushback in ethnography against masking, creating composite characters, and other forms of vagueness (Jerolmack and Murphy, 2019; Murphy et al., 2021). Before *Evicted*, Alice Goffman's *On the Run: Fugitive Life in an American City* (2014) was the last ethnographic book to make the leap into mainstream publishing. I was thrilled by *On the Run*, and on my invitation, Alice Goffman visited NYU Journalism to speak about it. Afterwards, the news that Goffman had embellished and possibly invented some of her subjects' encounters with law enforcement precipitated anguished discussions between me and my students. Though he didn't think Goffman “made up any data,” stated Columbia sociologist Shamus Khan, “I think there are

questions about reporting things she heard as if they were things she saw (which she is hardly unique in doing – most people do this, but they definitely should not).”³ Most journalists, I should add, do *not* do this.

I should here note that most of my journalism takes the form of longform articles (i.e., articles longer than 3,000 words) and books. Also I tend to write narrative, which means I may follow a set of “characters” over weeks, months, or years. In many ways, this makes me a cousin to ethnographers, who might get to know a small group of people quite well. It also introduces a tension with certain practices and tenets of traditional journalism. A reporter who is in touch with a congressional staffer, for example, is likely to explicitly discuss how the information can be shared—“off the record” means it cannot be shared, while “on background” usually means it can be shared without specific attribution (“according to a congressional source ...”). But with sources who are not sophisticated in the ways of journalism, a reporter needs to remind them that she is there doing a job. Ways of reminding such a source include writing down things they say when they say them, or referring back to something they said previously and clarifying some detail. Over time, people who start as strangers to us may also reveal information that is intimate or potentially sensitive. While a political reporter investigating wrongdoing might be excited to uncover dirt (“Republican candidate for U.S. Senate paid for girlfriend's abortion”), a journalist like me might need to suggest that a source be more cautious about sharing certain stories.

A final opportunity for sources to control what is written about them by longform journalists often comes during the factchecking process. Typically a factchecker will get in touch with a subject directly to verify the journalist's claims about them. Should a discrepancy arise, an editor might loop in the journalist to work out a solution.

After *Harper's* published my article in 2019, I was contacted by a reality television producer in Los Angeles. He told me he thought that the world I described in the article might be a good subject for a reality show—the genre is rich in series about off-gridders, gold miners, and wilderness explorers. I was not surprised by his interest, as I've been contacted by reality TV producers previously, as have other writers I know who write about subcultures: reality television has a huge appetite. But I was worried because sometimes the shows feel exploitative; and they require drama (i.e., emotional conflict between subjects); and it might expose people I had tried to portray sensitively to criticism on social media or even in old-fashioned media.

My first call, before I answered the producer, was to La Puente, the nonprofit I volunteered for. They didn't want any part of a reality TV deal. But they agreed with me that, if the prairie people I wrote about were interested, it should be their decision. There is, after all, money to be made by taking part in these shows. And so I started checking with the Grubers and others: did they want me to share their contact information?

³ Some reports suggest that Goffman *did* make up data, as here: <https://www.phillymag.com/news/2015/06/11/alice-goffman-book-philadelphia-on-the-run-criticism/>.

Almost all of them did, though some asked to discuss the matter with me first. And in these discussions, I learned that by and large they already were well aware of the devil in the details. They watched these shows; they knew what they were about. At the very least, they wanted to “hear the man out.” As for a possible loss of privacy, they didn’t seem worried. What was to be gained was perhaps money, and fame; and while they knew that getting famous could have a cost, they also knew that, for people in their circumstances, fame and money could be closely tied. Certain ones among them also saw this as a route to acquiring more social media followers, which to them was another route to fame and money.

In other words, with one or two exceptions, they were against me obscuring their identities. Most very much wanted me to use their names.

Secretive research, secretive notes

Ethnography is also in tension with investigative journalism, and particularly with undercover reporting. As mentioned, one traditional tenet of good journalism is that journalists are working in the public interest, for the general reader. That distinguishes journalism from, say, public relations, where the goal is to burnish the reputation of a client. The journalist, by contrast, aims for fairness and objectivity.

Investigative journalists strive to uncover truths that others with vested interests might prefer stay hidden. Those interests might be corporate (e.g., big oil companies don’t want to be associated with climate change) or governmental (e.g., a child protection agency might wish to avoid publicity around a missed warning or a mistaken removal). With such stories, the journalists might try to hide their true agenda from sources, in hopes of eliciting damning information. Or, in an extreme example, they might go so far as to hide their identity as a journalist in order to learn more. That is the case with undercover reporting.

Undercover reporting is controversial among journalists. Major outlets such as *The New York Times* and *The Washington Post* have policies that forbid reporters from actively misrepresenting themselves. Journalistic watchdogs who agree with this prohibition say things like, *if we know you didn’t tell the truth to your sources, how do we know you’re telling the truth to your readers?*

On the other hand, the potential power of undercover reporting is undeniable. Northern journalists secretly attended slave auctions in the antebellum South. Nellie Bly’s feigned insanity and commitment to an insane asylum for ten days prompted hearings and reform after her report was published in 1887. Upton Sinclair’s secretive visits to Chicago meatpacking houses occasioned national horror upon the publication of *The Jungle*. John Howard Griffin’s travels through the Deep South in blackface, originally commissioned by *Sepia* magazine, later collected in *Black Like Me*, opened the eyes of some White people to pervasive racial discrimination.

My book *Newjack: Guarding Sing Sing* was reported secretly, as was my article about working as a USDA meat inspector for *Harper’s*. While I was waiting to get hired by the USDA, I happened upon an academic study that was similarly researched: political scientist Timothy Pachirat’s impressive *Every Twelve*

Seconds: Industrialized Slaughter and the Politics of Sight. To research the book, Pachirat hid his identity—like me—and spent five months working in a Midwestern slaughterhouse that he doesn’t name. And in the fashion of many ethnographers, he anonymizes everyone who appears in the book. As a journalist, I kept wondering, why not share the name of the slaughterhouse that employed him? Using real names lends power to accounts of a way of life. It lets a reader know this was real, I didn’t make it up. To anonymize by default strikes me as potentially self-defeating: it disempowers a document. And in an age where the label of “fake news” is used to delegitimize media that speak truth to power, as a journalist, I think documentarians need to establish “facticity” wherever possible. Now that I’m in the academy myself, I appreciate that Pachirat was beholden to certain ethical obligations that I am not. The IRB presumably required Pachirat to ensure that he would minimize the harm that his research would produce for his subjects (e.g., the closure or sanctioning of the slaughterhouse)—no matter how morally repugnant the operators of the slaughterhouse may be. In contrast, journalists often celebrate if their work results in the powerful being held publicly accountable for their misdeeds.

Transparency, though, is tricky. In journalism, it has long been verboten to share a news story with a source prior to publication; the reason is that the source may ask for changes that accord with their interests but not with readers’. Fact-checkers are typically instructed not to read sentences to a source verbatim but rather to paraphrase them. However, when it comes to preparing longform articles reported through immersion, the rules can be relaxed, particularly when the story is about a vulnerable or underprivileged person. I know of two celebrated works of trade nonfiction about indigenous Americans published in recent years where the (White) writers were allowed to go over the manuscript word-for-word with their subjects, the thinking being that the subjects were particularly vulnerable, and traditional fact-checking might be less likely to unearth errors than with other kinds of work.

I’m intrigued by newer models of transparency proposed in recent years by ethnographers such as Reyes (2018), who discusses the issues involved in sharing data (e.g., interview transcripts and fieldnotes). I can see the benefits in terms of reproducibility, awareness of reflexivity, comparison over time, and, as she says, general facticity. But I have never heard a similar proposal seriously advanced for journalists or other writers of longform nonfiction. I can imagine many good reasons why a journalist would demur, including second-guessing by readers (“Why did you use that quotation instead of the other one?” “Why did you talk to Person X but not Person Y?” “Your notes reveal a complete failure to comprehend the underlying issues.”). Journalists, probably myself included, would prefer to be judged by the work itself. While we have an ethical obligation to be as judicious and fully-informed as possible, there is no presumption in journalism that one article will *build upon* previous work in an explicit way such as there is in social science/academic writing, nor that the next journalist to visit a town/organization/community should leave behind a scaffolding of primary data. I picture an inventive chef saying, *judge me by the dish, not by the recipe*. In contrast, it seems that ethnography’s positioning as a social science makes it more vulnerable to demands for “data transparency” than journalism.

Some 50 years after James Agee and Walker Evans went south to report *Let Us Now Praise Famous Men*, journalist Dale Maharidge and photographer Michael Williamson located the descendants of the sharecroppers described in that classic work and wrote an update of a sort unusual in journalism. Their postscript to Agee and Evans' work, *And Their Children After Them*, won the Pulitzer Prize for General Nonfiction in 1990. Maharidge then became a tenured professor at the Columbia Graduate School of Journalism. I've always been impressed by their achievement. But in journalism, such explicit nods to the greats are rare.

Audience

Mainstream journalists aim to reach a wide audience of general readers. Those working in the digital sphere may measure impact by page views and clicks; ad sales; and subscriptions. The coin of the realm is breaking news, what's happening now. Longform narrative writing, or literary nonfiction, as it's sometimes called, is practiced by people who may as readily call themselves writers as journalists. We are interested in telling in-depth stories that relate to what is happening in global culture but we are a few degrees separated from news of the day. Our stories might be historical or biographical; they might be investigative and political; they may speak to cultural trends, such as populism or #MeToo or the reparations movement. I think it is accurate to say longform writers have aspirations of creating work that lasts beyond the current news cycle. And many hope also to create writing that is graceful and beautiful—not simply a “just the facts” account but stylistically appealing. Some of us have connections to universities but most do not.

Ethnographers, by contrast, exist mainly in universities. And while sometimes their work may find a readership beyond the academy (Margaret Mead comes to mind, and Claude Levi-Strauss), most write for other social scientists. Data collection and analysis are prioritized. The use of specialized language is characteristic of much ethnographic writing, which is often in the service of building theories, comparing across cases, or identifying mechanisms—things that are usually not central to journalists and other nonfiction writers.

By contrast, I teach my students to tell stories—stories that relate to a bigger idea or topic, be it undocumented immigration, the incarceration boom, or the rise of Donald Trump—but stories just the same, because a main goal is to engage the general reader in a piece of longform writing. To get them in the spirit, I suggest an exercise that may strike an ethnographer as vampirish: read your chosen excerpt from Duneier's *The Urban Ethnography Reader* closely, and consider reading the longer work it was taken from. Then answer this question: if you had the access to people that X ethnographer did, can you imagine reporting out a more journalistic piece of writing—one more accessible to a general reader? Is there a character or two they might get to know? Is there a scene or two they could imagine including in a narrative account? Are there ways you could spend time with these people and *report for story*? In the next class, I try leading a further discussion into what might be lost in a narrative account, which ideas might fall by the wayside, and the way that the costs of simplification must be weighed against the benefits.

A note on “subjects”

In a time when even people in the humblest of circumstances use smart phones and publish on social media, when questions of privilege and representation are high on the public agenda and certain traditions of journalistic and ethnographic practice are being reevaluated, I'm wondering: Should ethnographers still use the term “informant?” I imagine, I hope, that it is going away, because of its hint of snitching for cops and its suggestion that “the significance of this person is that they tell me things.” A cognate term in journalism is “sources.” It again connotes “the person who gave me my information” but to my ear it is similarly othering. It suggests an extractive research posture. Another term, “subjects,” feels more benign but is still troubling, because it says, *I am the studier, you are the studied*.

Matt Little, the Rural Outreach Initiative worker whom I shadowed when I first got involved with La Puente, and later emulated, had a similar problem with the terminology of his workplace. La Puente is a social service organization that typically refers to the people it helps as “clients” (Clients who stay or eat at the shelter might also be called “guests”). Matt, who lived out on the prairie in a humble trailer like the people he tried to assist, actively resisted describing his work as “serving clients.” “It's not a restaurant,” he explained. Rather, he said, he felt like he was helping out his neighbors. More often, he would refer to helping out his “people” (“One of my people could use some firewood”). He also didn't like the positionality of the “serving clients” approach, because to him it connoted something close to charity. Rather, he liked to think of the help he rendered as mutual aid, neighbors helping neighbors.

Hanging around Matt and La Puente, I was fascinated by a value they seemed to share: both were against “judging.” You could never know why someone had ended up in the tough situation they had—maybe they were careless or lazy or codependent but really it didn't matter. Circumstances had conspired against them. You couldn't know and you shouldn't judge. What mattered was, they needed help. Even more impressive was when the people receiving help were angry, or dismissive, or even hateful of you or others: Matt and the La Puente workers didn't shout back. Privately, you might occasionally hear a cavil or reservation about a client: *She's too ask-y*, I heard one manager say about a woman who did, indeed, ask for help month after month after month. But seldom did they condemn or write off anyone, and never did I witness them doing that publicly. They didn't judge.

But what about me? Wasn't it my job as an educated person to exercise critical intelligence—to analyze and even theorize?

I think my peers and I would say here that, more than analysis, we depend on the power of story to carry meaning. Like ethnographers, we pick people to write about who stand for something larger. It might be unofficial immigration, the self-isolation of the rich, the boom in incarceration, the anger of working-class Whites. Our research, then, is in service to telling a story, and the story has a point of view. Meaning is conveyed by tone and by what we include or ignore; and in my case, by the situations I put myself in and then recount. The idea is always there

but particularity, and true detail—what actually happened—take center stage.

Ethics statement

The author's works discussed in this article were researched in accordance with professional standards and institutional requirements. All identifying information relating to the research participants has been previously published, with consent from the individuals involved.

Author contributions

The author confirms being the sole contributor of this work and has approved it for publication.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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OPEN ACCESS

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RECEIVED 08 December 2022

ACCEPTED 02 May 2023

PUBLISHED 18 May 2023

CITATION

Cera M (2023) Digital ethnography: ethics
through the case of QAnon.
Front. Sociol. 8:1119531.
doi: 10.3389/fsoc.2023.1119531

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Digital ethnography: ethics through the case of QAnon

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Introduction: Digital ethnography is a relatively new practice with unclear standards and guidelines. As a result, the ethics of the practice remain unclear. Scholarly debates have emerged surrounding the decision of many researchers and institutional review boards to treat social media data as public. Concerns have also been raised about how informed consent can be adapted to online fieldwork. How does a researcher make their presence known when they are not visible in the traditional sense? Which online interactions should be considered public, and which are private? How can we protect the anonymity of social media users?

Methods: This article leverages original digital ethnographic research on QAnon social media spaces to suggest ethical guidelines for digital ethnographic practices.

Discussion: It begins with a description of the research, followed by discussions of the public-private binary, lurking, data reconstruction, and institutional review boards.

Results: This article advocates for rethinking the public-private binary as it applies to the digital world, ameliorating the “lurker” concern by making the presence of the researcher known in appropriate spaces, and maintaining the integrity of the data by avoiding reconstruction. Although many digital ethnographers have chosen to reconstruct or paraphrase online data to protect privacy, this practice comes with its own ethical dilemmas. The ethical dilemmas and guidance discussed in this article are critical lessons for digital and in-person ethnographers alike.

KEYWORDS

digital ethnography, ethics, QAnon, privacy, lurking, data fabrication

1. Introduction

Digital ethnography, sometimes referred to as virtual ethnography, ethnography of cyberspace, and online ethnography, is a relatively new set of methods within the social sciences. It typically adapts offline methods such as participant observation and interviews to online space. This new terrain poses important methodological and ethical challenges. For example, how and from whom should informed consent be obtained in social media groups with thousands of users? When can a researcher reasonably assume a post is public? How should we think about privacy, anonymity, and the prevention of harm online?

This article focuses on three debates present in existing scholarship on digital ethnography: the public-private binary, data fabrication, and the “lurker” concern. Diverse perspectives and varying interpretations characterize each topic. Researchers continue to struggle with when to classify data collected online as public or private. Their decisions have implications for the wellbeing of participants. Many scholars have suggested data fabrication, or in other words paraphrasing easily searchable data collected online, to protect privacy and anonymity. Relatedly, because digital ethnographers are not visible in the traditional sense, they need to be careful to avoid “lurking” in digital spaces. They must decide when and how to make their presence known and participate in interactions. Additionally, this article considers the role of institutional review boards in evaluating digital ethnographies. I use the

case of QAnon to suggest these dilemmas can largely be ameliorated through transparency, attention to context, the use of real data, and open discussion of the research process rather than covert methods.

After a discussion of QAnon and the specifics of my digital ethnography, I divide the article into four sections. First, I think through the public-private debate and argue this binary should be considered a spectrum based on barriers to accessing digital spaces. The next section addresses lurking and how to ensure active participation and transparency. I then show how data reconstruction (or “fabrication”) poses its own ethical challenges and suggest alternatives. Finally, I explore my own experiences with institutional review boards and offer suggestions for digital ethnography proposals. All four sections leverage my research on QAnon to address the complexities of each debate.

Although this article reflects on ethical issues relevant to digital ethnography, it addresses age-old debates about in-person ethnographies as well. Duneier et al. (2014) emphasize enduring questions about urban ethnography that can be adapted to online space: *How* did the researcher develop their relationship with their subjects? What is the nature of their relationships, and how should they be managed? What is the standpoint of the observer? How can we be more open about the research process? My suggestions for ethical practice should be thought of in relation to the enterprise of ethnography itself. I draw on two perennial issues in ethnography: the ethnographic relationship and the influence of the local. Traditional ethnography is defined by the relationship between the researcher and the researched. It has also always been rooted in local contexts. Accordingly, digital ethnographers should incorporate and reflect on the importance of their relationships and the particularities of their case and contexts as part of ethical practice.

Further, QAnon is a useful case through which to think through general ethical issues with digital ethnography. The extreme nature of the group makes it particularly necessary to develop and uphold strict ethical standards. For example, QAnons often use violent or otherwise disturbing rhetoric. This forces the researcher to grapple with when and how to reproduce data in publication. It also means researchers need to be more acutely aware of their own safety within digital spaces. QAnons are also largely anonymous, complicating the kind of relationships which can develop with the researcher as well as increasing expectations of privacy. The nature of the group opens up pressing ethical questions which might not have surfaced in an investigation of more moderate groups.

2. A digital ethnography of QAnon

QAnon emerged on October 28th, 2017, when an anonymous account called “Q” posted a cryptic message about an oncoming “storm” on the social media site 4chan. Q and QAnons have since been responsible for the production and spread of conspiracy theories throughout social media, including the idea that a satanic cabal of lizard-like politicians control politics, media, and other institutions. It has also been linked to offline events such as January 6th and the “Pizzagate” shooting in Washington D.C. Among the American electorate, roughly 17% agree with the QAnon conspiracy theory that a satanic group of elites run a sex trafficking ring and control politics and media (Staff, 2021), 7%

have a favorable view of QAnon overall (Schaffner, 2020), and 8% of registered Republican voters identify as a QAnon “supporter” (Civics, 2020).

I began a digital ethnography of QAnon in January of 2022. My research design was inspired by scholars developing the craft (see, for example, Boyd, 2014; Markham, 2017; Lane, 2019; Stuart, 2020). Boyd (2016) suggests digital ethnography is much like traditional ethnography, in that it should include deep immersion in your field site(s), participant observation, content analysis, and semi-structured interviews. Although the research was conducted in a largely inductive manner, I approached my field sites with several questions: What repertoires, logics, and practices makeup QAnon political participation? How should we classify QAnon as a group, and therefore understand their activity as a whole? How do QAnons move through social media space? My goal was to understand the historical and technological specificity of QAnons’ participation in politics.

Field site selection was the first difficult task. Boyd (2008) and Beneito-Montagut et al. (2017) both argue that digital ethnography requires rethinking what counts as a field site by moving beyond a bounded sense of place. Social groups exist within and between multiple platforms and groups simultaneously. I began the process by directly messaging users on various platforms I knew were home to QAnons and other far-right groups, such as MeWe, 4chan, and Telegram. I asked them which platforms they prefer to use, as well as where they see most QAnon activity occurring. I then Zoom-interviewed a QAnon member with an outsized presence online.¹ “Operation Q,” as he calls himself, suggested MeWe, Telegram, the chans (4chan and 8kun), and Telegram. I continued to ask about platform preference in video-chats and direct messages with other QAnons, finally settling on five major platforms: Gab, Telegram, 4chan, 8kun, and MeWe. I selected three groups within each platform based on their size (excluding groups with fewer than 10,000 members), frequency of activity, and of course affiliation with QAnon.

Deep immersion in these digital communities involved behaving “in the same manner as my informants” (Bluteau, 2019, p. 268). I learned the norms and unique subcultures of each space to the best of my ability. Although many researchers choose to hide their identity when conducting research on extreme groups, I decided to use my real name, photos, participate with my real perspectives, and disclose my position as a researcher. I let participants know I was conducting research on their political participation. I felt this was an important ethical practice. After the first few weeks, I understood the protocol of each space well enough to participate more regularly. I began to comment on posts with my own thoughts and questions. I posted links to articles on relevant current events.

Content analysis involved pulling the “top” posts (most likes, comments, etc.) from each group every time I logged on. The goal of content analysis was to see what kinds of topics are discussed, how they are discussed, who discusses what, what content is particularly popular, and the emotional tone of the posts.

¹ Operation Q has accounts on 38 different social media platforms and a following of hundreds of thousands.

Interviews² were conducted over Zoom, FaceTime, email, and direct messages. Oftentimes, interviews would include screensharing with willing participants who would walk me through their daily social media activity, an approach suggested by Ardévol and Gómez-Cruz (2012) as well as Light et al. (2018). Participants who were particularly concerned about their anonymity decided to message back and forth with me rather than disclose their identity through phone calls or video chats. These “interviews” were essentially ongoing, as we continued to go back and forth for weeks even after we had signed off for the day. Interviews were conducted primarily to understand meaning-making behind interactions I had observed as well as how participants viewed their own activities and purposes.

3. Public vs. private

Matzner and Ochs (2017) remind us that debates about what counts as private and public go as far back as Ancient Greece. Ethnographers have long grappled with participation in public and private spaces, especially given deviance, social disorganization, and suffering are often the focus of ethnographic research (Katz, 1997). Traditional ethnographers have had to balance making their presence known as a matter of ethics while trying to get “behind the scenes” perspectives. Howard Becker has written extensively on sociologists balancing public and private spaces. He believed sociologists of his time still had no consensus as to what data can and should be made available to the public (Becker, 1974).

Today, those interested in digital ethnography typically look to the Association of Internet Researchers (AoIR, 2020). The AoIR has become industry standard. The international organization provides regularly updated guidelines for scholars who conduct research on the internet. The third edition of the guidelines, published in 2019, focuses on three primary issues: ethical pluralism, informed consent, and privacy. Similar to Nissenbaum (2011) and Eynon et al. (2016), the AoIR recommends treating ethics as a case-by-case approach to take into account the values and perspectives of other cultures. Informed consent, and ethical considerations overall, should be considered a process rather than a box to check at the beginning of the research process. Some researchers may choose to apply pseudonyms, some delete identifiable information, some avoid asking sensitive questions, and some only ask for consent at the dissemination stage. All decisions need to be contextual and relational. Privacy is discussed in relation to publicity, and the guidelines suggest the greater the acknowledgment of publicity, the less need there is for anonymity and confidentiality. However, the guidelines do not specify how a researcher determines the acknowledgment of publicity.

Internet researchers now have to contend with perennial public vs. private debates as they apply to our online social worlds. Digital research often heightens the risks of privacy violations (Marres, 2017). Approaches vary significantly both within and between disciplines. While some argue everything on the internet is public and can therefore be used by researchers without

informed consent, others take special care to inform each and every participant about the use of their data.

Kitchin (2003) takes the former approach. She reflects on two important issues relevant to digital ethnography: (1) What constitutes public and private on the internet? and (2) Should internet participants expect confidentiality and anonymity? She suggests that because the internet is by definition a public space, participants cannot expect confidentiality or anonymity. Accordingly, informed consent is unnecessary. Sugiura et al. (2017) ask similar questions. Should researchers use publicly available data on the internet without informed consent? Is informed consent feasible online? How can anonymity be preserved? They review several organizations whose purpose is to provide ethical guidelines. The Marketing Research Association (2010) guide to social media research suggests most participants understand their conversations are public, but most do not expect their data to be viewable to researchers. Yet they maintain informed consent is not needed because of the public nature of the interactions online. The Council of American Survey Research Organizations (CASRO, 2011)'s social media guidelines state informed consent should be obtained when research participants directly interact with researchers. They do not specify what constitutes direct interaction.

Boyd (2008) finds her participants struggle with defining public and private. There are cultural differences in how individuals and groups understand privacy, and individual-centered notions of privacy obscure the contextually-mediated nature of digital spaces as well as the role of technical affordances. Marwick and Boyd (2014) refer to this concept as “networked privacy.” Nissenbaum (2011) also argues privacy is contextually mediated: local expectations and norms dictate expectations of privacy. Although individuals might post on a public site, they might not understand their content is public. Ultimately, we need to be diligent about how participants define public and private (Boellstorff et al., 2012). These arguments can and have been applied to other ethical considerations, and many have argued for sensitivity to cultural differences throughout the whole research process (Hongladarom, 2017; Hutchinson et al., 2017; Luka et al., 2017; Weller and Kinder-Kurlanda, 2017).

Yet researchers still need to develop working definitions of public and private to ensure the protection of human subjects online. Boyd (2008, p. 21) ultimately defines public as: “A space where people may gather, interact, and be viewed and also an imagined community of people who share similar practices, identities, and cultural understandings. That which is public is potentially but not necessarily visible.” She highlights that public spaces might not be visible, but then how should researchers approach the more invisible spaces? Further, her definition speaks to a broad range of online spaces, such that groups, newsfeeds, direct messages, and more can be considered public. Some differentiation is necessary to guide ethical practice.

This differentiation need not be binary (e.g., all of x is public, and all of y is private). For example, Reilly and Trevisan (2016) call facebook “semi-public.” Eynon et al. (2008) refer to some spaces as “in between.” There is no clear resolution as to what is public and what is private online. What questions can we ask ourselves, and what guidelines can we follow to prevent harm to participants?

² Interview data is not included in the findings, but I mention the process to give the reader a better sense of the methods within digital ethnography.

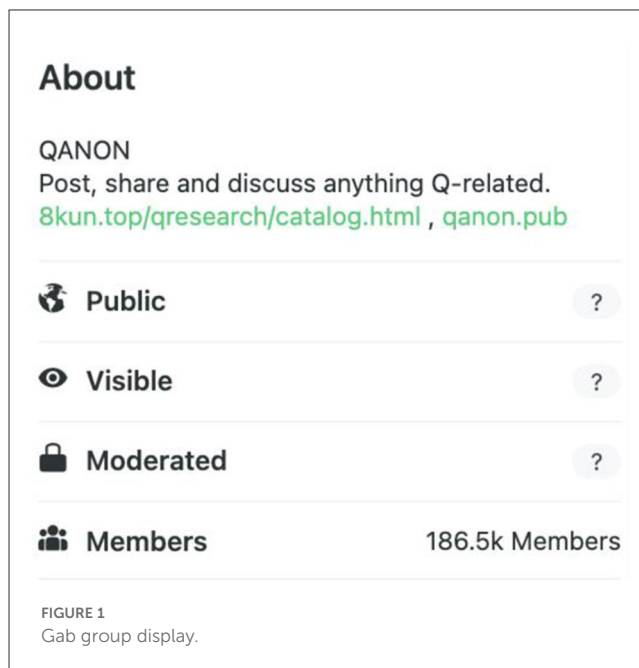
3.1. QAnon and the public vs. private debate

It is increasingly difficult to distinguish between public and private on social media as platforms develop new features and ways of communicating. Some QAnon researchers approach this debate by only using data publicly available to anyone with internet access (see Papasavva et al., 2020; Hanley et al., 2022; Kim and Kim, 2022). Others involve direct interaction with QAnons through interviews and surveys, and therefore necessarily use private information (see, for example, Garry et al., 2021). Researchers may even choose to use individual “user bios” which include personal information about gender, race, and other identifying factors (see Bär et al., 2022). To further complicate the matter, expectations and understandings of privacy vary not only nationally and culturally, but also by social media subculture. My research on QAnon has made it clear that public and private should not be treated as binaries, but rather as a spectrum.

QAnon presents an interesting case through which to think through issues of privacy. Their group is built around anonymity and secrecy. The supposed head of QAnon, Q, is unknown to their followers. Nearly all accounts following Q-related groups on 4chan, 8kun, Gab, and Parler (four of the most frequented sites by QAnon adherents) are anonymous. For example, three regular posters in the “QAnon” group on Gab are @TruthRevealer17, @TheStormIsReal, and @TheBigVirusHoax. All of their posts and profiles are viewable by all users of Gab. Moreover, each platform signals different levels of privacy. “QAnon” on Gab displays the following at the top of the group (Figure 1). If you mouse over the question mark next to “Public,” Gab indicates, “Anyone on or off Gab can view group posts.” If you mouse over the question mark next to “Visible,” it tells you “Anyone can find this group in search and other places on Gab.” Private groups must be requested to join and users are accepted or denied by administrators. They are labeled “Private,” and the question mark indicates, “Only members can see group posts.”

MeWe groups signal privacy in a different way. While many groups are public, some require users to fill out a questionnaire which is reviewed by administrators. Figure 2 below is a questionnaire issued by a private, far-right MeWe group known for promoting QAnon conspiracy theories. Questionnaires like these came up with about half of the QAnon groups I have joined on MeWe. They act as barriers to entry and signal to others that their content is not meant for public consumption. Yet once I filled out the questionnaire and told them I do not in fact support Donald Trump, I was still allowed in the group. Is the group then semi-public? Mostly private?

I now have accounts on dozens of QAnon-frequented platforms, including Telegram, Parler, MeWe, 4chan, 8kun, and Gab. Each platform offers different forms of privacy. Moreover, each platform offers different ways of communicating with each other. Some support private messages, some have chat rooms, some have groups, others are based on hashtags, and on. These features vary not only by platform but within platforms, and are constantly adjusted by companies seeking to make the most profitable products for users. Much like offline ethnographers



suggest, local context matters. Therefore, it makes little sense to universalize a definition of public and private online.

Instead, we should treat public and private as falling on a spectrum. This directly contradicts advice from Kitchin (2003, 2007) who suggests all content found on the internet is by nature public. Instead, it aligns with the work of communication scholars such as Scott (2013) who argues classification frameworks should be oriented away from binaries. Concepts such as identity, transparency, and collectivity fall on a spectrum. Nearly all social media spaces will fall somewhere in between public and private. The spectrum should vary based on barriers to access (such as questionnaires discussed above). In other words, the more difficult it is to access a social media space, the more private it becomes. If anyone with an internet access and account can access a space, it falls more on the public side of the spectrum. The more difficult a space is to access and therefore the closer it is to being “private,” the higher our ethical standards need to be. I have not pulled any data from the group requiring a questionnaire without directly messaging users and obtaining informed consent. I have never published direct messages with participants without informed consent. Small social media groups with <50 members should always be informed of the presence of a researcher. I feel it unnecessary to obtain informed consent for posts made by celebrities in more public groups or feeds. And for groups with hundreds of thousands of members, I typically feel it unnecessary to inform each member of my position as a researcher. “Walking through” social media apps with participants, as discussed in the methods section, can also be useful in determining how public or private participants consider their activity. Direct discussion with participants about privacy concerns strengthens the relationships that are necessary to produce accurate and meaningful portrayals of their lives, and these relationships and discussions are equally important in determining ethical practice.

The case of QAnon offers insights that can contribute to the resolution of current debates within digital research. The public

You are joining as Michelle Cera

Please answer the following question(s) to join this group:

do you support Donald Trump?

Your answer

If you are Trump Supporter invite your friends to join this group and let's get 1 million people in here, we believe that if each one of us invite atleast 10 people this will work.

Your answer

Follow our page on MEWE if you want : [REDACTED]

Your answer

We are trying to unite the America 1st movement but have trouble here on mewe. So were r on GAB as well join us better platform for this [REDACTED]

Your answer

Join

■ Report this group

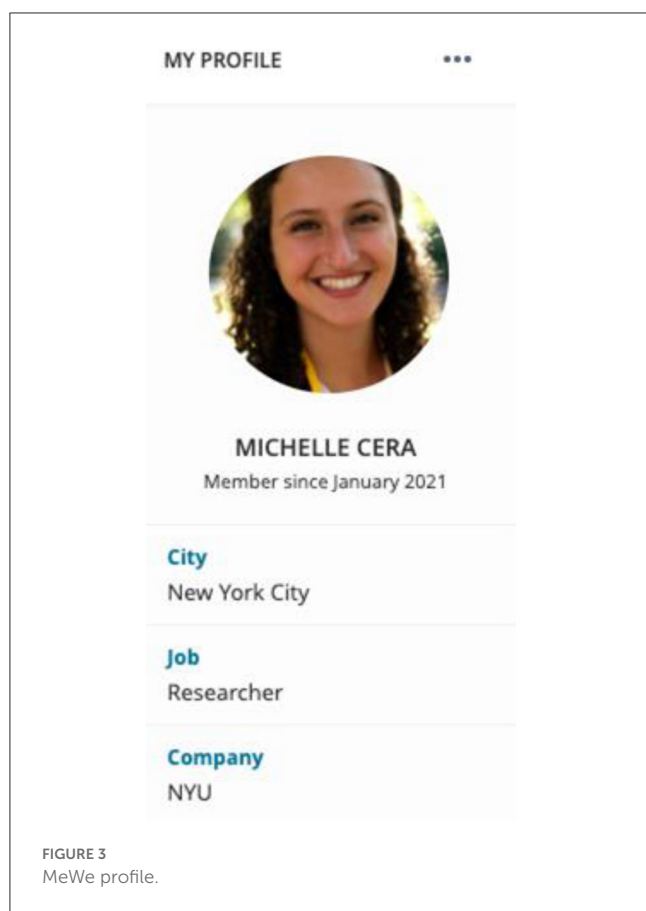
FIGURE 2
MeWe questionnaire.

vs. private debate is ongoing, and some researchers suggest all content on the internet is by definition public. Others suggest thinking of privacy on a case by case basis. Researchers have not developed guidelines for how to determine exactly what is public online. Some argue informed consent is not needed in internet research because of the public nature of interactions, others emphasize cultural variations in expectations of privacy. Eynon et al. (2008) and Reilly and Trevisan (2016) somewhat hint at a privacy spectrum with phrases such as “semi-public,” but this argument has not been fully fleshed out. My research on QAnon has led me to think of publicity and privacy not as a binary but as a spectrum based on barriers to access. Rather than using a universal definition of public, researchers might think about how difficult a digital space is to access to determine the nature of the space. Platform context is key here, as different platforms offer different barriers and indications of privacy. The following questions are useful: How many members are within the space? How many “private” signals are present? How many barriers to access are present? Is there a way to determine user expectations of privacy? These questions have proven useful in guiding my own

practice within QAnon spaces, and will hopefully be of use to other researchers.

4. Lurking

Lurking, a practice in which researchers observe social worlds and extract data without informing participants of their presence or use of their data, has plagued online and offline ethnographers alike. Some offline ethnographers believe covert observation can be beneficial to data collection and interpretation, especially in cases where the communities are difficult to access or vulnerable (Ellis, 1995; Becker, 1998; Humphreys, 2017). On the other hand, Lareau (2003) argues relationship building as well as the development of trust and comfort are crucial to ethnographic practices. Lurking by definition prevents meaningful relationships, trust, and comfort. Some offline ethnographers have approached these issues by involving participants more directly in their research processes. For example, Venkatesh (2002) had his participants interpret his research practices and narratives in informative ways.



Like offline ethnographers, lurking has been a central concern of digital ethnographers for some time. Researchers can enter digital worlds easily and without notice, as many social media platforms afford anonymity. Varis (2014) suggests the data gained by lurking cannot be considered digital ethnography, as the “participant” aspect of participant observation is missing. Part of the principles of informed consent is ensuring people comprehend what ethnography is and what researchers are doing, beyond a simple form (Boellstorff et al., 2012). Scholars have begun to address these issues and how they can be avoided. Kaufmann and Tzanetakis (2020) argue permanent connectivity of spaces on the internet allows the researcher to always be co-present. This first-hand, in-the-moment experience is crucial for qualitative research. Digital spaces also allow you to talk to people you might otherwise not be able to talk to, and they might divulge things they would not in person. Yet they note this type of research becomes complicated when studying vulnerable populations. They avoid lurking by making participants “co-researchers,” or in other words allowing them to make sense of their own data when possible. Marres (2017) make an important argument on this subject: digital platforms make it easier for researchers to involve their participants throughout the entire process. Platforms afford new and plentiful forms of interaction with participants, enabling research processes based heavily on *exchange*.

Eynon et al. (2008) specifically discuss observation of online communities. Strategies they suggest include approaching key stakeholders of groups to ask permission and visibly labeling yourself as a researcher (such as in your profile or username). They

also suggest being explicit about ethical dilemmas and decisions in publications. Boellstorff et al. (2012, p. 142) also recommends being honest and upfront about identity, as “undercover observation... runs counter to the heart and soul of ethnography.”

Hine (2000) was one of the first to problematize digital lurking. She later broadened the range of activities which should be considered participation, including browsing, following links, and moving between platforms (Hine, 2007). de Seta (2020, p. 87) introduces the notion of “participatory modalities” to undermine the seriousness of lurking. He argues participation can vary from non-use to active presence, none of which should be considered lurking.

On the other hand, lurking may be beneficial for the quality of the data obtained by the researcher. Grincheva (2017) acknowledges that lurking is a major issue but contends the practice allows you to observe participants in a more natural setting. While a researcher in offline settings has no choice but to affect the interactions they observe, the online researcher can observe interactions as they would take place in their absence. Grincheva still cautions against lurking unless participants understand their activity is accessible to the whole internet and not just their group.

4.1. QAnon and the lurking debate

Lurking has been defined differently by various researchers, but the principal concern is that social media study participants are unaware they are being observed and uninformed as to the use of their data. QAnons can be considered a vulnerable population given their status as an extremist group, potential for violent or unpredictable behavior, and the risk of losing employment or social ties should their beliefs be disclosed. Therefore, I find it particularly important to avoid lurking in their digital spaces. While I do not endorse their extreme views, I maintain that ethical standards must be applied universally.

Perhaps most controversially, I use my real name and photo in all of the QAnon spaces I study. Most QAnon researchers choose to hide their identity, yet some choose to disclose their position as a researcher and are candid about their beliefs (see Forberg, 2022). My research participants deserve to know my real identity and purposes, regardless of their discriminatory political views or the fact that they are typically anonymous themselves. Transparency helps build relationships which are crucial to ethical and accurate research. It can be exploitative to mislead study participants on your intentions. I grant all participants the same respect I expect to receive in return. I cannot expect participants to be forthcoming on the various personal and political discussions we have if the premise of the research is dishonest. I have often found study participants appreciative of my honesty and sometimes more willing to speak with me candidly in interviews.

Figure 3 is a screenshot of what my profile looks like on MeWe. I use a real photo, my real name, the city I live in, and indicate my position as a researcher and my institutional affiliation. Anyone who navigates to my profile can easily view these credentials because my profile is public. Further, I inform study participants of my research intentions in direct messages, group chats, questionnaires, and interviews. QAnons are particularly

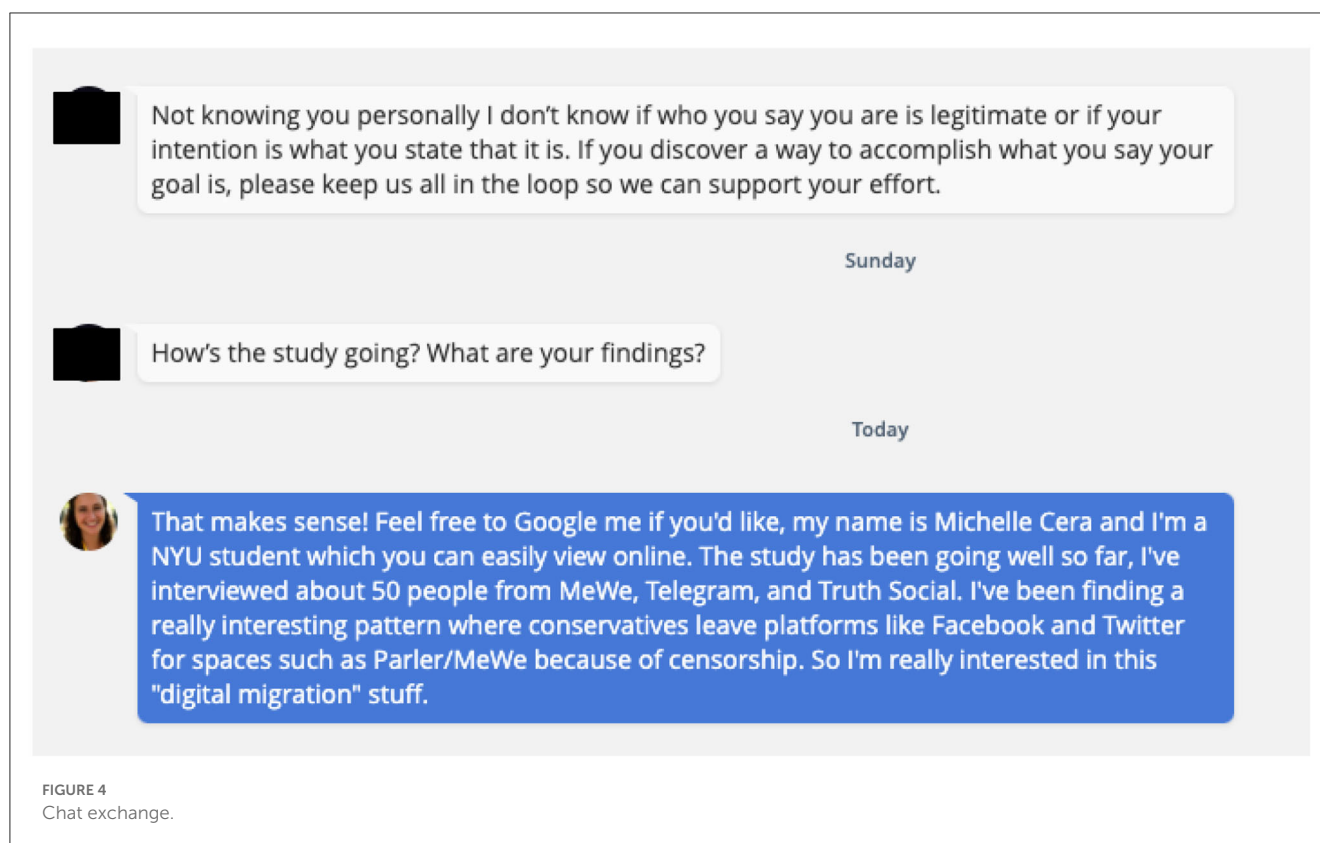


FIGURE 4
Chat exchange.

active in direct messages as one of their main goals is to spread information. For example, on MeWe, I receive upwards of 50 messages per day in direct messages or in group chats of spaces such as “WWG1WGA.” Whenever a QAnon reaches out to me personally or when I reach out to ask questions or request an interview, my first message indicates my name and my research intentions. I find I am able to get quality data not by hiding my identity but by being forthcoming.

The chat exchange below (Figure 4) is typical of my conversations with QAnons. Most users are immediately suspicious of me for living in a liberal city and being affiliated with academia, an institution most of them despise. After receiving a request for an interview in a direct message, Susan (a pseudonym) tells me she has reason to be skeptical of my identity and intentions. However, she indicates support for my research. I respond by suggesting she Google me to confirm my identity, and discuss one of my findings honestly. Susan then agreed to be interviewed at a later date. My interactions with Susan underscore the power of transparency in digital research.

Lurking should be especially avoided within small groups of up to 50 users. In my experience, this tends to be the cut off at which members stop recognizing most other members. In small groups, users tend to feel safe to discuss personal views and information. I have often come across discussions of drug use, marital issues, and health concerns in small groups. Users are also more likely to get to know each other, unlike in groups of tens or even hundreds of thousands of users. Small groups typically have more targeted purposes for select users in comparison to large groups which tend to be curated for public audiences. Therefore, small groups

fall farther on the “private” side of the spectrum. Accordingly, researchers should take into account the size of the group when deciding how to approach participants and observe their spaces. I tend to announce my presence in a post viewable to all members of small groups. I do not do this in larger groups as the likelihood all members view my post or care is lower.

Avoiding lurking is also about understanding how to ensure the “participant” part of participant observation. [de Seta's \(2020\)](#) participatory modalities underscore the various ways in which a researcher can be active in the communities they study. While announcing one's presence and intentions is a first step, participation is a continuous practice. Commenting, liking (or disliking) content, posting text, articles, or memes, and messaging users are all forms of participation. Another way to meaningfully participate in the digital social worlds we study is to engage participants more deeply in the research process, from developing questions to data interpretation to publication.

The key to digital and offline participant observation is to engage in a similar manner as other users within a space. This requires learning the norms and practices of each platform and group. For example, [Marwick and Partin \(2022\)](#) engage in what they call “deep hanging out,” a form of observation where researchers monitor and spend extended periods of time in a group. [Forberg \(2022\)](#) similarly consumes content within groups for long periods of time, but also conducts interviews with participants. [Conner and MacMurray \(2022\)](#) primarily rely on content analysis and video observation.

The lurking debate endures. While many researchers believe lurking might lend itself to higher quality data, others argue

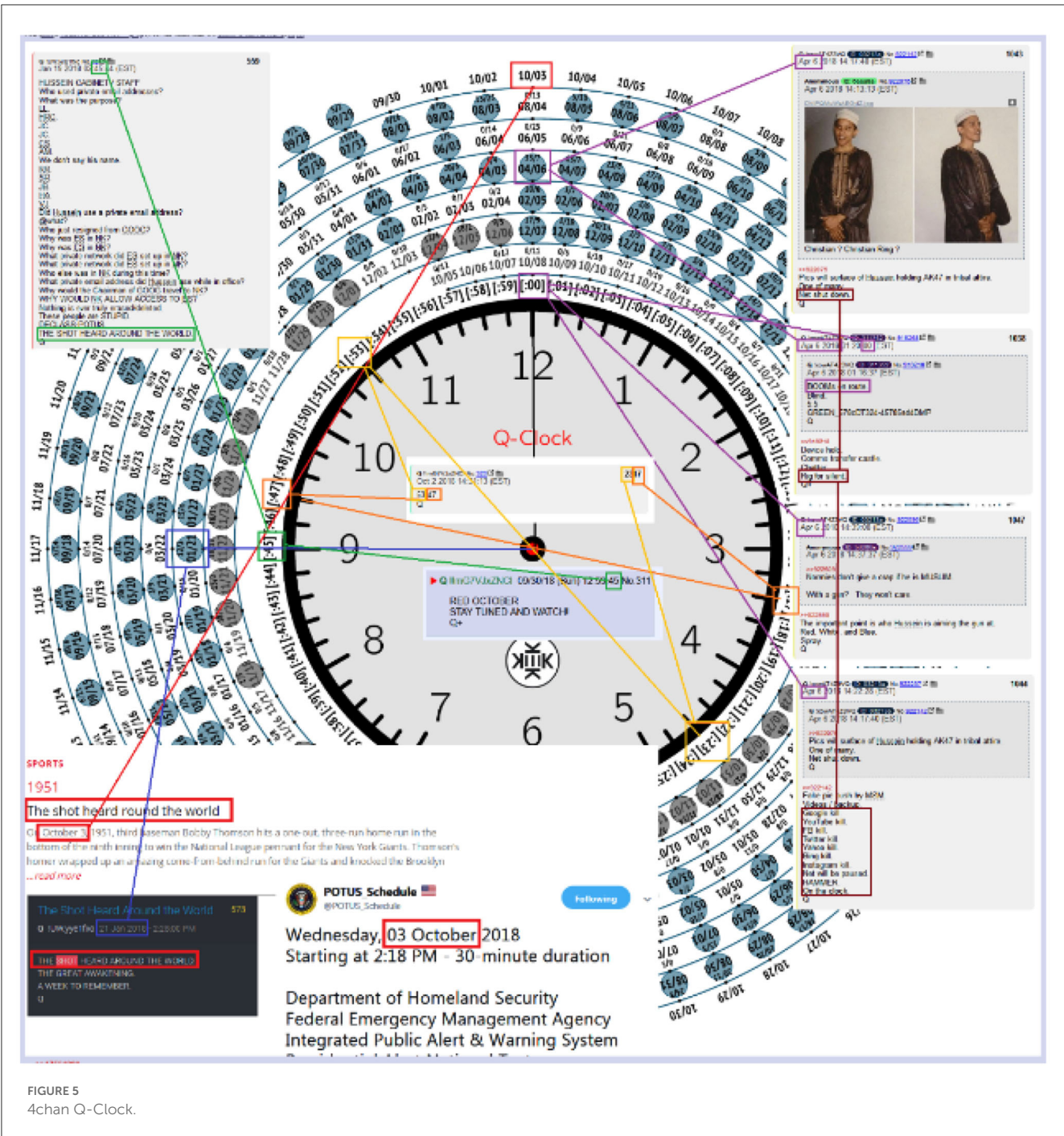


FIGURE 5
4chan Q-Clock.


Anonymous (ID: LjYQ39gT) 

FIGURE 6
4chan user identification.

lurking goes against the very definition of ethnography. Very few researchers have chosen to use their real identities, especially within extremist groups. I argue lurking is contrary to the methods of

ethnography itself. I am candid about my identity because it is an important ethical practice for all researchers, but I have also found it useful in developing trust and relationships with participants. Much

like the private and public spectrum, the need for transparency should increase as barriers to entry increase. The more difficult it is to enter a space or the smaller it is, the higher the need to identify yourself as a researcher. When ethnographers lurk, they lose the richness of relationships that are crucial to the endeavor of the method. They misrepresent their purposes and in doing so leave room for the distortion of their findings, relationships, and interpretations. I argue researchers should use their real names and indicate their positions when appropriate, participate in multiple ways, engage in a manner similar to other users in the space, and be forthcoming about the purpose of their studies.

5. Data fabrication

Data fabrication is the process by which researchers paraphrase or change the original speech or text produced by the study participant to avoid the participant being traced upon publication. Although digital ethnography has resurfaced these issues, offline ethnographers have always had to contend with the fair and accurate presentation of their data. Decision-making in regards to who should be quoted, when, and how has always affected offline ethnographers. Several notable ethnographers have written extensively about the challenges of protecting participant confidentiality, especially for those in vulnerable communities (Scheper-Hughes, 1993; Nakamura, 2006; Agustín, 2008; Desmond, 2016). While they do not address reproducing quotes themselves, they do address how recreating and representing data leave room for harm. Further, presentation of data is intimately tied to positionality for offline ethnographers (Collins, 1986; Smith, 1990; Borgois, 1996). Ellis (2004) underscores how researcher biases and assumptions shape the narratives they tell in the publication of their data. These questions are uniquely challenging in online contexts.

The use of traceable or searchable data in the social sciences is thought to be one of the biggest concerns for digital researchers (Kling, 1996; Beaulieu and Estalella, 2012; Townsend and Wallace, 2016; Lane and Lingel, 2022). As digital tools become more robust, so too do the risks of our participants being located and searched (Shklovski and Vertesi, 2013). The most recent edition of the AoIR guidelines also addresses this issue. Those who opt for data fabrication argue our primary goal as researchers is to protect the privacy and anonymity of our participants. Boellstorff et al. (2012) argues the most important thing to keep in mind are the consequences of participants being identified.

Screenshots, videos, and audio recordings taken from online spaces are particularly vulnerable to privacy violations (Boellstorff et al., 2012). Accordingly, many social media scholars argue we should paraphrase this kind of data and have done so in their own work. Boyd (2016, p. 91) tells us, “When I quote text from profiles, I often alter the quotes to maintain the meaning but to make the quote itself unsearchable.” Mukherjee (2017) studies a vulnerable community: victims of Intimate Partner Violence (IPV). She paraphrases all data she uses in publication to ensure the safety of her participants. Markham (2012) suggests fabricating posts can be done in such a way that is true to the broad themes of the data. de Seta (2020, p. 91) takes it a step further:

“Fabrication is thus inextricably linked to the idea of expertise. In claiming and embracing one’s role as editor, translator, and

fabricator of multimedia and multimodal vignettes, of composites of events, identities and inscriptions, the digital ethnographer implicitly establishes competence and knowledgeability over a certain sociotechnical context.”

Yet how can a researcher establish knowledgeability over a social world while also distorting the content it produces? For this reason, some QAnon scholars choose to use direct screenshots without alteration (see de Zeeuw et al., 2020; Hanley et al., 2022).

Other scholars suggest alternative methods to avoid the issues that arise from data fabrication. Sugiura et al. (2017) argue removing identifying data is not enough because search engines utilize advanced technology to trace data, and therefore we should summarize the data instead. Boellstorff et al. (2012) alters details such as the time and place of events, and takes care to avoid particularly loaded scenarios. Shklovski and Vertesi (2013) avoid searchability by “Un-Googling,” a practice where they remove all identifiers, including names, titles, and contextual details of the environment including city and country names. Reilly and Trevisan (2016) only quote directly from public figures because they do not have the expectation of privacy or anonymity. They also use word clouds to show the most commonly used words in posts rather than the posts themselves. And while Robson (2017) sometimes paraphrases the data, he typically puts direct quotations into narrative form.

Data fabrication ultimately speaks to a much broader issue within digital research, the “crisis of representation,” which is our decreased ability to adequately represent society (Marres, 2017). Researchers are less able to “establish knowledgeability” over digital spaces given anonymity and other features of digital interaction which prevent accurate representation. However, data fabrication has become an increasingly common practice within social media research. The implications of this practice are substantial. Inaccurate portrayal of the data has myriad consequences. When a researcher paraphrases or alters the original speech or text, the door is left open to bias and distortion of meaning. The integrity of the data is more compromised the farther the text gets from its original form. Word clouds and narrative summaries do not necessarily solve the problem. Care must be taken to avoid harm to participants, and especially to vulnerable participants such as the communities studied by Mukherjee (2017), yet care must also be taken to stay true to the data produced within the social worlds we study.

5.1. QAnon and the data fabrication debate

Data fabrication, or the process by which researchers alter the original text of an online post to avoid traceability, is preferred by many digital media scholars. The Google Age makes it easy to search for and find content and its associated author or authors, including not just text but images with the reverse image search option. Yet when researchers reconstruct digital data produced by study participants, they leave room for the alteration or mischaracterization of meaning.

This issue became immediately apparent when I began to study QAnon. QAnons post purposefully cryptic messages which are meant to be decoded by insiders. They also frequently “troll,” or

in other words post insincere and inflammatory messages. They have developed their own language and each platform and group have unique subcultures. Outsiders simply do not have the cultural knowledge to meaningfully reproduce primary data, and even if they do, there is no way to determine the intention of the poster.

There is also no way to determine the identity of a poster. Social media platforms make it easy to present fake profiles, names, photos, and more. QAnons disproportionately remain anonymous online (hence their name). Therefore, researchers have no meaningful way to discern age, race, gender, or other attributes social scientists find important. Analysis then becomes particularly difficult because researchers cannot determine how perspectives might be influenced nor draw out meaningful patterns among social groups. In line with the “crisis of representation” (Marres, 2017), the issue of unknown identities places the researcher at a farther distance from their study participants, making it difficult to faithfully represent their data.

Further, sometimes data is far too complex or difficult to reproduce. Take, for example, the screenshot (Figure 5) taken from a QAnon board on 4chan. The figure depicts a “Q-Clock,” a tool QAnons use to decode posts on mainstream social media platforms as well as “Q Drops” (messages from Q). As a researcher, how would I reproduce this Q-Clock while staying true to its original meaning? Each line drawn and text circled has a unique meaning to the anon, and hundreds of circles appear on the clock with numbers and dates of unknown importance. The cryptic nature of QAnon communications makes it nearly impossible to reproduce their text and imagery.

Data fabrication also inherently privileges the position of the researcher. Social scientists have been calling for attention to power dynamics between researchers and the researched for decades (for example, Merton, 1972; Garfinkel, 1984; Collins, 1986). To change text (or other forms of data) based on our own interpretations is to exert a form of power. It insinuates the researcher fully understands the original meaning and can alter it in a way that they see fit for the purposes of their research. We come to our research sites with various biases and unknowns, none of which can be avoided when fabricating data. This process of reproduction is dangerous not only because a researcher might misinterpret meaning, but also because it opens the door to unfair and biased representations.

Researchers might also unknowingly reproduce power dynamics should they fabricate content of a racist, sexist, or otherwise discriminatory nature. QAnons are known for hateful content, so to reproduce it as a researcher can become problematic. The reconstruction of anti-Black or antisemitic posts might reproduce harmful biases in the words of the researcher. It can be argued these kinds of hateful posts should be avoided entirely, so as to not amplify hateful messages to broader audiences. The ethics of this kind of direct reproduction should be an ongoing debate. Yet it seems a closed case that reconstructing an anti-Black post in the researcher’s own words, for example, is an unethical practice.

A third and more logistical problem concerns the form of media researchers might want to fabricate. People do not simply communicate through text on social media. They use images, videos, audio, memes, GIFs, articles, hashtags, and more. How can a researcher fabricate a meme? An audio clip? While data fabrication might work for text, it does not hold up for various other mediums used frequently by social media users. Fabrication only seems to hold up for text-based data.

The lurking debate can be summarized as follows: Searchability and traceability are made easier online, so many researchers have decided to paraphrase text, videos, and audio while maintaining meaning. According to certain scholars, the practice of data fabrication may not sufficiently safeguard the anonymity of participants, thereby prompting a preference for qualitative data summaries or limiting direct quotations to public figures. The solutions I engage in my research are fairly straightforward. I ask for permission and provide participants with as much information as possible on the potential risks with special attention to how the nature of the internet changes the types of risks involved. I build transparent relationships with participants. The participant can then decide if they want their data to be used in a publication available to widespread audiences, and further if they want their name or username to be anonymous. Traceability and searchability should be discussed with participants candidly, and I have done this through direct messages and in Zoom interviews. This process has involved more steps with data that falls farther on the “private” side of the spectrum. For example, when a post is within a group with only a few members and I have to apply to join, I check in with participants at multiple points throughout the study. At first I ask if I can store their data, then again when I am considering using it in publication (at which point issues surrounding searchability can be discussed), and again when I know where the data will be published. I do not anonymize or request permission for posts that fall farther on the “public” side of the spectrum, such as a post on a mainstream platform by a celebrity. The case of QAnon reveals how difficult it can be to reproduce or fabricate complicated digital artifacts of different mediums, and the power dynamics inherent in reproducing data of a racist or otherwise hateful nature. Further, QAnon has various subcultures which make it difficult for the researcher to have enough cultural knowledge to reproduce data.

Another solution is description rather than fabrication. Researchers might opt for a qualitative account of the posts they come across if they believe using the original post in a publication might harm the participant or themselves. With this method, researchers do what qualitative studies are known for: providing narratives. Although the narrative form is also subject to biases, misinterpretations, and power dynamics, it does not tell the reader: “This is the same as what my study participants produced.” Narratives might be most suitable for those studying vulnerable populations.

6. Institutional review boards

Institutional review boards (IRBs) affiliated with universities typically provide the first—and sometimes only—ethical analysis of research conducted on human subjects in the United States. Their work is integral to the research enterprise. First established in the 1970’s, American IRBs were created to ensure the welfare and protection of human subjects involved in in-person research. It was only in the 1990’s that IRBs began to oversee research in the social sciences and humanities, and the model for evaluation was still heavily based on biomedical research (Babb, 2020). IRBs are also notoriously misaligned with qualitative research because of excessive standardization (Babb et al., 2017). As a result, IRBs are not particularly well equipped to evaluate social science research conducted largely or exclusively online.

Kitchin (2007)'s guidelines on web-based research in Canada heavily influenced how Canadian IRBs classified submissions (Seko and Lewis, 2017). Kitchin determined that “non-intrusive” web-based research, by which they meant research using publicly accessible online material, did not constitute human subjects research and should therefore not be reviewed by IRBs. Text itself in cyberspace was not “human” enough. Some guidelines even indicate internet research is by definition public (see, for example, ESRC, 2015). Others think the IRB itself is non-sensical. Markham et al. (2018, p. 3) claims: “It is impossible to standardize or universalize what constitutes the ethically correct actions in technology design and research contexts.” Regardless of justification, many scholars view the involvement of IRB in internet-based research unfavorably.

On the other hand, some contemporary scholarship argues the IRB process is not enough. The digital landscape has changed the nature of research, and IRBs need to adapt (Miller, 2012; Bailey, 2015). For example, Bailey (2015) argues social media users are not the traditional infantilized subjects that IRBs assume researchers are dealing with in offline contexts. It is also particularly difficult to clarify the nature of an online field site (Boellstorff et al., 2012). Further, traditional conceptions of informed consent are grounded in offline research yet internet researchers have far less control over the lifetime of their data and therefore cannot be fully forthcoming with participants about potential risks (Matzner and Ochs, 2017). Despite the changes brought about by the internet, IRBs tend to lack the proper resources and processes for reviewing online research. Hutchinson et al. (2017) emphasize that most IRBs have little or no training in the ethical review of internet research. Seko and Lewis (2017) similarly claim American IRBs have no specific internet research ethics training nor guidelines for internet protocols. IRBs are also not equipped to understand the public vs. private nature of social media interactions (Hutchinson et al., 2017), an important conundrum discussed above.

A few internet researchers have suggested expanding the scope of IRB oversight. Luka et al. (2017) argue the IRB should be involved in more than just the proposal stage, as ethical considerations are needed at every stage. Hutchinson et al. (2017) suggest the IRB should be active throughout the entire research process including the dissemination of findings. This approach makes sense if we consider ethics as *choices* at critical junctures (Markham et al., 2018).

Important questions are still unanswered. Clearly not all internet interactions are public, but how should the researcher and the IRB determine what is public vs. private? If we take the approach that we should only use data when the participants consider their activity to be public, how do we know what participants are thinking when they post? Further, given IRBs remain embedded in different policy frameworks, national cultures, histories, and institutions (Babb, 2020), how do you account for differences when internet users can come from anywhere? How does a researcher identify themselves to a whole group if there are upwards of 100,000 members? Should our ethical standards change if the population we are studying is vulnerable?

I suggest some answers to these questions in the section following. Our decisions as researchers have implications for the endeavor of social science itself. Some practices such as deleting

data can cause issues for replicability, reproducibility, and can introduce bias (Tromble and Stockmann, 2017). Sharing data is an important part of the process of ensuring valid research (Weller and Kinder-Kurlanda, 2017). So how do we ensure ethical practices while maintaining the integrity of the research process?

6.1. QAnon and institutional review boards

This section builds on my own experiences with the IRB for a project on QAnon activity on social media. Scholars have mixed opinions as to the role of the IRB. Should all social media research be exempt because it is technically public? Should the IRB be *more* involved and intervene at multiple stages of the research process? Should there be internet-specific protocols? First, I discuss the ways in which current review processes are ill-equipped to evaluate social media research. Next, I suggest what might be done to ameliorate the issues debated by scholars.

One of the first questions I was asked by the IRB was whether or not any participants will be located outside of the United States. This is done because privacy and consent laws vary considerably by country. However, national boundaries are not so simple on the internet. In most cases, the researcher has no way of ascertaining where participants are coming from. Perhaps more importantly, there is no way to know the age of participants. Minors and other vulnerable populations are likely to be included in most social media research. This is particularly true for research on the far-right where users are often anonymous. Even if a user claims to live in the United States and be of a certain age, there is no way to verify the accuracy of their statements. For example, I conducted a phone interview with a QAnon adherent named “Sean” who claimed to be 40 years-old and living in New Hampshire. I later Googled his username and discovered he was 64 years-old, living on the other side of the country, had an entirely different name, and was a convicted felon. Social media researchers can never claim to know where their participants are coming from or who they really are.

Consent procedures are problematic as a result. One of the most crucial tasks of IRB reviewers is to determine how consent will be obtained. Yet how can a researcher provide informed consent when they do not know their participants? What happens when researchers observe groups with hundreds of thousands of users? How can researchers avoid obtaining data from minors? Moreover, it can be incredibly difficult to contact users to ask for consent in the first place. My own experiences asking 4chan and 8kun users for consent to use their posts has been incredibly difficult. All users are anonymous by default on these platforms and there is no tool to directly message someone (like there is on Facebook and Twitter). See Figure 6 for how users are identified on 4chan. Everyone is labeled “anonymous,” assigned an ID number, and identified by their country's flag. Although I can be certain of where the user is posting from because the platform uses IP addresses to assign a flag, I have no sense of the user's name, age, race, ethnicity, or anything else of importance to researchers. Consent has become increasingly complex as people across the world take more of their lives online.

Another question asked by IRBs concerns the type of information obtained from participants. The IRB for my institution currently provides the following guidelines to determine private vs. public: “Private information is information that an individual can reasonably expect will not be made public. Generally, data sets that require specific permission from the data owner or are restricted access are considered ‘private.’” Just as there is no way to determine who an individual user is, there is also no way to determine what they expect will be made public and what they expect will be private. Next, what constitutes “specific permission” on social media? Is clicking “join group” asking for specific permission to view the data? There are various levels of restricted access which vary by platform. The private vs. public binary does not address the complexities of social media research nor does it tell IRB evaluators much about a proposed study.

The IRB also asks for a description of the overall methodology of the study including how the researcher will gain access to the communities they would like to study. Scholars have noted IRB evaluators are not formally trained on social media research protocols and specifically lack knowledge of the practices included in digital ethnography. Gaining access might mean creating an account. It could also mean directly messaging a group administrator and filling out a questionnaire to be accepted. Digital ethnography also changes the nature of participant observation and interviews in important ways. For example, I can scrape the data from hundreds of thousands of users on Twitter fairly easily and then publish whichever posts I deem necessary for my research. I am then confronted with the searchability and traceability of the posts.

The adequacy of IRBs in assessing social science research, particularly online research, has been a topic of debate among scholars. Some contend that web-based research universally entails publicly accessible data, negating the need for IRB intervention, while others suggest more involvement throughout the entire research process. The matter of how the IRB determines the distinction between public and private data remains unanswered.

There are no universal solutions to the problems IRBs face in regards to digital ethnography. How can we protect hundreds of thousands of unknowable users? Put simply: we cannot. Yet there are some practical steps researchers can take to ensure minimal risk to participants. First and foremost, social media researchers should almost never claim to know specific demographics of all of their participants. They need to make clear what they are able to find out and what they cannot know. I am particularly careful with descriptions of my participants in the discussion of my QAnon data because of the lack of certainty. Accordingly, it might be useful for social media researchers to engage in ethical practices as if all of their participants are vulnerable. This would mean taking extra care to de-identify data and avoiding particularly sensitive questions. Second, IRB evaluators should be aware of the nuances of ethnography adapted to online worlds in order to competently evaluate the potential risks of the research. Third, as the case of QAnon shows, understandings of privacy, consent, and identity are highly variable, and IRBs should be sensitive to these variations. It would be valuable and fruitful for IRBs to engage digital researchers to develop protocols more sensitive to online research.

7. Conclusion

This article has addressed three ongoing debates within the literature on digital ethnography: the public-private binary, lurking, and data fabrication. It has also discussed issues surrounding institutional review boards and their evaluation practices. Overall, I argue ethical digital ethnographic practice requires considering the nuances and complexities of our online social worlds. There are no universal rules which can be applied to all studies. There are, however, guidelines which can be followed and applied to diverse circumstances. My findings have implications for how digital ethnography should be conducted as our lives become even more enmeshed with digital worlds.

It makes little sense to designate all social media content as “public.” Instead, public is a determination which can be made by keeping in mind how difficult a space is to access. The broader the audience which can access the content, the farther it falls on the public side of the spectrum. The more barriers there are to access, the farther it falls on the opposite side of the spectrum. These barriers will look different across and within platforms. Researchers will need to make difficult decisions when deciding what data they should analyze and publish. It is important to keep in mind the various levels at which data exist, and to be in conversation with participants when questions arise. Treating privacy as a spectrum of accessibility and direct communication with participants about content publication are key to ethical practice.

Lurking has been a concern for decades. The first digital ethnographers had to grapple with the invisibility of their presence, and today it has become even more of a concern with the various tools available to hide one’s identity. Although not everyone will choose to use their real name and information, it is imperative researchers make their presence known. This practice gives participants the right to decline, and also more generally raises awareness of who they choose to share their information with. Moreover, active participation benefits the researcher in that they are more deeply immersed in the space and better able to understand their participants.

Data fabrication is perhaps the most controversial practice. Many prominent ethnographers have chosen to reconstruct data in their publications. As I have shown, this practice leaves room for bias and misinterpretation, privileges the position of the researcher, and can even reproduce harmful ideas. Researchers should instead use data in its original form with direct, informed consent from the content producer. Content producers should be fully aware of searchability issues and other concerns unique to the affordances of the internet. Additionally, those who conduct research on extremist groups will likely encounter violent rhetoric and open threats directed toward public figures, marginalized communities, opposing political parties, and other groups. Some threats may be direct and indicate imminent danger to a person or group. These should be reported to proper authorities. Other threats come in the form of vague but violent rhetoric. The researcher then bears responsibility to expose the nature and degree of this form of violence to wider communities through various channels. While publishing or reconstructing this kind of rhetoric might reinforce harmful views, it may also serve to

educate general publics on the reality of these spaces and act as a push toward regulation and other protective measures. Open discussion of these sensitive issues needs to take place to realize meaningful change.

Institutional review boards should require evaluators to do specific training on internet research, and questions need to be tailored to the complexities of digital space. In-person and online researchers are confronted with different ethical quandaries. Their proposals should reflect these differences. And although the internet might make it difficult to answer questions especially in regards to the identity of participants, researchers need to be forthcoming about what they know and do not know.

Many of the above suggestions can thought of through the lens of *relationships* and *local context*. Ethnographers have been grappling with their positionality vis-à-vis their subjects as well as the influence of social and cultural differences from the very beginning. In Geertz's (1973) famous discussion of "thick description," he argues the entire enterprise of ethnography is predicated on analysis of our different webs of significance. In Tally's *Corner*, Liebow remarks that ethnography is defined by "local place stories" (Liebow, 1967, p. xiv). Duneier (1999) reflects on the power dynamics inherent to his relationships with his participants of different races, classes, and statuses. Yet he draws on his embeddedness within these relationships to portray an accurate and ethical account of his experiences. Questions of positionality and locality are particularly complex in digital space because of the ease of lurking and the global reach of the internet. This article draws on decades of reflection within traditional ethnography and argues we must continue to prioritize the ethnographic relationship and the importance of context in digital ethnographic research.

Many questions remain as to the ethics of digital ethnography. Are there types of posts or even groups which should be avoided by researchers entirely? What benefits and harms come from using real names and photos? Do some ethical practices affect the quality of the data, and if so what can be done to ensure data integrity? How can we reasonably make claims about groups or individuals in the absence of identifiable information? If researchers choose to take digital ethnography offline and get to know their participants in person, should a different set of ethical practices be applied?

Researchers will need to contend with these and other questions as digital ethnography becomes more widely accepted.

Data availability statement

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

Ethics statement

The studies involving human participants were reviewed and approved by New York University Institutional Review Board, IRB-FY2022-6722. The patients/participants provided their written informed consent to participate in this study.

Author contributions

MC contributed to the conception and design of the study, collected, organized and analyzed the data, wrote all drafts of the manuscript, revised, read, and approved the submitted version.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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OPEN ACCESS

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RECEIVED 01 February 2023

ACCEPTED 16 May 2023

PUBLISHED 02 June 2023

CITATION

Forberg P and Schilt K (2023) What is ethnographic about digital ethnography? A sociological perspective.
Front. Sociol. 8:1156776.
doi: 10.3389/fsoc.2023.1156776

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What is ethnographic about digital ethnography? A sociological perspective

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When COVID-19 health guidelines vastly restricted or shut down in-person ethnographic research in 2020, many researchers pivoted to forms of online qualitative research using platforms such as WeChat, Twitter, and Discord. This growing body of qualitative internet research in sociology is often encapsulated under the umbrella term “digital ethnography.” But the question of what makes digital qualitative research ethnographic remains open. In this article, we posit that digital ethnographic research necessitates a negotiation of the ethnographer’s self-presentation and co-presence within the field that other forms of qualitative research, such as content or discourse analysis, do not require to satisfy their epistemological stance. To make our case, we provide a brief overview of digital research in sociology and related disciplines. Then, we draw upon our experiences conducting ethnographies in digital communities and in-person communities (what we call here, “analog ethnography”) to explore how decisions about self-presentation and co-presence facilitate or block the generation of meaningful ethnographic data. We think through pertinent questions such as: Does the lower barrier for anonymity online justify disguised research? Does anonymity generate thicker data? How should digital ethnographers participate in research environments? What are the possible repercussions of digital participation? We argue that digital and analog ethnographies share a common epistemology that is distinct from non-participatory forms of qualitative digital research—namely the need for the researcher to relationally gather data from the field site over an extended period of time.

KEYWORDS

ethnography, anonymity, participant observation, digital ethnography, qualitative research, internet, hybrid ethnography

1. Introduction

The proliferation of digital and interactive technologies such as social media in the 21st century has necessitated an expansion of the sociological toolkit for studying social life (Hampton, 2017). Platforms such as Twitter create opportunities for social scientists to gather data at a scale never-before-possible that can be used to assess questions such as how anti-immigration laws shape public sentiment (Flores, 2017), or to shed light on the drivers of political polarization (Bail, 2021). As the percentage of Americans who use social media grew from 5% in 2005 to 72% in 2021 (Auxier and Anderson, 2021), qualitative researchers have also adapted their research methods for a world in which there is increasingly less distinction between people’s digital lives and their so-called real or in-person lives. While a few anthropologists delved into how people made community in immersive virtual worlds in the late 2000s (see Boellstorff, 2008; Nardi, 2010), researchers today face a cultural landscape where online lives and practices are increasingly normative and integrated into people’s everyday lives (Bluteau, 2021). Illustrating such a shift, the late 2010s and early 2020s saw

a rise in violent actions in the “real world” that resulted in part from misinformation shared at massive scales across digital platforms by proprietary social media algorithms and the use of the internet by extremists (Ndlela, 2020; Gaudette et al., 2022). To more fully understand how people’s digital interactions co-exist with, shape, and are shaped by their lives offline, social scientists face the challenge of creating research techniques *for* digital technology rather than techniques conducted *through* digital technology (see Hine, 2015).

While anthropology, communications, media studies, and computational sociologists have been grappling with the opportunities and challenges of studying the interplay between virtual worlds, social media, and face-to-face interactions since the early 2010s (Boellstorff et al., 2012; Dicks, 2012; Hampton, 2017), qualitative sociologists entered these discussions with new vigor during the 2020 COVID-19 pandemic.¹ As countries locked down shared in-person spaces, researchers began to follow people to “where the action is” (Goffman, 1969) on digital platforms. Without leaving home a sociologist could conduct rigorous and insightful qualitative research using Reddit forums, Instagram stories, and TikTok posts. A prolonged engagement with an online community could be accomplished at hours that fit the researcher’s schedule, particularly for those with family and community responsibilities, and without the risk of viral exposure. The pace of interaction on a platform such as Discord could generate volumes of data in a week that might take 6 months to gather in a face-to-face setting. As the publishing expectations for sociology graduate students doubled in the last 25 years (Warren, 2019), digital ethnography also offered hope of faster and more efficient data collection for researchers worried about the slower tempo of in-person research.

The growing number of conference presentations, courses, and articles about digital ethnography in sociology suggests that the validity of examining mediated, networked life has become more widely accepted in the discipline (see Lane and Lingel, 2022). This growth in qualitative digital research is a much-needed corrective for the long-standing dismissal of online social life that has characterized mainstream sociology. At the same time, we question the use of “digital ethnography” as a catch-all term for any qualitative research done through social media platforms regardless of whether there was a participant observation component of the study. In thinking through what is ethnographic about digital ethnography,² we offer the following proposition: generating meaningful data in a digital ethnographic study necessitates

reflexive decisions about the researcher’s participation and self-presentation that other forms of qualitative digital research do not require to satisfy their epistemological stance. In content and discourse analysis, for example, researchers convert existing social artifacts into data (Lune and Berg, 2012). In contrast, ethnographic research requires that “the observer is embedded in the data themselves,” a “reactive interaction” that means that “even the most passive researcher, merely by being present, inevitably shapes what is observed” (Small and Calarco, 2022, p. 12). It is the researcher’s “co-presence” (Beaulieu, 2010)—the “exposure” to the social world and its people (Small and Calarco, 2022)—that makes a research project ethnographic, whether such prolonged, direct contact happens in physical spaces, through screens, or both. For digital ethnography, our increasingly “enmeshed” “digital landscape” (Bluteau, 2021) means that exposure to our participants and the digital technology in their lives can entail anything from becoming an orc in a virtual guild to posting “fit pics” on Instagram. We do not offer this proposition as a gatekeeping effort. Instead, we hope to offer a step toward greater epistemological clarity across qualitative digital methods—though we focus here on digital ethnography.

In this article, we use the term “digital” to reference forms of networked and mediated socialization.³ Digital ethnographic approaches could include participant observation in virtual or online communities, as well as interactions that bridge on- and offline worlds, such as hack-a-thons, esports tournaments, or workplace information systems. Of course, the ubiquity of social media platforms, proprietary algorithms, and artificial intelligence in our everyday lives can make it feel like all ethnography is now to some extent digital—or that it should be. Because technology is never evenly distributed across a population, however, we see value in considering digital ethnography as a distinct *mode* of ethnographic research that could be used alongside or in lieu of ethnography conducted solely in physical, in-person settings—what we will call “analog ethnography.” Sociologists today continue to produce distinctly analog ethnographies for a variety of reasons—digital interactions and records may be legally protected, such as in hospitals, less accessible to community members, such as among people who are unhoused, or purposefully restricted by structures of power, such as in prisons and jails. We do not make this distinction to position analog data collection as more authentic, as some audiophiles might say about classic vinyl records, or digital data collection as representing a brighter, more efficient future, as some technophiles might claim (Hassan, 2022). In contrast, we position analog and digital modes of ethnography as intimately related, increasingly overlapping, and intertwined while acknowledging that each mode has some unique affordances. While we will touch on “hybrid ethnography” (Przybylski, 2021), we marshal this ideal type comparison here to consider the possibilities, ethics, and outcomes of different researcher strategies across analog and digital ethnography.

¹ While we see sociology in the U.S. as being slow to acknowledge how the internet was changing social interactions at a societal scale, we want to acknowledge here a few notable sociologists who saw the importance of studying online cultures long before this current wave of digital ethnography. T.L. Taylor published a qualitative study of online gaming culture in 2006 and was a co-editor of *Ethnography and Virtual Worlds* (Boellstorff et al., 2012). Dhiraj Murthy (2008) made early appeals to the need for more digital qualitative research in sociology, and Kozinet (2011) produced the first how-to book on internet ethnography in sociology.

² Our title question is a reference to Aspers and Corte’s (2019) article, “What is qualitative about qualitative research?” Taking insight from Small (2021) critique of the article, we recognize the impossibility of providing a definitive answer. We offer instead some ways forward for a broader discussion.

³ Much of the initial discussion of digital ethnography focused on “online,” “internet,” or “virtual” communities. This language necessarily persists in our discussion of this literature. We view these terms as specific subsets of digital ethnographic inquiry more generally.

In what follows, we draw on sociological writing about the craft of ethnography and our own fieldwork experiences to contribute to sociological conversations about “qualitative literacy” (Small and Calarco, 2022) and digital ethnography (Lane and Lingel, 2022). Forberg (2022) conducted a digital ethnography of QAnon, an anti-establishment conspiracy theory that began on the fringe web forum 4chan and migrated across the internet. Over the course of four months, Forberg used a new smartphone to follow online communities, tracking how his digital engagement influenced algorithmically generated app recommendations, newsfeeds, and notifications. He formed relationships with QAnon followers, conducted text and audio interviews, kept up with QAnon content and current events, and performed quantitative analysis on Twitter data scraped via an application programming interface. Schilt conducted an analog ethnography with people undergoing a major change to their embodiment, such as significant weight loss through surgical procedures. She negotiated access as a researcher to in-person conferences, instructional classes, and support groups designed to aid people in their life-altering transitions. Later, she conducted interviews with people undergoing these changes and their communities.

We start with a short overview of digital research in sociology. Then, we explore two major components that we see as central to an ethnographic epistemology: researcher self-presentation and ethnographic co-presence with community members. First, we consider the gains and trade-offs of being anonymous, pseudonymous, or known. Second, we consider what kind of data is generated by different degrees of participation, from being an anonymous “lurker” to a known participant. Of course, much ink has been spilled over the years about these considerations in sociological approaches to ethnography. We revisit these debates with an eye toward the experiences of ethnographers working in or across digital settings where anonymity, purposeful misrepresentation, and playful identity exploration is part and parcel of many community forums and where publicly identifying as a researcher can create a risk of being trolled or doxxed by disgruntled forum members at a speed and volume unprecedented in analog research.⁴ In our conclusion, we argue that a more “reflexive” (Abidin and de Seta, 2020) and intentional approach to ethnographic presence in the digital world can facilitate robust, ethical, and digitally-informed data collection that is applicable to settings across modalities.

2. Sociological approaches to digital life from Web 2.0 to Web 3

The study of social interactions is a central concern of qualitative sociological research. From Erving Goffman’s “interactional ritual” (Goffman, 1967) to West and Zimmerman’s “doing gender” (West and Zimmerman, 1987) to Randall Collins’s “interaction ritual chains” (Collins, 2004), researchers have theorized how face-to-face interactions, both real and imagined,

can create a shared sense of social reality, exacerbate stigma, bolster a sense of self, and generate collective emotions. Yet, as the internet became ubiquitous to the social life of many young people in the U.S. in the late 1990s—a cultural shift that DiNucci (1999) labeled as “Web 2.0”—sociological research and theorization did not keep pace. This emergent online culture was characterized by increased user-generated content, easy-to-create websites, and participatory engagement in new formats such as blogs, virtual worlds, massively multiplayer online role-playing games (MMORPGs), and nascent forms of social media. A few notable ethnographers recognized the importance of these new forms of sociality in the 2000s. In *Play Between Worlds* (Taylor, 2006), T.L. Taylor provided an in-depth look at the complex social networks of the community surrounding the networked multi-player game *EverQuest*. Taylor analyzed her own experiences playing the game with others online and her in-person participant observation at fan conferences. Boellstorff (2008) conducted a two-year ethnography within the virtual world of *Second Life* which, in 2007, had 8 million online inhabitants, and Nardi (2010) did ethnographic work in the *World of Warcraft* community, an MMORPG that had 10 million players in 2009. However, these hybrid and virtual approaches to ethnography did not become central to U.S. sociology for reasons we speculate have to do with the marginalization within the discipline of forms of engagement associated with the young, with games, and with play and leisure, as well as the dismissal of the interdisciplinary, progressive field of cultural studies in favor of the more politically neutral sociology of culture (see Long, 1997).

As usage of social media sites such as Facebook and YouTube spread across older age groups in the mid-2010s, quantitative sociologists began to take notice of these platforms as mechanisms for data collection (Hampton, 2017). The launch of Amazon’s crowdsourcing website, Mechanical Turk, gave academic researchers a new format to reach millions of users willing to perform discrete online tasks. Rather than mailing expensive surveys or rounding up undergraduates for an experiment, sociologists could run their research projects with just a click of a button. Social media sites offered volumes of user-generated content. Computational sociologists sought data-sharing agreements with tech companies and used programs such as Python to “scrape” large quantities of data from user profiles and posts to learn about the formation of social networks, political views, activism, and consumer tastes. Data-scraping social media posts offered a way around the problems of both low response rates and social desirability bias, as researchers could access data made by users for public posting and metadata about users collected by companies. Rather than asking respondents about their social networks, researchers could pinpoint exactly how people were connected to one another via social media. The availability of these new data quickly raised thorny ethical issues about user confidentiality, proprietary algorithms, and online privacy (Parry, 2011).

Qualitative sociologists too began to explore how social media interactions factored into people’s offline lives, such as Ilana Gerson’s *The Break-Up 2.0: Disconnecting Over New Media* (Gerson, 2010). Studies examined how online content creators imagined their personas and their audiences (Marwick and Boyd, 2011), the offline repercussions of online interactions (Wang et al., 2011), and the expansion of virtual communities for marginalized

⁴ “Doxxing” is the release of someone’s personal information, such as telephone number or home address, online. The term comes from 1990s hacker lingo for releasing classified or sensitive documents stolen from a computer.

and geographically isolated people (Gray, 2009). These studies mostly maintained the focus on people's lives offline, as the new affordances of technology still operated predominantly as an augmentation to face-to-face interactions. As social media usage and smartphones became ubiquitous in the U.S. in the mid-2010s, sociologists began to study how people's online interactions flowed into and shaped their lives offline—a more hybrid approach to ethnographic inquiry (Przybylski, 2021). Notable innovations came out of urban sociology, with researchers such as Lane (2019) and Stuart (2020) using analog and digital ethnography to trace how neighborhood violence in poor communities is driven by interactions on social media that turn deadly. Within studies of youth and schools, ethnographers such as Miller (forthcoming) and Outland (2020) explored how digital interactions inside and outside of school between students, parents, and teachers shape the conditions of students' learning environments in ways unimaginable only a decade ago. Such hybrid ethnography entails developing a long-term co-presence in a community's overlapping digital and analog settings, fulfilling Small and Calarco's (2022) expectation of ethnographic "exposure" to each space. In our view, these studies embody the type of research proposed by some of the early digital ethnographers because they abandon the arbitrary distinction between online life and "real" life that only "provide[s] *a priori* answers to some of the most intriguing questions" about the increasingly digital social world (Leander and McKim, 2003, p. 223).

2.1. What is digital about digital ethnography?

The emergent conversations about the shift to Web3 in the 2020s (Stackpole, 2022), a culture characterized by decentralization, immersive virtual and augmented reality (e.g., the metaverse and *PokemonGo*), block-chain technology, and artificial intelligence, have been accompanied by a formalization of digital ethnography as a method in its own right—an ethnography for the 21st century. Across the social sciences, researchers offer a host of digital ethnographic concepts to replace the analog language of field sites, such as "metafields," "connection," and "flows" (Leander and McKim, 2003; Hine, 2015; Airoidi, 2018). Yet, as with many emerging fields, we have seen a confusing proliferation of "buzzword ethnography" (Abidin and de Seta, 2020), wherein various forms or styles of ethnography are proposed for niche use cases. Buzzword ethnography—which includes methods such as "interface ethnography" (Ritter, 2021), "hashtag ethnography" (Bonilla and Rosa, 2015), and "appnography" (Cousineau et al., 2019)—has excelled at reimagining ethnography for specific, mediated environments. This platform specificity has a downside—namely a very short life span. Such fragmented theorizing privileges narrow digital use cases, some of which become antiquated with changing technology in less than 5 years.⁵ This approach, what

we would call "media ethnography," also often departs from the aim of analog ethnography to tell "thick," "social stories" (Geertz, 1973; Murthy, 2008, p. 837), turning instead to digital stories where technologies rather than people become the subjects of ethnographic investigation.

We do agree with media ethnographers that a digital approach to ethnography should retain a sensitivity to the affordances of technology at the level of hardware and software, user interfaces, and entire platforms—essentially asking, how does digital technology work and what does that technology *do*? For us, a sociological approach to digital ethnography should relationally uncover how mediated social experiences engender behaviors, interactions, processes, and identities, connecting the distinctly digital with broader societal practices, norms, and experiences. Hine (2015) provides useful language for this linking of digital processes to social phenomena. She argues, extending the work of Geertz (1973) to the digital, that:

[T]he terms "experience-near" and "experience-distant" could usefully be rendered as "technology-specific" forms of engagement and "technology-neutral" forms of engagement. The ethnographer's task as a participant in a Facebook group is to bridge between the technology-specific status update, and the technology-neutral social act that the status update performs (p. 28).

Tufekci's (2017) research on social media activism in the mid-2010s is a good illustration of Hine's formulation. Much of the activist organizing that she studied took place on Facebook, a platform that requires first-time users to sign up with a verified name and encourages users to report violations of this rule to community moderators. Her research shows how activists who adopted pseudonyms for protection were targeted by opposition campaigns who spammed Facebook with reports as a method of censure. To apply Hine's (2015) concepts to this research, the *technology-specific* act of reporting on the platform enabled the *technology-neutral* act of political censorship. Tufekci extracts the social phenomenon from the technological process, which can help resolve the question of whether and how technology is creating new social patterns or reproducing and repackaging old ones (Seaver, 2017). Similarly, Phillips (2015) work demonstrates how the *technology-neutral* act of harassment is made *technology-specific* due to the anonymity afforded by the internet which enables the development of personas engaged in violent and hateful trolling. Finally, while Seaver's (2022) ethnography of music recommendation algorithms may be classified as hybrid because it took place in the analog corporate workplaces that create platforms such as Spotify and Pandora, he retained digital technology as his focal subject. When he shows that engineers create music recommendations by profiling users based on an assortment of data streams, he is describing the computational and industrial contexts that give rise to a *technology-specific* experience—one that forms the basis of a shared cultural experience for listeners worldwide.

What unites these ethnographies in our view is that the authors seek to "capture the complex imbrications of technology and society" (Sassen, 2002, p. 365) rather than to detail the function of a particular digital technology. Such studies investigate the affordances of digital technology by developing relationships to

⁵ Here we can think about, for example, the rise and fall of Myspace, LiveJournal, and Tumblr in terms of users, or the attrition of young people from Facebook and Instagram in favor of TikTok. And, at the same time, a how-to for TikTok ethnography would be short-lived.

the people, places, and mechanisms of a digital community, and by participating in the technology at hand via these platforms. In doing so, the researchers capture something important about our increasingly networked world, leading some ethnographers to argue that “it is no longer imaginable to conduct ethnography without considering online spaces” (Hallett and Barber, 2014, p. 307), or to make a case that all ethnography is becoming hybrid (Przybylski, 2021). In contrast, we posit that there are still spaces where investigation does not require an attention to the social role of digital platforms, where such attention would not necessarily yield richer data, or where digital records are legally protected or restricted from sociological investigation. Hoang (2022), for instance, demonstrates the lengths that the very rich go through not to leave a virtual or physical paper trail of financial transactions when “playing in the gray.” Organizational and institutional ethnographies that center actors who handle legally protected or confidential records, such as hospitals, rely heavily on in-person modes of data collection. Some settings also actively restrict access to online life—we think, for instance, of Walker’s (2022) ethnography of doing time in jail, which is the ultimate illustration of what it means to be trapped in “meatspace” with other people with no virtual escape. To imagine the social world that we currently inhabit as evenly hybrid runs the risk of excluding the experiences of people who are very rich, very poor, very old, or who are in institutional setting, which means we should think carefully about what mode or modes of ethnographic research are most salient to our particular setting.

2.2. Okay, but what is ethnographic about digital ethnography?

We envision a digital ethnography that blends technological analysis and sensitivity with the sociological epistemology of analog ethnographers—namely an emphasis on co-presence in the field and decisions about researcher self-presentation. Digital techniques such as content analysis, distant reading, algorithm auditing, or user experience research are useful tools in an ethnographer’s toolkit but are not, we argue, ethnographic on their own. The crucial question for the researcher is what form of knowing the data collection strategy provides. Here, we juxtapose two studies to make our point: Panofsky and Donovan’s (2019) digital discourse analysis of a white supremacist forum and Blee’s (2003) analog participant observation of white supremacist groups. Panofsky and Donovan’s research question centers on how avowed white supremacists made sense of new genetic testing technologies that typically reveal some amount of “non-white” ancestry. To answer this question, they went to the white supremacist website, Stormfront, after learning that users were challenging each other to prove their racial purity by publicly posting their test results. The affordances of anonymous digital spaces meant that Panofsky and Donovan could collect data from posts unobtrusively. They selected online conversations about test results at one point in time, which allowed them to see how community members bolster or challenge each other’s identity management strategies. In contrast, Blee examined the social processes that lead women to enter, stay, and exit racial hate movements. Her initial historical

research showed the crucial role white women had played in these male-dominated movements. Yet, the existing sociological research omitted any engaged discussion of women’s roles. To fill this gap, her research question focused on how women’s racist beliefs developed, how these beliefs shaped and were shaped by their family and romantic relationships, and whether they enacted their racist beliefs in their everyday lives. To answer these questions, Blee spent years reading materials from hate groups, attending in-person meetings and events, and conducting life history interviews.

Both studies take a sociological approach to understanding racist beliefs and attitudes. But the method of data collection they adopt shapes how they operationalize these concepts in fundamentally different ways. Blee (2003) incorporated textual analysis into her research about women in racial hate movements, as she used documents produced by these groups, such as fliers, pamphlets, and radio programs, to identify the public-facing discourse that members circulated. To see these discursive frames in action, she attended rallies and meetings. She built long-term connections with women leaders and members of these groups and was immersed in these worlds for more than two decades. Conducting life history interviews, she was able to learn about how women in the movement saw their racist beliefs develop and evolve over time and the different pathways in and out of the movement they took. Building in-person connections over time also gave her access to participants’ affects—one of the affordances of analog ethnography that is harder to gain across screens or through avatars. She could consistently share space with the same people and observe their emotional displays and body language over time. Her inclusion of an ethnographic component of the study further allowed her to explore the possible contradictions between “saying” and “doing” racist ideologies, a distinctively ethnographic endeavor (Martin, 2003). Adopting this triangulated approach of textual analysis, life history interviews, and ethnographic participant observation positioned Blee to make an argument about how white women enter and exit hate movements, and how their ideological statements about race do or do not shape their everyday interactions.

Panofsky and Donovan’s focus on online posts allowed them to capture how racist beliefs operate at the discursive level on the Stormfront website at one point in time. What they can see in these online conversations are how users collectively and publicly work through the cognitive dissonance produced by scientific information about racial ancestry (genetic tests) and racist ideological beliefs about the desirability of white racial purity. They can document the range of discursive strategies in a historical moment when far-right movements are simultaneously adopting and dismissing scientific understandings of race. They also can draw some inference about social hierarchy on the forum and the relative social standing of individual users by looking for patterns about which users receive support and which users receive ridicule after posting similar test results. What this study cannot do is to tell us anything about how these users came to their beliefs or whether they act on these beliefs in their offline lives. To get this kind of data, they would need to change methods. They could, for instance, approach users on Stormfront for interviews to learn more about their beliefs and attitudes. Digital researchers have gained access to criminal or far-right fringe groups with similar approaches

(Gehl, 2016; Forberg, 2022). Or Panofsky and Donovan could build connections with users and begin to observe them in their offline lives. But—and this is crucial to our argument—the *absence of interviews or observations in a digital discourse analysis is not a limitation*. This choice reflects a particular approach to knowledge formation that we should not expect to mirror the epistemological stance of other forms of qualitative research (Lamont and Swidler, 2014; Small and Calarco, 2022).

Distinct digital research methods share some similarities, such as the unique affordances around researcher anonymity on many digital platforms, the access to large volumes of data, and ethical concerns about what is public and what is private online. Panofsky and Donovan's study, for instance, has a lot of overlap with Bail's (2021) research about far-right polarization on the internet, though the sample sizes in these projects are vastly different. But, we argue, analyzing digital content does not make a digital ethnography. We suggest that digital ethnographers share research opportunities, challenges, and ethical concerns with analog ethnographers that are not present—and do not need to be present—in other forms of digital research. Making this point, De Seta (2020) revisits Fine's (1993) classic "ten lies of ethnography" for the digital age to show how struggles with participant observation, insider knowledge, and the bounds of a field site shape ethnographic research in both modalities. While the large-scale content analysis and data-scraping developed during the rush for big data suggests that digital researchers can use the increasingly large and complex internet to "know it all," digital ethnography is still grounded in small data that is collected relationally and dependent upon a negotiation between the ethnographer's engagement in the field and what the field can provide. Unlike content analysis's retroactive capacity to download and archive data (Angelone, 2019), the analysis gained through ethnographic engagement must acknowledge the researcher's dependence upon digital platforms, the opportunities and challenges of emerging and evolving relationships with anonymous users, and the researcher's own ability to be active in the field in the field over extended periods of time.

3. Sociological considerations for digital ethnography: self-presentation and co-presence

To make our case for a sociologically centered digital ethnography, we focus on two decisions that are central to an ethnographic research design: the degree of anonymity a researcher will have in their field site (self-presentation) and the degree of participation they will engage in over the course of their study (co-presence). As we show in our first section, digital ethnography opens new questions about the ethics of anonymity and disguise as a research strategy. Within the interdisciplinary field of digital research, these ethical issues have received much coverage, particularly around extreme cases where self-presentation strategies are closely linked with researcher safety, such as studies of criminal behavior on the darknet (Barratt and Maddox, 2016). In many cases, however, the ethics of creating a pseudonym for a web forum or reading a public thread as

an anonymous user are left to Institutional Review Boards, who often know little about internet norms, and ethics guidelines offered by professional associations which may not have been updated for the digital age. We consider these ethical issues in light of the epistemological stance of ethnographic research in sociology. Thinking through the affordances of digital platforms alongside debates about the ethics of disguised research in analog settings, we explore the gains and trade-off of being anonymous, pseudonymous, or known in a field site.

Next, we examine the degree of participation a researcher adopts in digital and analog ethnographies. Co-presence provides the interactive, real-time perspective on events that makes ethnographic research unique from interviews or content analysis (Small and Calarco, 2022). However, decisions about how to participate are shaped by many factors, such as the feasibility of a particular approach in a setting, the positionality of the researcher vis-à-vis the community of study, and the degree of risk a certain strategy brings to the researcher and the respondents. Within digital ethnography, digital platforms also shape the possibility of participation and community norms for engagement in complex ways. What constitutes participation in digital ethnography—as well as when active participation is appropriate—are integral and unresolved questions for contemporary sociological research. We conclude with a discussion of the unique ethical quandaries faced by digital ethnographers when considering the potential risks of online participation to themselves, their respondents, and society.

3.1. On the internet nobody knows you are a sociologist: digital anonymity

As one of the possible tools in a researcher's methodological toolkit, ethnographic observations present a way around a research quandary that has long kept social scientists up at night: the gap between what people say they do in interviews and on surveys and how they behave consciously or unconsciously in their everyday lives. Within a workplace or a school, for instance, people might tell an interviewer that they deeply believe in gender equality or meritocracy. Observing these same people over time, however, an ethnographer may see them engaging in behaviors that reproduce forms of stratification (Martin, 2003; Khan and Jerolmack, 2013). This extended engagement in a setting captures interactional mechanisms in a unique way, as the ethnographer functions as the research instrument (Small and Calarco, 2022). At the same time, openly observing people can have a chilling effect on behavior. Researchers often adopt some degree of a cover story to get meaningful data. The depth of a cover ranges from "shallow," an approach where the researcher is known to be collecting data, but the exact questions of interest are slightly obscured, to "deep," where the researcher is a total participant in a setting where most people do not know that they are being observed (Fine, 1993). Studying the Levittown suburbs, Gans (1967) adopted a shallow cover by telling neighborhood residents he was conducting a historical study. Seeking to observe customer interactions in a big box toy store, in contrast, Williams (2006) adopted a deep cover where her co-workers knew her as just another cashier. As ethnographic relationships are always evolving, a researcher may

move between no cover, shallow cover, and deep cover over the course of a single research project (Hoang, 2015).

While deep cover, a form of disguised research, is not explicitly forbidden in sociology, this approach is governed by special considerations from the [American Sociological Association \(1999\)](#). Disguised researchers must anonymize research subjects and settings and consider a specific set of questions about whether this approach will increase possible harm to respondents during and after publication. As long-standing debates in sociology demonstrate, however, there is not a definitive answer to the question of what constitutes an ethical disguise. We are accustomed to researchers making strategic choices about their presentation of self in face-to-face research, such as wearing more professional clothing than they might in their everyday lives or displaying buttons or clothing styles that locate them within a shared subculture with respondents. But what if a researcher goes along with sexist conversation that he does not agree with to build rapport with law enforcement (Leo, 1995)? Is that an ethical cover? Taking a more extreme case, sociologists would widely agree today (as would the Institutional Review Board) that it is unethical to ask graduate research assistants to join a doomsday cult as undercover participants (Festinger et al., 1956). At the same time, some disguised studies that raised hackles in sociology generated insights into inequality and discrimination that would not have been possible through other methods at that time (Nardi, 1996). Yet, with the emerging conversations about unmasking field sites in sociology (Jerolmack and Murphy, 2019) and the push for greater data transparency (Murphy et al., 2021), analog ethnographers who adopt a deep cover face increased scrutiny from reviewers, IRBs, and the public.

Expectations about identity and self-presentation on the internet present new opportunities for deep cover. Beginning with the earliest “cyber” ethnographies, researchers have explored how people experiment with identity online (Turkle, 1995; Kendall, 1998). Identity experimentation can mean developing anonymous personas distinct from one’s “real self,” a topic widely explored in research on online political harassment and trolling (Phillips, 2015; Bail, 2021). It can also mean developing an online persona that is a bridge to a future self outside of these networked spaces, as is the case for some in the non-binary and trans community (Brown, 2019). Anonymity remains the default experience on many modern digital platforms, such as MMORPGs, the darknet, and forums including Reddit and 4chan (Phillips, 2015; Barratt and Maddox, 2016). These situational norms lower the barriers for disguised research online. While the graduate student researchers of the 1950s had to remember the details of their cover story in daily face-to-face interactions with members of the doomsday cult, the research assistant of the 2020s can create an online persona, sign up for a public forum, and begin integrating herself into one of many doomsday prepper groups as a novice. She could watch conversations unfold online and take screenshots of interactions as a record, rather than hiding in the bathroom of the cult leader’s home frantically taking notes. Anonymous or less traceable platforms also can provide a researcher with greater access to hard-to-reach or criminal groups and may offer more protection to the researcher from being doxxed by community members (Gehl, 2016). Of course, this norm of anonymity also presents challenges

to researchers who are unable to determine the demographics of the people whose hashtags and posts they are analyzing. Researchers can draw some inferences about, say, the gender of participants on an incel chat room based on avatars and shared details, but it is hard to verify these assumptions. This lack of identity verification for posts and online interviews can be a barrier to publishing digital research in sociology, as reviewers typically expect demographic information even for “small n” studies that do not purport to offer generalizable conclusions.

Community norms that privilege digital anonymity mean that users are less likely to expect that someone posting on a forum is who they say they are—as captured in the long-standing meme that nobody on the internet knows you are a dog—or to assume that a username like @sexyboi47 reflects anything about the user’s physical presentation (or that the user is even a human). These digital norms may ease a researcher’s concerns about the ethics of entering these spaces without acknowledging himself explicitly as a sociologist. Some researchers go so far as to argue that such an acknowledgment in a digital setting that privileges anonymity violates community norms and puts the researcher and the community at risk (Ferguson, 2017). While this may be true in some cases, our point here is to consider whether using this strategy generates more meaningful data than being known as a sociologist in an online space. We work through this question using Forberg’s digital ethnography of QAnon, a political conspiracy theory primarily associated with the far-right which alleges that an anonymous user on the fringe internet forums 4chan and 8kun has insider access to former U.S. president Donald Trump’s plans to overthrow a Satanic cabal controlling the world. QAnon followers believe that fervent internet activism will usher in a political and religious Great Awakening, which has resulted in a proliferation of QAnon communities across social media platforms (Forberg, 2022). While followers are eager for new recruits, however, they are often paranoid about outside infiltration by researchers, the mainstream media, and government agencies.

Forberg began his study by accessing the community through anonymous accounts that he created for research purposes. This decision reflected the community norms he observed others using in Q forums. As a researcher, he was disguised in the sense that Q followers did not explicitly know that his accounts were tracking their posts and conversations for research purposes. He followed users on various social media platforms to cultivate a trackable internet presence that would produce algorithmic recommendations and experiences potentially similar to those of QAnon followers. This method provided a great deal of insight into the public structure of the QAnon movement, as it allowed Forberg to see how conspiratorial discourses developed, what kind of content and online personas gained traction, and how the network of QAnon influencers attempted to break into the mainstream. This attention to different platforms also gave him a deeper understanding of the digital processes that QAnon members relied upon to spread their propaganda. To maintain his own ethical standards, Forberg did not use his anonymous accounts to gain access to aspects of the community that were not publicly accessible. If he wanted to message followers or join private groups, he used an identifiable account that could be traced to his researcher profile. After a few weeks he abandoned anonymity all

together. While identifying himself as a sociologist did result in the occasional “block” from a Q follower or a nasty direct message, he found that this strategy overall did not prohibit him from access to the community, a point we discuss further in the second section.

In contrast, Schilt did not consider adopting an anonymous or disguised strategy in her analog ethnography of major life changes because this strategy felt unequivocally unethical in this setting. When she began researching the experience of undergoing a major life change, such as significant weight loss, she recognized that she was an outsider to the experience. While her main project was interview-based, she hoped to use ethnographic observations to learn more about the range of experiences people had and the language they used to talk about their own lives before she began interviews. She sought out free support groups, a forum where she imagined she might be able to observe a wide range of experiences from people across social class lines—a similar strategy to Forberg’s initial observation of digital Q forums. Her preliminary research revealed, however, that while there were many free groups, the groups were not open to the public in the sense that just anyone could drop in. Most of the groups were run by a therapist or counselor who decided whether someone fit in the group based on their explicitly disclosed personal experience. This gatekeeping meant that there was no way to come as an anonymous participant. She did look at online forums but found similar expectations around participation.

Schilt had two choices at this stage. She could identify herself as a sociologist and ask to attend the groups as a researcher, a strategy she guessed would yield access to at least one group. The second strategy was to create a cover story related to one or all of the cases she was researching and seek access to the group in disguise. This approach is not without precedent in sociology (Lofland and Lejeune, 1960). But Schilt deemed this second strategy unethical, as we imagine most sociologists today would. Her goal was to attend these groups over time, not as a one-off event. As the norms of support groups typically include encouraging everyone to share, she likely could not stave off contributing a fake experience to a therapeutic setting for more than a few weeks. Contributing a fake experience in a setting where people shared deeply personal experience could generate a false sense of connection—which might be good for research but was bad for Schilt’s ethical sense of self. She could have unwittingly provided erroneous information about a medical procedure or created anxiety by sharing a story that was too negative or too positive. Even if no immediate harm would come to support group members during the meeting, they could feel a deep sense of trust violation after the publication of a book or article. Schilt decided to adopt the first strategy of asking for access as a researcher. Most of the time, the group leader decided not to admit her as an observer. But she did get access to a few groups, which we discuss in the next section.

We do not offer these two examples to make a case for or against anonymous research, disguised research, or deep cover. There are many examples in sociology of ethical, disguised research in workplace ethnographies. However, these ethnographies are a far cry from a researcher pretending to adhere to the beliefs of a cult and moving in with them—a strategy that is likely to bring emotional duress to the researcher regardless of the quality of the data. Our point is that there are context-specific expectations about

anonymity and privacy that should inform the balance between gathering the richest ethnographic data possible and doing the least harm to the researcher and the respondents. In Forberg’s work, user anonymity was the assumed pretext for engagement on Twitter while Facebook users expected names and profile pictures to correlate to a real person. In Schilt’s work, support groups members could be anonymous in the sense that they only used first names in the group. But the expectation in these settings was that people shared a personal experience, such as weight loss surgery, in common. This assumption that people are who they say they are shapes most interactions in face-to-face settings, even though the amount of personal information people have about each other might be minimal (Garfinkel, 1967). In contrast, the default assumption that people are good actors with a shared sense of reality in face-to-face interactions does not govern most digital spaces. And those assumptions matter when considering the ethics of anonymous and disguised ethnographic research.

At the same time, anonymous or disguised research that carries low ethical risk cannot be assumed to generate richer data than no cover or a shallow cover. If we set aside the ethics of Schilt’s case, adopting a disguised strategy would have generated *more* data as she likely would have had access to a wider range of support groups. But it is an open question as to whether she would have gotten thicker data. While she can never know what she was unable to access by being known as a researcher, she still gained a nuanced understanding of the ways that people talked about their experiences of major life changes across several settings—the goal of her ethnographic project. In Forberg’s case, the expectation of online anonymity in his setting created ready opportunities for preliminary reconnaissance of the QAnon community. But this strategy did not generate deep ethnographic inquiry over time. On Twitter, where bots were common and sometimes indistinguishable from highly active, real QAnon followers, this technique mainly gathered public stories that lacked nuance. Without engaging community members, accessing private communities, or breaking the public veil of the community’s activity, Forberg’s initial research was akin to a content analysis of a living archive. Forberg used this tactic to learn more about the way that digital platforms functioned within the QAnon movement than about participants’ experiences of the conspiracy theory—indeed, interviewing participants often demonstrated how misleading internet users’ online personas can be (see Bail, 2021). If we were to set ethics aside once again, Forberg would have likely gathered *different* data by faking his way into private QAnon spaces—especially those run by viral promoters or white supremacists—but the salacious nature of this data may not have been any thicker or more useful to his research questions. Even as a self-identified researcher, he found that the more extreme QAnon followers he interviewed were comfortable being candid about their fringe beliefs and practices, and about their negative interpretations of him.

The fact that many digital spaces enable greater flexibility in self-presentation than analog spaces does not mean that ethnographers should always be taking advantage of this flexibility. The appeal of digital disguises is evident: In describing researchers’ *ethnographic toolkit*, Reyes (2020) highlights how ethnographers’ “visible (e.g., race/ethnicity) and invisible tools (e.g., social capital)” (p. 221) can be *strategically* employed to relationally access field

sites and build rapport with participants—opening, closing, and keeping open doors.⁶ Anonymity is a viable strategic decision in both analog and digital spaces, as Schilt could attend conferences and Forberg could browse web forums without making their identities known. But a researcher who attempts to enter a physical space for which they do not have the expected “visible tools,” such as a woman seeking access to a bathhouse that caters to gay men, will likely face barriers. Applying Reyes’ toolkit to digital ethnography, however, it would be easier to overcome these barriers by *strategically* creating profiles that establish the researcher as an insider. Yet, this strategy too creates ethical dilemmas for the ethnographer, who we expect to begin developing relationships, building rapport, and conducting interviews—but now through this insider caricature. While we believe that there is room for playing with identity in digital ethnography, especially since this already strategically occurs in analog ethnography (Reyes, 2020), such a strategy is not necessary for gaining quality data. For us, once an ethnographer uses a disguised persona to engage with participants, he betrays the “principle of care” (Boellstorff et al., 2012) owed to participants and increases the risk of doing harm to the community he studies and himself. We turn now to these thorny issues of participant engagement in analog and digital ethnography.

3.2. Smash that like button! Liking and lurking in digital ethnography

The feature of ethnographic research design that distinguishes it from other qualitative methods is some degree of researcher participation in the particular social world or field of interest (Emerson et al., 2011; Small and Calarco, 2022). The role of the ethnographer, which can range from a peripheral observer to a complete participant, structures interactions in the field in ways that shape the type of questions she can ask and the data she is able to collect (Adler and Adler, 1987; Fine, 1993). Some ethnographers have made a case for embodied “carnal” participation in a field site—a “sociology of flesh and blood” (Wacquant, 2015, p. 1). Rather than observing how people train as boxers, for example, a researcher can use his body as a research instrument by training as a boxer alongside other community members (Wacquant, 2015). Embodied ethnography offers the promise of a closer approximation of habitus development in the Bourdieusian sense. As the ethnographer-turned-dancer sees his body and instincts change through training, for instance, he learns something about unconscious embodied practice that can be difficult to get through observations alone or interviews (Hancock, 2013). This approach is infused with assumptions about ability and access, however, that are rarely acknowledged. There is also a gendered and racialized component to the reception of carnal sociology. White people (mostly men) engaging in embodied ethnographies in urban settings receive an ethnographic premium in which they are lauded by the discipline for getting their hands dirty, so to speak, with deviant or criminal subcultures (Chancer,

1993; Small, 2015). In contrast, women engaging in embodied participation in a sexualized field site, such as a strip club or a hostess bar, face prurient questions about how far they went to get good data (Frank, 2002; Hoang, 2015). The pressure to prove oneself as an ethnographer through intensive in-person participation can compound the sexual harassment and violence that minoritarian ethnographers face in their research settings (Hanson and Richards, 2019).

Embodied participation can enable the researcher to become the phenomenon of study to some extent—though how close that approximation comes is an open question. The researcher is always a tourist in the sense that he returns to his home to write notes and can exit the world of study. He may be marked by his engaged, embodied participation with, say, new skills or new tattoos, but his simultaneous doing and observing separate him from the people he studies even if he shares their positionality to some degree. There also are ethical limits to what we can know as ethnographers and how we might go about this knowing. These ethical limits are, perhaps, more clearly delineated in analog ethnography because of the length and depth of these conversations in the discipline. To this point, we work briefly here through an example from Schilt’s analog study of how people experience significant weight loss after a surgical procedure. Schilt could not become the phenomenon in this case because she did not qualify for weight loss surgery. She also did not attempt to approximate the experience of being heavy as some ethnomethodologists have done to learn more about the experience of being differently abled (Goode, 1994). Recognizing both the ethical quandaries and physical impracticality of an embodied form of participation, Schilt elected to attend support groups for people who had undergone weight loss surgery. She found that the experience of significant weight loss in a relatively short period of time made many group members “practical methodologists” (Garfinkel, 1967) in that they thought deeply about how this weight loss transformed their habits—how they ate, how they exercised, how they dressed—and their habitus—the way they inhabited their bodies and how they navigated physical space.

As the concept of a sociology of flesh and blood is distinctly analog, a prioritization of the carnal may seem to leave digital ethnographers out in the cold. From the cyberpunk world of Gibson’s (1984) *Neuromancer* to Mark Zuckerberg’s utopian vision of the 2020s metaverse, the allure of virtual worlds for many people is the ability to leave the body and its infelicities behind. But the digital ethnographer is still embodied, whether working on a laptop or participating in virtual interactions. Even the most basic forms of participation in online communities, which are text- or emoji-based, “liking,” retweeting, or upvoting a post, comment, or video, are embodied interactions. Digital platforms use these interactions to determine the spread of content across their site and to promote similar content to their viewers. YouTubers or Instagram influencers who have a monetized channel or page encourage this sort of participation from viewers, often ending videos with a loud, “Don’t forget to smash that like button!” to generate more subscribers. A digital ethnographer can use these forms of online participation as a way into a setting, to build connections with community members, or to study the recommendation algorithms generated by user engagement (Forberg, 2022). Researching speed-running communities organized around the video game

⁶ For digital ethnography, we can also think about “audible” tools, as many networked social media sites allow for voice chat.

Super Mario Bros, for example, a researcher could watch videos of runs on YouTube. He might subscribe to the channel of a speed-runner and comment on videos as a way into the community, or he could like speed-running videos to see what else gets recommended to him from the YouTube algorithm. In this case, the ethnographer is still making decisions about his degree of participation in a setting based on the local norms and practices, but how he is able to enact that participation is shaped by the particular digital platforms he is working through. And none of these forms of participation get at the habitus of the speed-runner, which may require the incorporation of an autoethnographic embodied approach as seen in early sociological studies of videogame play (see [Sudnow, 1979](#)).

Anonymity norms in online spaces also facilitate a form of unintrusive participation that can be difficult to achieve in analog settings: lurking, the act of creating a profile on an internet forum or social media site but rarely engaging with others ([Nonnecke and Preece, 2003](#)). Lurkers may want to learn more about a medical condition, for instance, or like to follow the drama of the Twittersphere without fear of drawing ire over an unwitting comment. Embedded within communities of hundreds or thousands of users and followers on an account or on a platform, online lurkers may attract notice only from market researchers curious about how to monetize their attention. In contrast, analog lurking, which we imagine as observing interactions in a setting without engaging anyone, can raise immediate concerns from the people being observed. Sending students to take field notes in a grocery store or on public transit has long been a training exercise in sociological methods courses. But, with the constant refrain of “if you see something, say something” echoing across urban spaces in the U.S., this exercise can be rife with potentially dangerous misunderstandings. People may assume that a woman observing a group of children at the park is a mother or babysitter but immediately challenge the legitimacy of a man in the same space. Ethnographers of color, disabled ethnographers, and trans or non-binary ethnographers can face scrutiny and harassment from customers and security guards during a public observation exercise that white, able-bodied, cisgender researchers are less likely to face. This is not to say that analog ethnographers do not adopt degrees of lurking in their research. Such a tactic may be used in settings where voyeurism is a legitimate community role, such as in bathhouses catering to gay men ([Tewksbury, 2002](#)), or where many people are simultaneously observing an action or event, such as a protest ([Tufekci, 2017](#)). But the feasibility of this approach is always shaped by the formal and informal social rules of the space and the positionality of the researcher vis-à-vis other people in the setting.

The boundaries between lurking and peripheral participation are fuzzy in digital ethnography, where a researcher can upvote a Reddit post or follow a Twitter user without making a textual contribution to a setting. While it may be easy to identify active participatory acts—such as direct messaging forum members or commenting on YouTube videos—the boundaries between lurking and peripheral participation online require sensitivity to social context and an awareness of the specific functions of a given digital platform. Anonymously browsing the static, archivable forums typical of the early internet may feel hardly participatory, while the

responsive, ephemeral nature of algorithmically-driven platforms such as TikTok makes any time spent online a fleeting ethnographic opportunity—yielding relational data that is dependent upon the ethnographer’s presence and engagement with socially-networked systems. In Forberg’s QAnon research, he transitioned from anonymous lurking to known participation over a short period of time. While he initially adopted an anonymous username, his presence in multiple Q forums became a source of suspicion and confusion among already suspicious participants. Forberg had started to direct message some participants to request interviews, at which point he disclosed his name and his university affiliation. As information traveled quickly in these forums, he made the decision to put his name on his profile for consistency. Prior to making this change, he did his own research into what information was publicly available about him in case he was doxxed by a community member and prepared himself for a backlash. Some people did look him up and post information they found about him on the forum, namely that his thesis adviser was a gender theorist (gasp) and that his research had been funded by an organization that sounded Jewish to the poster. A few times he received angry tirades in his direct messages about his presence as a researcher. More frequently, he was blocked by people he reached out to, likely due to his association with what Q followers saw as a liberal academic institution.

While abandoning his anonymous persona opened up Forberg to angry messages and limited his access in some ways, he felt that it made for more genuine engagement with potential interview participants who could ask questions about his background and work. Further, the few tirades he did receive from angry Q followers provided new insight into the social performances of QAnon—specifically how followers defended themselves from perceived threats and upheld their belief in Q as indicative of moral and intellectual superiority. Over time even some of the initially angry respondents agreed to an interview due to an appreciation for his curiosity and honesty. At the same time, Forberg’s experience with this approach is inseparable from his positionality vis-à-vis the field. QAnon is a predominantly white, cisgender community, and members likely read Forberg to be “like them” even if he was attending an elite university. His visible status as a white man on his profile pictures allowed Q followers to project their own values onto him. Some assumed he was an ideological ally and joked at the expense of academics and progressives, while others recognized him as a distinct outsider and found it interesting to compare their beliefs to his own. This plasticity of Forberg’s identity via participants’ interpretations of him granted access to the QAnon community that another researcher with different social characteristics may not have gotten. People in minoritarian communities face far more online hatred and threatened violence than white, cisgender men ([Vogels, 2021](#)). We use Forberg’s research, along with other studies of internet trolls ([Phillips, 2015](#)) and darknet users ([Barratt and Maddox, 2016](#)), to demonstrate that being known as a researcher in a fringe digital space can generate rich, interactional data that we see in many analog ethnographies. But this strategy comes with the risks associated with having a presence of any kind on the internet and the possible risk to the ethnographer from online abuse should be considered at the start of the study and continually re-assessed over time.

Schilt's role in support groups also ranged from peripheral participation as a known researcher to anonymous lurker but carried less personal risk. Unlike Forberg's case, Schilt's connection to an elite university lent her credibility, even when she was not given access to a group. When she did attend a group, the group leader would introduce her as a sociology professor in her first meeting or ask her to introduce herself. During meetings, she adhered as much as possible to the norms of the setting. If she observed people taking notes, she took notes. If no one took notes, she made minimal jots and then recorded voice memos in her car after the meeting. As a white, cisgender woman in multiracial and mixed gender spaces, she did not draw much attention with her presence. She nodded when people spoke, laughed at jokes, and smiled, all forms of common peripheral participation in support groups. To a newcomer who missed her initial introduction, she could come off as just another participant who never spoke (an analog lurker). When possible, Schilt introduced herself to newcomers to make her role clear. And if someone asked her a question that assumed she had a shared experience of weight loss, she quickly explained her presence as an outsider. Over her time in the group, regular attendees began to engage with her more during the free time before and after the meeting, referencing events from previous sessions she had witnessed or making jokes with her. Once when Schilt missed a meeting, a member of the group contacted her via email to tell her a funny story about something that happened that night, demonstrating that her presence had become expected. Schilt felt that her peripheral participation allowed her to make connections with people, and to gain an in-depth understanding of how significant weight loss shifted people's sense of self and social interactions. People knew who she was, so they could have sent her negative emails or text messages if they wanted to. She did not experience this, however, likely due to the group leader vouching for her at the onset and her lack of verbal participation.

We end this section by thinking through the ethics of participation in digital and analog ethnographies. While analog ethnographers may push for a sociology of flesh and blood, it is widely acknowledged that some forms of embodied participation, particularly disguised participation, are unethical. Returning to Festinger et al.'s (1956) study of a doomsday cult, it is hard to imagine a research design today that would send graduate students to participate as cult members—particularly when that participation took the form of pressuring new recruits to give up their worldly possessions and their children in preparation for the end of the world. While you as the researcher could be fairly certain the world was not going to end and that your assistants could leave the cult after doomsday failed to happen, the stress you caused your assistants and the harm they may have caused to cult members would not outweigh the empirical findings of your study. A known researcher engaging in embodied ethnography can bring harm to community members, such as sexual involvement with respondents that results in an unintended pregnancy (Goode, 2002), or to themselves, such as getting a broken nose in a sparring session at the boxing gym (Wacquant, 2015). Our point is that we have developed more of an ethical barometer for what we should and shouldn't do in analog ethnographic participation than we have developed for digital ethnography.

Such an ethical barometer for digital ethnography is difficult to assess. Some of the major questions of digital social science research are about the extent to which digital actions can cause “real world” damage. The unique affordances of the digital environment means that even peripheral participation or just a few likes here and there can cause societal harm at a scale unimaginable in analog research. Liking a post, as @Peterforberg or @sexyboi47, pushes up this content online, making it more accessible to others in their feeds. If you are studying K-Pop stars, reposting a popular video or liking the account of a singer may be an innocuous drop in the ocean among the digital engagement of the 89 million fans of the genre. Retweeting posts from racial justice movements, in contrast, may feel like a way to do a more progressive form of participant ethnography that gives back to community activists by publicizing their work. These same forms of digital participation look very different for researchers studying far-right movements and hate groups. As Forberg discovered in his research, QAnon content typically includes scientific misinformation about vaccines or an overview of the type of government conspiracies about election theft that spurred the January 6th Insurrection at the Capitol Building in 2021. Liking a QAnon post on TikTok, Instagram, or Twitter may contribute, however marginally, to that poster's broader success on the platform, monetarily reward the poster, and vindicate the poster's beliefs. The risks of such participation are exacerbated by the possibility that the ethnographer has adopted an anonymous persona where they may be expected to engage in potentially harmful activities. In analog ethnography, IRBs would likely take issue with a researcher picketing for an anti-vaccination protest, even if this protest was seen by only a handful of people. To push the comparison further, for QAnon, the most carnal or embodied form of participation—developing conspiratorial interpretations and posting recruitment material online—is eerily similar to the analog 1950s doomsday cult recruiter, with the exception that this performance could happen almost completely in physical isolation. For digital ethnography, then, we must investigate more fully the ethical line around peripheral and embodied participation, such as retweeting an anti-vaccination video or liking an anti-vaccination post, that can spread misinformation at a large scale. While internet users may adopt anonymity or active disguises with impunity, we believe that researchers should hold themselves to a higher standard when it comes to making decisions about participation in digital ethnography. We should always ask ourselves what effect our disguise or persona could have in these virtual communities to avoid reifying the fallacy that what we do in the digital world is less real or impactful than what we do in the “real world.”

4. Conclusion

In this article, we pose the question, “What is ethnographic about digital ethnography?” Working through our cases and examples, we suggest that a digital ethnography should share the same set of techniques as what we call an analog ethnography—namely participant observation in a social world that occurs over an extended period of time. Whether an ethnographer is following

a hard-to-reach digital community that has no analogous in-person setting, participating in a social world with people whose lives blur the distinctions between on- and offline, or studying neo-Luddites who do not use technology, she should aim for the direct contact or “exposure” with the social world and its people that is the defining feature of ethnographic research (Small and Calarco, 2022). This is not to say that digital settings lack unique forms of social interaction that are worthy of study. We think, for example, of the prevalence of anonymous death threats on social media that happen at a scale and volume we do not see in face-to-face interactions, or of the long life of poorly worded social media posts that can never really be erased from the internet. The rise of social media platforms, smartphone usage, and user-generated content also creates new forms of social artifacts that are ripe for sociological analysis every day. But we argue here, qualitative research done through digital platforms is not by default digital ethnography. We make this point, following Lamont and Swidler (2014), to encourage “methodological pluralism against methodological tribalism” (p. 154). As they remind us, “the selection of methodological approaches should depend on the questions being pursued—different methods shine under different lights, and generally have different limitations” (p. 154). Digital discourse and content analysis can tell us a great deal about temporality and historical change over time through rigorous analyses of social artifacts, whether this be Reddit forums, video game guilds, or Instagram posts. These methods do not allow for—nor do they need—the extended researcher co-presence we would expect from an ethnographic project to satisfy their epistemological stance. Even real-time observation of such digital spaces does not necessarily produce relational data between the ethnographer and participants.

We offered two questions that we think ethnographers must consider regardless of the modality of their research: First, is the ethnographer’s identity as a researcher known to others in their field? Second, what is the ethnographer’s role in the field? Decisions about how to present oneself and how to participate are necessarily informed by one another, as well as by ethical, epistemological, and contextual concerns. Juxtaposing Forberg’s digital ethnography of QAnon and Schilt’s analog ethnography of support groups, we have shown how digital ethnography raises familiar questions about research strategy at the same time that it provides new affordances for self-presentation and co-presence. What the researcher seeks to know, where she looks to answer this question, and how she understands the norms and expectations of this setting should shape her approach to self-presentation and degree of participation in digital and analog work. A decision about how to present yourself in the field can change over time, as Forberg’s research shows. And, in some cases, deep cover is not possible or ethical, as Schilt’s research shows. What unites both approaches in our view is a careful consideration of the epistemological stance of the research, the possible risk to the researcher depending on her approach to self-presentation and co-presence, and the possible harm to respondents, the broader discipline, and society at large that a study poses.

To us, ethnographers across modalities must be aware of the self-presentation norms of a particular setting. Anonymous

lurking online may more closely align with some digital community norms (Ferguson, 2017). Yet, the internet is also becoming increasingly authenticated, with users posting under real names and providing searchable, personally identifiable information (Barratt and Maddox, 2016). The increase in reverse image searching and widespread availability of tools for doxxing also means almost anyone is “unmaskable” on the internet. The growth in government and personal security cameras also limits analog anonymity, as we saw in cases of the police using facial recognition software and crowdsourcing video footage to identify protesters during the social unrest around racial injustice in the summer of 2020 (Vincent, 2020). And, in any form of disguised or anonymous research, anonymity evaporates as soon as data is published online. While researchers may strategically create profiles online that provide access to communities and help build rapport with participants, we caution against developing fake personas when trying to understand real people. Instead, we suggest that participation should focus on building relationships with participants, learning about their practices through them, and participating in acts such as content creation or video game playing when it is appropriate and meaningful to do so—which in many cases will require that the ethnographer makes herself known to the participants as a researcher.

In building known relationships with participants, ethnographers should also be cognizant of the behavioral norms in their research setting and be reflexive about how their degree of participation—whether that is lurking, liking, or tweeting in a digital space or observing, nodding, or engaging in an analog space—can transform the group dynamics. Lurking online or observing a physical space anonymously over time can be a diagnostic tool that helps triangulate other forms of data (Duneier, 1999), or it can be the main source of data about a community. In many digital spaces, peripheral forms of participation such as liking, following, or viewing often are appropriate to the norms of the setting, and can produce valuable data about how digital systems work and respond—especially in algorithmically-mediated environments where any degree of presence necessarily incorporates users into the algorithm’s all-consuming logic. However, in certain spaces, such as in deviant digital spaces, the ethnographer may be liable for supporting harmful content and vindicating harmful users, or, if using a disguised approach, be pushed to produce harmful content akin to 1950s graduate students encouraging new cult members to give up their worldly possessions. In our view, decisions about self-presentation and co-presence should not be made on the basis of what is possible in a setting but rather on the basis of what is both ethical and efficacious for gathering quality data.

We end this article with the acknowledgment that our distinctions between ethnographic research modalities may seem to some readers already outdated. We use “digital” and “analog” as heuristics but recognize the impossibility of a firm distinction in many social contexts. As the many works cited throughout this piece demonstrate, the use of the term “digital ethnography” does not foreclose mixed-methods techniques that work across digital and analog contexts, leverage big data, or study drastically

different digital environments—as the ultimate goal is finding the method that will suit the research objectives. However, at this moment, in proposing digital ethnography vis-à-vis analog ethnography we hope to both elevate digital ethnography within sociology and to reiterate that present social contexts require intentional approaches to understanding the role of digital systems in the social world. We can envision a future in which the proliferation of digital technology has rendered such distinctions between modes of research entirely moot—perhaps in a universe where the proposed “metaverse” replaces analog spaces such as schools and office buildings. In this version of the future, all ethnographic research might be hybrid. But, as we write this in 2023, many of the institutions that produce and maintain the rampant structural inequality in our society, such as prisons, public housing, or financial institutions, remain difficult to study without physical co-presence. Further, the most vulnerable people in our country, including people who are incarcerated, elderly, unhoused, or living in deep poverty, have difficulty maintaining a digital presence through a smartphone, a social media account, or even email, making digital forms of research less possible or applicable to large parts of the social world. Our suggestion is that ethnographic research in sociology should fit the reality of the people’s lives we are studying, adopting a single-mode or hybrid approach as appropriate to the context. Hybrid ethnography may indeed be the future, but we would do well to remember, paraphrasing science fiction writer William Gibson, that the future is never evenly distributed.⁷

⁷ As Kennedy (2012) notes, this quote may be apocryphal, but it sums up much of Gibson’s philosophy in his writing.

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Author contributions

All authors listed have made a substantial, direct, and intellectual contribution to the work and approved it for publication.

Acknowledgments

We are extremely grateful to editors Colin Jerolmack, Alexandra Murphy, and Victoria Reyes, as well as to this article’s reviewers, for their constructive and thoughtful feedback on earlier versions of this article. We also wish to thank Forrest Stuart, Patricia Richards, and Patrick Jagoda for their contributions reading and discussing this article.

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OPEN ACCESS

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RECEIVED 30 January 2023

ACCEPTED 22 May 2023

PUBLISHED 09 June 2023

CITATION

Su PN and Su PH (2023) Discomforting surplus: gender, sexualization, and omissions in ethnographic fieldwork.
Front. Sociol. 8:1154435.
doi: 10.3389/fsoc.2023.1154435

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Discomforting surplus: gender, sexualization, and omissions in ethnographic fieldwork

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As sisters and sociologists, we shared an unnerving experience of sexual harassment in one of our preliminary field sites. Our research pursuits split thereafter, with one of us leaning into questions of gender and sexuality and the other steering clear. Despite our diverging interests, we both encountered discomforting moments that raise questions about what data we render surplus in our analysis. In this article, we draw on ethnographic and interviewing data from our respective projects to conceptualize “discomforting surplus” as ethnographic data that we omit from our analyses. We offer two types of discomforting surpluses: those that reveal dissonance between our actions and self-conceptions, and those that seem not just uncomfortable, but inconsequential. We mine these discomforting surpluses, calling for introspection about our subject positions and the potential benefits of trying out analytical frames we have ignored. We conclude with practical suggestions for reflecting meaningfully on our relationships to the field and engaging in thought experiments that center discomforting surplus. These contradictions, omissions, and unnerving questions in ethnographic research are important to grapple with as we encounter a push for greater transparency and open science.

KEYWORDS

gender, sexual harassment, embodiment, data surplus, positionality

1. Introduction

Hoping to avoid the “ethnographic fixations” (Hanson and Richards, 2019) on solitude and danger, we traveled together to Berlin for the first time in July 2013. Phi was undertaking preliminary fieldwork for a dissertation in sociology, and Phung was about to begin her doctoral training in the same discipline a month later. We are sisters who grew up in the same household and studied in the same grade and, often, the same courses, until college. As siblings, we largely shared a subject position, a relational set of characteristics that included young (in our mid-20s at the time), Vietnamese American, bilingual (English and Vietnamese, with faltering German), heterosexual, and cis woman. We had also spent time in Germany in 2004 as part of a high school study abroad program. As teenagers, we experienced Germany as profoundly transformative, a respite from the intergenerational, intercultural tensions and unsettledness of our working-poor migrant household in a Los Angeles suburb. Nearly a decade later, we excitedly plotted our return to Germany.

We thwarted solitude on this much-anticipated trip, but danger found us. Phi was interested in relations between Vietnamese who had arrived in former East Germany as contract workers and those who had come to the former West as refugees. Because of our southern Vietnamese, forced migrant backgrounds, Phi anticipated having a trickier time gaining access to former contract workers, whom the media depicted as northern (Schubert, 2004). We therefore planted ourselves in the eastern part of the city near Dong

Xuan Center, a bustling wholesale market where Vietnamese former contract workers and new arrivals congregated. “Bustling,” though, is in the eye of the beholder. Though Phi had read descriptions to that effect, the market and the surrounding district of Lichtenberg felt to us inert: Dong Xuan Center fell outside of the railway that circled Berlin’s core, and the buildings in the east appeared to us a series of undifferentiated concrete slabs. In contrast to the throngs of tourists around Alexanderplatz, the area around Herzbergstraße appeared deserted as we later exited the gates of the large market complex. Against this gray backdrop, with few signs of life save for the occasional metal humming of the tram passing through, we were stalked in broad daylight for several blocks.

Though the exact details elude us now, our pursuers appeared to be a group of young men, some with shaved heads gesturing lewdly and catcalling us. Realizing they intended to approach us, we broke with our typical leisurely pace. Practically leaping across the empty street, we hoped our chasers would give up, but their echoing jeers trailed us down the block. As petite women (4/11” and 5/4”) in a city we could not fully navigate in the native language, we panicked. We ducked into a gas station but, lacking the vocabulary to explain our situation and with no excuse for loitering, we reluctantly headed back outside. There, the men had been waiting at a distance, and continued their chase. We sped up again and turned off Herzbergstraße, uncertain of what to do should they catch up with us. Hearing their voices booming around the corner we had just turned, Phi frantically grabbed a loose brick sitting atop a pile of rubble on the curb. Wielding it belligerently in her hand, Phi recalls shouting, “I’m gonna fucking bash their heads in!” With Phi’s death grip on the brick, we rushed the remaining few hundred meters back to our hotel.

Recounting this confrontation proved difficult—and here’s the rub—because Phi, intending to abandon her project after repeated instances of sexual harassment during this trip, disregarded the cardinal ethnographic task of recording notes. Instead, we offer this timeline of events from messages we shared in writing with others much later and from reconstructing our route from Dong Xuan Center to the gas station and our (no longer operational) hotel using Google Maps. Yet unrecorded does not mean forgotten.

Long after the adrenaline and fear dissipated, our discomfort remained. Discomfort at having felt weak because of our size. Discomfort at being unable to seek help because of our insufficient language skills. And, most frustrating of all, discomfort at being confronted with feelings of powerlessness in the face of sexual harassment. The trip would spark an ongoing conversation of our (mis)adventures as gendered, sexualized, and racialized bodies moving through space. However, these vexing experiences and our subsequent conversations about them seemed extraneous to the research because Phi was interested in ethnic politics and community formation, not in gender and sexuality.

Though Phi would eventually revive her research in Berlin, she left that initial visit wary of situations obviously imbued with sexualization; meanwhile, Phung leaned into questions of gender, sex, and mobility. Our shared encounter of sexual harassment in Phi’s field site motivated Phung’s exploration of sexuality in relation to reproduction and women’s bodies. Significantly, we both selected field sites where we were not readily marked as racial others. Phi focused on cultural and religious organizations

attended predominantly by Vietnamese migrants, and Phung would eventually immerse herself in the study of brides from rural Vietnam to Taiwan and South Korea, in the process also engaging with the Vietnamese men they left behind. To some degree, our site selections mitigated the racialized aspects of sexual harassment, though, as we discuss later, our national differences continued to mark us. Yet because of her topic of study, Phung frequently had to humor sexualizing comments and gazes in conversations with matchmakers, marriage brokers, and other male interlocutors. She rehearsed a gendered role rooted in non-threatening femininity and submission to gain access to potential gatekeepers. Doing so meant she felt viscerally the “costs of conducting the kind of ethnography that does not conform with feminist expectations” (Hoang, 2015: 192). But rather than treating this as a split of our true and false selves, we take this opportunity to explore how discomfiting situations in the course of fieldwork reveal situational selves, including selves we may not recognize (Verdery, 2018).

In this article, we consider how uncomfortable ethnographic encounters—henceforth, discomfiting surplus—can deepen our analyses. Our starting point is Joan Fujimura’s awkward surplus, those “unanticipated research results that experimenters [in studies of sex genes] recognized as problematic or awkward and that they thus ignored in their final conclusions” (2006, p. 51). As a framing device, awkward surplus invites us to critically reexamine our claims. Hanson and Richards (2019) have usefully deployed awkward surplus to highlight sexual harassment during ethnographic fieldwork. Yet for two reasons that we elaborate below, we distinguish the data surplus of interest to us from what Fujimura considered awkward surplus. First, we offer discomfiting surplus to better aid the analysis of ethnographic, rather than experimental, data. Second, we consider how the inclusion of such surpluses might well strengthen, rather than always undermine, our initial analyses.

With an eye toward the growing call for transparency and open science, we find that exercising reflexivity is crucial for advancing our understandings and practices of ethnographic fieldwork. In our case, we recognize that the projects we subsequently pursued and how we selected our field sites bear the imprint from this episode in Berlin, shaping the questions we asked and the social environments we could comfortably enter. Because we are not alone in pursuing research topics and navigating fieldwork with the remnants of personal experiences and encounters directing our analytical vision, we draw explicitly on prior contributions in the section that follows. In the conclusion, we reflect on how patterned omissions in ethnographic fieldwork pose challenges and possibilities for the push for open science.

2. Discomfiting surplus

We are interested in what data ethnographers omit, as data surplus is built into our research methods. For example, social scientists drawing on large datasets do not make full use of every variable or potential correlation. To do so, particularly before establishing hypotheses, would constitute data mining. It is for this reason, among others, that researchers increasingly offer pre-analysis plans before collecting or beginning to analyze data. Yet

cases that do not fit researchers' hypotheses do not disappear into the void; they continue to inform regression analyses. By contrast, ethnographers and interviewers not only share the data surpluses that quantitative researchers do, but the researcher who uses qualitative methods "also produces the data, such that the data collector is explicitly in the data themselves" (Small and Calarco, 2022, p. 12). Ethnographers and interviewers affect social interactions through our wording and mere presence. Because we are part of the data, they more obviously reflect our interests than, say, a battery of questions posed by the World Values Survey. Our concern here is therefore not *that* data surplus exists in qualitative research, but *how* such surplus might be patterned. Hence, we need to interrogate which data we discard or ignore if we are to take seriously the call for more transparency in research.

Here, we look to Fujimura's (2006) concept of awkward surplus, which she mobilizes to explain discrepancies in scientific experiments that sought to isolate sex genes in mice. Fujimura argues that scientists ignored data that challenged their initial assumptions or hypotheses, including instances in which the supposedly male-determining gene *Sry* actually resulted in more fertile female mice (2006 p. 51). The concept of awkward surplus offers three contributions:

"...first, to help us attend to unanticipated results that are recognized as problematic or awkward by experimenters and are thus ignored in their conclusions. Second, the concept provides an opportunity to reexamine unexpected experimental results either by using different frames or perspectives or by reexamining them in conjunction with data from other sources. Third, the examination of awkward surpluses provides a space where scientists and social scientists can work together in the production of new knowledge" (Fujimura, 2006, p. 71).

Sociologists Hanson and Richards (2019) have fruitfully applied the concept of awkward surplus in *Harassed: Gender, Bodies, and Ethnographic Research*. They observe that as "calls for reflexivity have become more common, they have paradoxically resulted in only superficial acknowledgment of the effects embodiment has on fieldwork" (2019, p. 154–5). The authors offer suggestions toward the valuable goal of reducing sexual violence in ethnographic fieldwork, starting with rejecting the "ethnographic fixations" on danger and intimacy, recognizing all research as embodied, and revamping ethnographic training to center that recognition.

We welcome Hanson and Richard's application of awkward surplus to ethnographic methods, but extend and refashion the concept as discomfiting surplus for two key reasons.

First, discomfiting surplus refers to the embodied experiences of inquiry in the social sciences, whereas awkward surplus refers specifically to experimental STEM contexts. We find that transporting the concept of awkward surplus as such to the social sciences sidesteps what Fujimura saw as a key contribution, which is to allow for collaborations across the natural and social sciences. For Fujimura, this contribution is crucial because debates on topics such as "the biology of sex is too important to leave to biologists alone because they usually are not trained to attend to and analyze how sociocultural frames influence their own experimental processes" (Fujimura, 2006, p. 74).

Rather than stretch the concept of awkward surplus and disregard its call for dialogue across the natural and social sciences, we draw on our experiences as sociologists to show how intradisciplinary brainstorming can similarly illuminate discomfiting moments. Interdisciplinary collaborations within the social sciences may do so as well: one example comes from sociologist Ulrike Bialas and anthropologist Jagat Sohail addressing what they acknowledge to be an uncomfortable question: "If [migratory] flight is so traumatic, how can refugees fantasize about repeating it" (2023, p. 9)?

Second, discomfiting surplus might well strengthen instead of contradict our initial analyses, in contrast to Fujimura's awkward surplus. One example comes from a discomfiting moment that nearly became surplus. In *Divided by Borders: Mexican Migrants and Their Children*, Dreby (2010) recounts witnessing an interlocutor, Efrén, "take a swing at [his wife, Claudia] in front of [the guests]" (2010, p. 58). Efrén then followed Claudia into another room and repeated his transgression. After consoling the couple's oldest son, who had witnessed this violence, Dreby spoke at length with Efrén and "said that he should not worry and that [Dreby] would forget about the whole incident for [her] book" (2010, p. 58). In this instance, an ethnographic event can be discomfiting if its disclosure harms interlocutors reputationally or emotionally. At Efrén's insistence, however, Dreby included the incident. Dreby mobilized this instance to illuminate gender roles and the family tensions exacerbated by restrictive border regimes. The discomfort that nearly became surplus ultimately deepened rather than countered Dreby's analysis. But what about when ethnographic events are rendered surplus because they pose discomfort or harm to the researcher(s)?

Building on Hanson and Richards, we take as our starting point our shared experience of sexual harassment. To borrow from Kathy Davis and Janice Irvine, "our own stories have shown us that silences, neglected feelings, and blind-spots can beset virtually all research areas" (2022, p. 6). The "stigma of *feeling* our research and feeling *bad* about our research" forms what Ghassan Moussawi refers to as "bad feelings" (2021, p. 78–9). Similar to "bad feelings," our concept of discomfiting surplus rejects the binary of "field/non-field... [a separation that] does violence to people's embodied and temporal experiences of research, and reinforces notions of disembodied, privileged researchers" (Moussawi, 2021, p. 80). As a case in point, our shared experience of sexual harassment in Berlin did not stay confined to the physical space of the city. Instead, Phung carried those embodied reminders to later field sites in South Korea, Singapore, and Vietnam. Phi also transported her heightened anxiety of public spaces back home to Los Angeles. There, she made a habit of carrying pepper spray, even in the neighborhood where she and Phung grew up.

Our discomfiting feelings were thus not just internal, but social and relational, affecting how we experienced spaces and situations (citing Ahmed, 2004; Ngai, 2007; Moussawi, 2021). These experiences imprinted us in discomfiting, rather than simply awkward, ways because they defied and redefined how we previously navigated public space and how we understood ourselves. And we rendered them surplus by omitting them from our subsequent analyses. What follows is our attempt to excavate these discomfiting surpluses.

Discomforting surpluses may be patterned in a number of ways; here we offer just two: moments that highlight dissonance between our self-conceptions vs. actions, and moments that we assume are tangential to our key interests. The first maps onto discussions of ethnographers' subject position in the relational fields that constitute our research sites. The second offers a different type of thought experiment—what if I lean into this angle I had discarded, bringing this thread from background to foreground? What does that do to and for my analysis? We elaborate these by first revisiting Phung's performance of passive femininity to highlight discomforting surplus around subject position. We then explore Phi's avoidance of a gendered framing to demonstrate discomforting surplus we deem insignificant for our analyses. Both instances of discomforting surplus remind us of the importance of transparency in our analyses of our own subject positions and experiences in the field.

3. Methods

This article draws on field notes from our respective projects, each on Vietnamese migration but with different empirical questions, interlocutors, and sites. The ethnographic and interview data from Phung's work on gendered patterns of outmigration from rural Vietnam come primarily from her time in two countries, Singapore and Vietnam (the Mekong River Delta region and Ho Chi Minh City). Ethnographies and interviews were conducted between June 2014 to August 2014 and August 2017 to December 2018. In Singapore, Phung conducted ethnography and interviews at two bride market agencies. In Vietnam, she spent time at various locations, such as interlocutors' workplaces, homes, cafes and restaurants in the city, in addition to visiting interlocutors' family homes in the Mekong River Delta region. From there, she carried out semi-structured interviews with 62 interlocutors, 56 men and 6 women. Interviews covered various topics, such as the Vietnamese bride market, interlocutors' personal histories, views about Vietnamese women and gender norms, international marriages, work, and migration experiences. Phung conducted the interviews, which often lasted between 45 min to 2 h, in Vietnamese and English (in Singapore). With her interlocutors' permission, she recorded the interviews, which she transcribed and translated within 2 days from when they took place. Identifying information was edited out to protect her interlocutors' privacy. All names provided in the findings are pseudonyms.

Phi's discussion draws on data from ethnographic fieldwork in Berlin in Summer 2013, Summer 2014, and Fall 2015 to Summer 2016. She engaged in participant observation at two Vietnamese cultural organizations and three Buddhist pagodas. Additionally, she conducted 81 semi-structured in-depth interviews, largely in Vietnamese with some smatterings of English and German. She voice-recorded a majority of interviews and took notes by hand in instances in which interlocutors did not consent to being recorded. Further, Phi shadowed key interlocutors across various aspects of their social lives, sitting with them at their workplaces, translating for them at medical offices, and sharing conversation over home-cooked meals. She has anonymized the data that appears in this article to protect her interlocutors' confidentiality.

4. Results

4.1. Performing unassuming femininity: discomforting surplus that challenges self-conceptions

Among the things Phung left out in her dissertation are the doubts and tension about her own self-conception and how it is tied to the changing forms of power, privilege, and vulnerabilities individuals experience in fieldwork. As Victoria Reyes tells us, researchers carry an ethnographic toolkit, which “consists of researchers' social capital and backgrounds, among other characteristics, and shapes field access, field dynamics, and data analysis” (2020, p. 221). Ethnographers' visible and invisible toolkits can be marshaled to secure data, at times through solidarity and at other times with ambivalence. Shared experiences can facilitate access in ethnographic fieldwork and interviews, as demonstrated by [Enriquez \(2020\)](#) and [López \(2022\)](#), both spouses in mixed-status marriages. Life experiences and roles that we do not consider paramount to our research can also inform how we build trust. This is demonstrated in Nadia Y. Kim's rapport with her environmental justice co-organizers. It was not her organizing background, but her “willing[ness] to endanger [her] baby's health to partake in the movement” that seemingly earned Kim the organizers' trust ([Kim, 2021](#), p. 169). In addition to gaining access, our subject positions also shape what we notice. For instance, Gowri Vijayakumar's new role as a mother during fieldwork animated her attention to mothering in her analysis of sex work in Bangalore ([Vijayakumar, 2022](#)).

The following encounter between Phung and the owner of a bride matchmaking company in Singapore illustrates how she leveraged her relational ethnographic tools to gain access, yet more pressing and perhaps unbeknownst to her is how Phung's analysis of the interplay between masculinity and femininity for her project was informed by the steps she took to access the field and the social scripts she rehearsed to stay in it. It illustrates Phung's ambivalence and doubt about the self-narrative she believed she embodied. As with other interactions throughout her field work that were discomforting and, as a consequence, eliminated from analysis, this example demands further contemplation about the situational selves that we assume in the field ([Verderby, 2018](#)).

I am dressed in a light pink chiffon dress and sit with my legs crossed at the ankles, a picture of demureness. My face is lightly powdered, not too much, but enough to hide any blemishes and project an image of flawlessness and paleness. Opposite me sits Travis, the owner of a matchmaking agency in Singapore. This is my second visit to his agency. Travis tells me about how Singaporean men who come to his agency want Vietnamese women who are skinny and big-chested, proclaiming, “I [am] like Starbucks, high and good quality of class. I don't want low quality.”

I press further to ask him what is considered “good” quality and Travis leans back in his chair, his hands cupping the back of his head as he smiles indulgently at me. His eyes wander up and down my body and settle on my chest for a slight second before settling on my face. He smirks and leans forward, resting his arms on the table to tell me. “Women who look like

you. You understand? Men will pay a lot for you.” I laugh off his comment in an effort to maintain cordiality. Travis’s grin widens as he looks back at me.

Travis approves. He orders his assistant to get some contracts from the drawer to show me how they conduct business. He hands me a copy of the contract with enthusiasm and tells me to keep it for my research. Now that he sees I would not repudiate him for the comments he made about me and Vietnamese women, Travis starts to tell me more about the operations of the bride market in Singapore.

Phung’s field notes: Singapore, August 7, 2014

Although she felt inwardly repulsed and conflicted, Phung knew that her ability to secure a copy of the matchmaking contract relied on her performance as a gendered subject. She enacted a form of femininity that is rooted in the heteropatriarchal expectations of womanhood that Travis deemed appropriate: meek and wide-eyed. It was through this performance that Phung could learn about the workings of a bride market agency and the ideologies about gender, race, and nationhood that shape the preference for Vietnamese brides. In other similar moments, Phung often spoke submissively, always referring to herself by *em*, a pronoun that identifies a younger woman in the company of older people. The pronoun usage in Vietnamese communicates and demands adherence to hierarchies around age, gender, and social status. As such, casual exchanges between Phung and her interlocutors were imbued with hierarchy, as when a younger male interlocutor adopted the pronoun of older brothers (*anh*) as he urged her, “...you should come back and talk to me.”

Beyond interrogating the cultural orthodoxies that might limit our field interaction and inform our view of participants, bringing in discomfiting surplus means purposefully integrating certain elements of our selves that come into conflict in the field. These “embodied costs” (Hoang, 2015) exemplify a dilemma between our participation in the field sites and the ways we behave within them that do not comport with our feminist sensibilities (Avishai et al., 2013; Hanson and Richards, 2019). This leads to questions such as: How do I reconcile my feminist politics with my performance of gender in the field? Am I who people think I am, even if they have misjudged me in some crucial way (Verdery, 2018)? Engaging, rather than shying away from reflecting on such questions, might have sharpened Phung’s awareness of her positionality and made visible the unrecognized power and privilege, as tied to nationhood, that researchers have at their disposal. And doing so explicitly, in the body of her writing, would have allowed readers a fuller sense of the scene.

Had Phung included analysis of the toolkits she utilized in ethnographic field work, it would have clarified the assumptions Phung made about rapport as well as the limits of shared experiences. Initially, Phung believed that rapport would be automatic because of the ethnicity she shared with her interlocutors. But in fact, Phung’s assumptions impeded her from recognizing Glenn’s (1999) insight that we ourselves are carriers of unequal power as we tread through different cultural landscapes. That is, individuals can be part of the majority in one setting and members of the minority in another, and vice versa. This was made abundantly clear during Phung’s first attempt to enter a place of business in Ho Chi Minh City that employed primarily men from

the countryside. It is worth noting that, having accompanied Phi on her first entry into the field in Germany and experienced sexual harassment alongside Phi, Phung elected to bring a companion to her field sites. This came in the form of her then-unemployed and recent college-graduated cousin, who was born and raised in Vietnam and joined Phung especially when she spent time in male-dominated spaces.

“The men are in the back napping,” Tuan, the owner of VT Gas, tells me. I make my way into the kitchen with his wife and my cousin. Tuan’s wife announced to the group of workers, about nine to ten men in their 20s and 30s who are dressed in navy jackets with the name of the company embroidered on their left chest pocket, that I want to interview them. Before she could finish her sentence, the men, who were lounging in the kitchen, started to push one another out of the way in a race to run as fast and as far as possible out the back door. The phrase, “They are scattering like flies” comes to mind as I stand there, mouth agape. They’ve made their way to the outside, and many are leaning against the wall, some pulling out cigarettes and lighting each other’s buds. I asked why they did that and they yelled out different versions of “I’m shy/embarrassed,” “I don’t know how to talk to women.” Tuan’s wife explains that “Rural men without wives don’t know how to talk to girls,” especially educated girls from abroad.

Phung’s field notes: Ho Chi Minh City, November 28, 2017

Given that Phung’s dissertation project centered on human mobility, weaving this discomfiting surplus into the analysis would have revealed insights about unequal nationhood and who has the ability to participate in spatial mobility. Though this group of men was willing to talk to Phung’s cousin, they grunted and shook their heads at Phung despite countless requests from all three women for them to speak with her. Reflecting on this moment now shows how individuals on the move, such as ethnographers, must confront the ways their experiences of minoritization in one setting might not carry into another. This is particularly pertinent to those whose positionality locates them at the intersection of multiple oppressions in the West. Illuminated in this encounter is the disparity that exists between Phung and her potential interlocutors, men with limited education from poor, rural origins who were employed as manual laborers. Her surprise at the men’s behavior highlights her limited understanding of the boundaries of respectability that were made so sorely palpable during this scene. Her inability to conceal her gender, nationality, and education further underscores a moment of rupture between Phung’s assumed familiarity with the men because of her ethnicity and the recognition that she was an outsider with a markedly different set of economic and cultural capital. The power differential between researcher and interlocutors is neatly captured in this image of the men standing on the outside of the house instead of resting in the kitchen during their break while Phung remained in the kitchen, a figure of disturbance and strangeness.

Leaning into this discomfiting surplus and embedding it in analysis of nationhood would illuminate how power differentials between individuals take shape within shared territories and across different national boundaries. Phung’s field notes detailed the

countless times that her interlocutors brought up her identity as an American “overseas Vietnamese” (*Viet Kieu*), such as when one man claimed that he wanted a “*Viet Kieu* girl from the United States to come and marry [him]” or when Vietnamese women in South Korea and Taiwan remarked on how an American *Viet Kieu* inspires more awe from Vietnamese people than a Taiwanese or Korean *Viet Kieu*. However, these moments did not find their way into Phung’s analysis. Similar to Phi, who focused on ethnic nationhood to the exclusion of gender and sexuality, Phung’s analytical centering on gender and sexuality came at the expense of attention to the privileges that individuals embody as a result of unequal nation-state dynamics. Hence, this discomfiting surplus illuminates how nationhood and citizenship shape perceptions of women’s desirability.

Finally, we suggest that mining discomfiting surpluses can alert us to how ethnographers measure privileges as well as possible dangers and vulnerabilities in field research. Here, we build on thoughtful reflection on the researcher in ethnographic fieldwork (Rios, 2011; Cobb and Hoang, 2015; Hoang, 2015; Small, 2015; Reyes, 2018; Hanson and Richards, 2019; Shulist and Mulla, 2022). For example, in 2014, Phung’s initial research plan was to study the Vietnamese bride market phenomenon, which necessitated that she be in conversation with various actors such as brides, grooms, their families, government officials, and matchmakers. Commercial matchmaking is prohibited in Vietnam, and as such, Phung’s access to the matchmakers was contingent on a government official, Binh, an important gatekeeper to access to the bride market in Vietnam, who was working outside the remit of the law. An encounter with Binh ultimately led Phung to shift her research focus.

Binh, a government official in Vietnam, promises to get me access to a house of girls (*nha nuoi gai*). He says that that is where there will be plenty of women and matchmakers to interview. I tell him that I will have my cousin come along with me but Binh shakes his head. He says it would be better if it were just the two of us, so that others would not get suspicious. He proceeds to tell me that he would introduce me as his younger sister or younger woman (*em*), that if I enter any space at his side, I would be able to speak to anyone I want. I insist that it would make me feel more comfortable to have my cousin who can balance my lack of knowledge about Vietnamese phrases and accents. Binh waves this off and tells me not to worry, he would be with me. During this conversation, Binh ignores my cousin, Hien, who has been by my side the whole time. He only speaks to me.

Phung’s field notes: Ho Chi Minh City, August 26, 2014

Although we deploy discomfiting surplus to clarify ethnographic analyses, the example with Binh reveals how analysis is necessarily intertwined with initial access and ongoing data collection. Because Binh was a well-connected and influential individual involved in the bride market in Vietnam, his promise to grant Phung behind-the-scenes access to the workings of matchmaking agencies was exciting. But more urgent was Phung’s concern about safety if she were to be left alone with him. Phung ultimately declined Binh’s offer. Her unwillingness to go along with Binh’s suggestion and his refusal to have anyone else accompany them meant that she lost an important means of connection to a

key population for her study of the bride market, sparking a series of adjustments to her research design. By ceasing communication with Binh, Phung forestalled potential discomfiting surplus. At the same time, her maneuvers to prevent harassment meant that the specter of discomfort to come informed her access, data collection, and subsequent analysis. Through this example, we encourage a more deliberate consideration of our complex and contradictory subject positions, and how they shape questions of safety that guide the type of power dynamics we fix our analytical lens onto. We can further recognize how risk assessments are tied to access as well as the ways in which the glorification of danger ignores important intersections of vulnerabilities that shape individual experiences in ethnographic fieldwork.

In sum, when we omit encounters in the field that compromise our self-conceptions or gloss over the adjustments we make to our research plans, this can create a misleading portrait of a distant observer telling an “objective” story. By bringing discomfiting surplus into our discussion and analysis, we make clear that ethnography does not occur in a controlled setting. Phung’s modification to her initial plans demonstrates how experiences that derail research or drain us actually elucidate the very ways that risk assessment and vulnerabilities mediate social relations. Further, in the process of accessing a field site or disengaging from it, we must view with wary eyes the endless praise granted to the “cowboy ethnographer” in dangerous situations (Hoang, 2015, citing Contreras, 2012). By contrast, discomfiting surplus locates ethnographers, as social actors, at the nexus of gender, class, race, nationality, and ability, which can inform their level of vulnerability, safety, and purview (Hanson and Richards, 2019). By mining this surplus, we are able to better recognize the type of knowledge that receives recognition and the kinds of “truths” that are subsequently produced. By centering discomfiting surplus in our ethnographic analyses, we can better understand the situational selves—selves that are full of ambivalence and contradictions—that we embody and that exist as glimpses into how individual proximity to power and privilege changes in different national contexts.

4.2. Rejecting—and reevaluating—key narrative frames: discomfiting surplus that seems inconsequential

Whereas, Phung rehearsed a mild-mannered femininity that contrasted with her self-understanding, Phi comfortably inhabited that role in the presence of her Vietnamese interlocutors, who were largely elderly and female. Like her sister, Phi grew up in a Vietnamese-speaking household and social environment that relied on kinship pronouns to refer to self and others, rather than less hierarchical addresses such as friend (*ban*). Most of her interlocutors also referred to her through such second-person addresses as niece, older sister, or younger sister. Only rarely did interlocutors refer to Phi as “friend” and to themselves as an unmarked “I” (*minh*), a practice that tries to avoid conveying hierarchy (Sidnell and Shohet, 2013). For Phi and the majority of her interlocutors, the default kinship figures of speech rendered natural the hierarchies of age, gender, and status that organize membership in the nation as imagined family (Seol and Skrentny,

2009). Phi did take note of, for example, how interlocutors instinctively ushered her into gender-segregated spaces. Yet she did not analyze this further.

In deciding to resume research in Berlin, Phi strategized ways to reduce the threat of sexual harassment; she succeeded so much so that she came to regard her earlier encounters as simply bad luck. Phi relied on her plain self-presentation, marital status, and research sites to spare her further harassment. In her everyday life, Phi dressed conservatively and rarely wore makeup. Although Phi did not wear a wedding band, her interlocutors knew she was married and occasionally interacted with her partner when he joined her for outings. Phi also never went out without her trusty pepper spray. Moreover, Phi spread her time across two cultural organizations and three Buddhist pagodas that were attended nearly exclusively by Vietnamese border crossers. She made these adjustments in part because the influx of forced migrants from Syria in 2015–16 meant that the umbrella migrant-serving organization she initially intended to shadow no longer had capacity to accommodate volunteers who did not have Arabic-language skills. As a consequence of changes in her field sites, Phi dramatically reduced the time she spent outside of predominantly Vietnamese social spaces. These precautions succeeded so that, in time, Phi no longer had a visceral reaction to returning to places such as Dong Xuan Center—albeit largely in the company of her White partner.

But as she felt confident that her body was not subjected to a sexualizing gaze, Phi also neglected the ways that other women's bodies were. One example comes from a young woman Phi called Kim, who had arrived in Berlin just a few months before she and Phi met. Kim looked forward to refining her language skills and pursuing undergraduate studies. Like several other young women Phi would come to know, Kim “knew” that she would ultimately have to birth a child on German soil or marry to stay in Germany. Kim recounted knowing this before she went abroad, but hoped that she might delay what she saw as an inevitability so that she could study for as long as possible.

Others in the field site similarly “knew” young Vietnamese women's reproductive fate. During a car ride with a group of aunts and uncles from a cultural organization she had been observing, Phi was uncomfortable hearing them discuss how Kim would lose her figure as soon as she became a mother. They gossiped to the effect of, ‘Her waist is so nice now, but it'll explode after she gives birth.’ Phi was not surprised by the interlocutors' comments about weight and ideal Vietnamese beauty; indeed, the aunts would often lovingly chide Phi for putting on weight during fieldwork even though their cooking and insistence on her overeating contributed. Instead, what caught her off guard was that despite being married and older than Kim, Phi did not receive the same messaging about when and why she needed to have children. Less than a handful of interlocutors pressed about why she had no children, and most simply assumed she would one day. But the aunts and uncles impressed a sense of urgency to and about Kim.

The reproductive imaginaries to which young Vietnamese women were subjected often became realities. In late Summer 2016, as Phi prepared to depart from Berlin, she invited Kim and two other international students, Xuan and Yen, over for a home-cooked meal. Kim, Xuan, and Yen reflected on how much they

had changed socially and politically since leaving Vietnam a year ago. They daydreamed about what transformations and possibilities the following years might bring. By the next year, Kim and Yen, by then just barely entering their 20s, would discontinue their studies and bear children. Xuan would marry, with Phi and her partner serving as witnesses to the union. Shortly thereafter, Xuan became a mother. To be clear, we do not mean to imply that motherhood is something to be lamented. Instead, what is crucial is that these women expressed wanting something different for themselves, but understood reproduction as the only way to secure long-term residency.

These descriptive, narrative threads of gendered paths toward staying in Germany feature in Phi's field notes, but what would it mean to take seriously such discomforting surplus in the analysis? In the book that resulted from her dissertation, Phi focused on nationhood after border crossings. She argued that Vietnamese in Germany still identify as one ethnic (if unequal) nation, but have rejected the nationalistic principle that their shared nationhood requires a shared state to represent them. But if Phi had foregrounded Kim's, Xuan's, and Yen's gendered migration pathways in her previous analysis, she would have more convincingly revealed the ways that nationhood is stratified.

Specifically, Phi might have better recognized how such national stratification is read and expressed differently through the bodies of women. Here, Phi's ability to triangulate and compare with other women in the field offers valuable clues. It was not just education that mattered for how others interpreted women's reproductive obligations or lack thereof. Another woman, a White German PhD student, likewise built relationships with Phi's refugee interlocutors. Yet, like Phi, she did not recall pervasive comments about whether and when she should proceed to the next stage of a heteronormative reproductive life course.

It was also not just citizenship that mattered—or, at least, not just Phi's U.S. citizenship. A prime example comes from another interlocutor, Ina, who was a few years older than Phi and a German citizen daughter of Vietnamese refugees. During Phi's field work, Ina was expecting her first child. Though Ina's parents were excited, neither they nor she suggested that her first pregnancy was occurring unreasonably late in life. What mattered for Ina as well as for Phi was where they ranked in the ethnic nation. Ina's German upbringing seemingly gave her a pass in a context where the average age of first-time motherhood was over 30 years (Janjevic, 2022).

By virtue of being tethered to Western citizenships, Ina and Phi could exercise more autonomy in their reproductive choices without ubiquitous pressure. Because of her Vietnamese American background, Phi was assumed to embody the best of what the nation could offer: educational and economic successes, frankness and sincerity in her relationships (Su, 2022, Chapter 4). By contrast, women who were recent arrivals to Germany or who grew up in northern Vietnam were positioned lower in the ethnic nation. This meant that they were seen as mere vessels for achieving socioeconomic mobility through staying in Germany.

Though unintended, Phi's insistence on streamlined storytelling came at the cost of fuller transparency and reflection about how the construct of the nation is inherently gendered. In her book, Phi focused on how nationhood served as a central organizing principle in the everyday lives of Vietnamese border

crossers in Berlin. Yet she saw this framing as demanding primacy over, rather than complementing, an analysis of gender. As a result, she missed the ways that a gender and sexuality lens could have productively amplified her argument by revealing how understandings of nationhood are mobilized through and enacted on women's bodies. Indeed, striking discrepancies emerge in Phi's field notes, as when an organization she called Refugees for Germany insisted that for the Lunar New Year celebration, women must don traditional silk dresses, whereas men were free to sport Western suits.

Even as she concluded her project, Phi recognized that her omission of an analysis of gender was all the more conspicuous because "state power, citizenship, nationalism, militarism, revolution, political violence, dictatorship, and democracy—are all best understood as masculinity projects, involving masculine institutions, masculine processes and masculine activities" (Nagel, 1998, p. 243). Yet Phi insisted that while she observed the actions of cis men and cis women, she did not study gender and its constitutive relationship with border crossings. But as this thought exercise suggests, the leap from description to analysis need not have demanded an entirely different narrative arc. Instead, an analysis of gender and sexuality beckoned from within the framework Phi offered, and had more to contribute to the analysis than its treatment as irrelevant, discomfiting surplus would allow.

5. Discussion

We have demonstrated how data surplus can manifest through moments that illuminate tensions between our self-conceptions and actions as well as events or threads we assume to be inconsequential for our analyses. The first discomfiting surplus highlights ethnographers' performances of gender, anxieties about vulnerabilities, and unrecognized privileges and power. Such instances remind us of the sociological insight that different situations produce versions of the self, including selves we might not readily recognize. The second discomfiting surplus offers an opportunity to reexamine themes we might otherwise dismiss. It provides a generative platform in which we take seriously the oft-posed question, "What about X," but offer a modification that invites further analysis: "How might the inclusion of X further my analysis of Y?" We consider these discomfiting surpluses, augmenting Fujimura's concept of awkward surplus and building on the work that Hanson and Richards began to mine the insights of this concept for a social scientific and ethnographic audience.

Contradictions, omissions, and unnerving questions in ethnographic research are important to grapple with, particularly as we encounter a push for greater transparency and open science. Yet, as Small and Calarco (2022) contend, calls for open science and reproducibility ask us to judge qualitative methods by the standards of quantitative or experimental ones. Moreover, calls for transparency as an unqualified good ignore the vulnerabilities of the populations with whom many of us work (Bloemraad and Menjivar, 2022). Rather than full transparency, then, we encourage exercises to deepen reflexivity, particularly about data we might be inclined to silence. We arrive at this through demonstrating how deeper reflexivity regarding what we systematically omit can enrich our analyses.

We conclude with two practical suggestions, the first of which concerns when and how we reflect on our relationships to the field and its impact on our claims. Often, we see key, enlightening discussions of subject position exiled to the ends of monographs in a methodological appendix. We offer one example from a celebrated ethnography. Not until the very end of *Evicted: Poverty and Profit in the American City* does Matthew Desmond disclose that he is the "friend" who lent an interlocutor the money she desperately needed. He cites being motivated by the fact that "there is a bigger game afoot," such that his interests lie "in a different, more urgent conversation" about housing policies and the persistence of inequality (Desmond, 2016, p. 335). We can appreciate the delicate dance between making transparent our impact on the site vs. making ourselves the story's center. Yet by removing ourselves from the storytelling completely, we obscure how our subject positions shape our data.

Our point is as much about *whether* as about *how* and *where* we reflect on our subject positions in relation to our analyses. One instructive example comes from Moussawi's fieldwork on LGBTQ formations in Beirut in the context of local, regional, and global politics. He reflects on initially trying to separate the affective from the analytical by keeping separate field notes (2021, p. 81–2) or confining the discussion of "bad feelings" to methodological appendices. But collapsing this distinction between his feelings of unsafety as a queer person in Beirut during episodes of publicized violence allowed him to center "the situation" not simply as a descriptor but as a theoretical intervention" (2021, p. 90). We therefore find that acknowledgment of our subject positions—indeed, of how all knowledge is situated (Haraway, 1988)—strengthens rather than detracts from our contributions.

Our second suggestion is to deliberately engage in conversation and thought experiments that center discomfiting surplus. Dialogue can be intradisciplinary, as between us as two sister sociologists. It can also be interdisciplinary, as between Bialas and Sohail (2023) about their shared focus on forced migration in Berlin. Conversations do not need to be exclusive to academic circles, either. Those of us doing community-engaged research already do share our questions, thoughts, and writing with relevant audiences, and invite their contributions, hopes, desires, and points of contention. Talk of feelings matters as a first step to counteract how "rarely... feelings of shock, irritation, fear, boredom or, for that matter, amusement, excitement and delight find their way into the analysis itself" (Davis and Irvine, 2022, p. 1).

Our fateful brushes in Berlin in 2013 stayed with us long afterwards, intellectually and behaviorally. As Phung began her doctoral studies after that trip, she nursed an interest in the ways that women's bodies are commodified toward social mobility for individuals, families, and the nation. Phi avoided such a study, and only recently stopped clenching pepper spray in public. Though our reactions diverged, the discomfiting surplus of our shared experience of sexual harassment nevertheless informed our interests and expressed disinterests. Calling attention to two ways these surpluses can be patterned, we invite others to reexamine the experiences that, though we render them invisible, still inform our interpretations. This reflexivity nods to calls for transparency while still recognizing the need to protect the confidentiality of our often-vulnerable interlocutors.

Data availability statement

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

Ethics statement

The studies involving human participants were reviewed and approved by University of California Berkeley University of California Los Angeles. Written informed consent for participation was not required for this study in accordance with the national legislation and the institutional requirements.

Author contributions

The authors confirm responsibility for the research design and data collection, to the analysis of the results and writing of the manuscript. All authors contributed to the article and approved the submitted version.

Funding

Our projects received funding from the Berlin Program for Advanced German and European Studies; University of California, Berkeley Institute of East Asian Studies and the Institute of International Studies; University of California, Los Angeles Institute of American Cultures Graduate and Predoctoral

Fellowship; University of California President's Postdoctoral Fellowship Program, and U.S. Department of Education Foreign Language and Area Studies Dissertation Fellowship. We also acknowledge supplemental funds through the Williams College Dean of the Faculty's Office.

Acknowledgments

We extend our gratitude to the reviewers and editors for their helpful comments. We also thank Irene Bloemraad for the nudge to develop this conversation, Weiwei Lu for providing research assistance, and Eli Wilson for suggestions on an earlier draft.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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OPEN ACCESS

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RECEIVED 04 February 2023

ACCEPTED 30 June 2023

PUBLISHED 03 August 2023

CITATION

Ince JI (2023) Between the witness and the
observer: what ethnography can learn from
James Baldwin. *Front. Sociol.* 8:1158520.
doi: 10.3389/fsoc.2023.1158520

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Between the witness and the observer: what ethnography can learn from James Baldwin

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What is the role of the ethnographer during a time of increased racial hostility, political mobilization to keep racial minorities “in their place,” and commitments to revisionist interpretations of the country’s past and projected future? While the traditional, classic ethnographic approach would recommend that the researcher should avoid taking a stance on so-called political matters and merely *observe* them, I argue that that position is insufficient to address the issues that people are currently facing. Ethnography can, and should, do more. Therefore, this essay argues that the role of the ethnographer should be oriented toward what the late author James Baldwin calls *the witness*. The witness is different from the observer because it rejects a positivistic orientation toward ethnographic fieldwork that prioritizes spectatorship to remain “scientific.” To be a witness is to transgress traditional epistemological understandings of ethnography that ignores how the researcher’s position within the racial system shapes how one knows and does not know, what one sees and does not see, and how one imagines freedom and justice. Ethnographers can learn from Baldwin’s method because it provides a rich vocabulary to describe the inequality that research participants encounter while in the field and embraces the possibility of an apocalyptic future, which is a future that is not guaranteed if we continue to seek neutrality. In this article, I detail three lessons that we can learn from Baldwin’s method and status position as the witness: (1) Connecting empire to the global racial order via the international outsider; (2) Paying one’s dues as a within-nation outsider; and (3) Representing the wretched as a within-community outsider. These lessons are instructive for ethnographers because they provide a lens to understand classic ethnographies of the past, while not wallowing in the doldrums of present arrangements, and challenges future research to ground reality as it is rather than what it “should” be.

KEYWORDS

ethnography, racism, reflexivity, method, Baldwin

“You are bearing witness, helplessly, to something in which everybody knows. And nobody wants to face.” -James Baldwin (“The Artist’s Struggle for Integrity,” a speech given at the New York City’s Community Church, 1962).

Introduction

In the winter of 1971, Nikki Giovanni and James Baldwin met in a London studio to discuss the different approaches of their respective generations to the Black struggle for justice in the United States. The near 2-h conversation for a public television show called *Soul!* was rich with insights about race, gender, sexuality, family, work, and identity— and the generational responses to each of these conditions. The first question that Giovanni asked Baldwin was: why did you move to Europe? Baldwin paused, elicited a classic grin,

and provided several explanations. He described the difficulty he had with focusing on the craft of becoming a Black writer while living in the United States and that living in Europe provided him an appropriate distance to grow as a writer.

He also mentioned a pivotal moment: learning about the murder of Martin Luther King, Jr. while living in the south of France. He ended his answer with the following statement (Nikki Giovanni James Baldwin in conversation on 'SOUL!', 2022):

There was no way ever to leave America. I would be a fool to think that there was some place I could go where I wouldn't carry myself with me. Or if there was some way for me to live if I pretended I didn't have the responsibility which in fact I do have. I'm a cat trying to make it in the world because I'm condemned to live in the world.

Baldwin's response is an apt reminder of the importance of leaving what is familiar in order to better understand oneself and one's relationship to one's country. Living in France, quite literally, saved his life. This move allowed him to make sense of his own life and what was happening in the United States during the civil rights movement. The year after Baldwin's conversation with Giovanni, he published *No Name in the Street* (hereafter *No Name*), which is colloquially known as the sequel to his book *The Fire Next Time* (*Fire*). However, the tone in *No Name* was markedly different from his earlier work. Baldwin expressed a deep cynicism—indeed, condemnation—toward his white countrymen, and the American experiment. He forsook the ideas that he presented in *Fire*: that the United States would be able to deliver on the promises embedded within the country's founding documents. Instead, in *No Name*, he turned his gaze toward an international framework that departed from individual solutions to systemic problems. The tone was so piercing that one reviewer asked how “one of the most sensitive writers in the Western world... [could] come to this?” (Ford 102 as quoted in Sinykin, 2020).

It might seem odd to include a discussion of Baldwin in an issue dedicated to sociologists and ethnographers in particular. Baldwin's texts are not included in sociology training programs; it would be a surprise to find his work in courses on sociological theory, method, and even race relations. Nonetheless, the lessons he provided in *No Name* are instructive for ethnographers and the discipline of sociology because, like many scholars who are marginalized, he is not unfamiliar with the pangs of epistemological erasure and the consequences of being misread by the predominately white field of knowledge production. In this essay I will incorporate his insights from *No Name* to show how his work is sociological and that it aligns with other ways of producing knowledge for qualitative researchers. Specifically, I introduce the concept of the witness as a tool that fieldworkers can use while conducting their fieldwork.

Before moving forward, it is important for me to clarify the defining features of the witness. By using the term witness, I am not referring to merely observing phenomena individually, but, more importantly, observing with a pious relationship to history, that is, using one's status position to publicly unveil the hypocrisy of blind reverence to the nation's history and its institutional arrangements. A key argument that I am making is that inhabiting a pious relationship to history is a tool that ethnographers can bring with them as they conduct their fieldwork and write their analyses

(Reyes, 2020). Let me discuss an example that might resonate with readers who are familiar with the sociological tradition. Consider the following statement that Du Bois (2014) provides the reader in his preface titled “To the Reader” in *Black Reconstruction*:

It would be only fair to the reader to say frankly in advance that the attitude of any person toward this story will be distinctly influenced by his theories of the Negro race. If he believes that the Negro in America and in general is an average and ordinary human being, who under given environment develops like other human beings, then he will read this story and judge it by the facts adduced. If, however, he regards the Negro as a distinctly inferior creation, who can never successfully take part in modern civilization and whose emancipation and enfranchisement were gestures against nature, then he will need something more than the sort of facts that I have set down. But this latter person, I am not trying to convince. I am simply pointing out these two points of view, so obvious to Americans, and then without further ado, I am assuming the truth of the first. In fine, I am going to tell this story as though Negroes were ordinary human beings, realizing that this attitude will from the first seriously curtail my audience.

Du Bois had to be explicit about the importance of dispelling the lies that many believed about Black people. But, he makes it clear that he is not writing for readers who believe that Black people are inferior, and that this inferiority is a social, observable fact.¹ In the preface, he does not express reverence for the Civil War or frame slave masters and their allies in the chattel slavery economy as victims of their time whose agency was swallowed up in this economy of social death (Patterson, 1982). Instead, he states that he is writing to honor Black people's humanity. This is not just a rhetorical strategy—this is the execution of a research strategy and an instance of what it means to inhabit the role of the witness.² One

1 See “A Suggestion on the Negro Problem” by Charlotte Perkins Gilman, published in the *American Journal of Sociology*. The (1908) essay begins with the assertion that the “superior” race must find a practical means for speeding up the “racial evolution” of that “large body of aliens, of a race widely dissimilar and in many respects inferior, whose present status is to us a social injury.” Gilman argues that people of African descent were largely incapable of progressing to the level of whites. The “problem,” she noted, was that, “He [the Black person] is here; we can't get rid of him; it is all our fault; he does not suit us as he is; what can we do to improve him?” Astoundingly, she suggests that each state enlist all “negroes below a certain grade of citizenship” into a quasi-military organization that would perform dignified labor for society and thereby develop the work habits and personal discipline that will make them “productive” members of society. This is the type of person that would likely refute the historical account that Du Bois provides in his magisterial work.

2 I am also reminded of Zora Neale Hurston's research strategy in *Barracoon*. Hurston did not elect to interview former slave masters to understand the violent history that Black Americans endured. She understood that there was a concerted effort to silence Black people from telling their own stories and instead replace them with watered down and false narratives about benevolent slave masters. Yet, according to today's publishing standards in sociology, most reviewers would likely argue that her study design is weak or invalid because of her proximity to the subject matter

of the most important decisions a researcher makes is where they begin their analysis, which includes shifting the reader's attention to privilege the narratives that center the humanity of marginalized groups to tell the full story of inequality.

Like Du Bois, Baldwin's writing is prolific because he brings what West (1989) labels "prophetic pragmatism" to understanding social phenomena and the world(s) we inhabit. Throughout his career, Baldwin practiced an intense public intellectualism to remind his countrymen of the routes/roots that have been taken to arrive at this particular cultural moment. In the 1960s and 1970s he forwarded a discourse on Black American citizenship and American democracy to disrupt the traditional American narrative that our arrival here is coincidental or accidental. The pious relationship to history must serve to understand the structures of oppression of the present and imagine and work toward building new arrangements in the future. In this way, he took on the burden of the public witness to propose (re)building new arrangements in lieu of the ones we currently have.

To inhabit the role of the witness entails: (1) reflecting deeply about one's own history and place in the world (one's family, social relationships, and position within the nation's institutions), so that (2) one can do the collective work of participating in the ongoing discourse of "fresh understanding [and] greater resolve" to pursue freedom for all people (Benjamin, 2022). Baldwin sees collective freedom and personal responsibility as inseparable. Indeed, his own definition of freedom required acknowledging personal responsibility, an "honest appraisal of the historical roots, as well as the current conditions, of one's situation" (Balfour 131, as cited in Muyumba, 2014). The witness understands personal responsibility not as an individual accomplishment. Instead, the purpose of acknowledging personal responsibility is as Muyumba (2014, p. 164) describes it: "as a set of contingent practices: *phronesis*—active sagacity, thoughtful action; intellectual experimentation and invention; and rhetoric" so that one can "create exchange and participation among the members of a public community, thus inspiring them to make their individual, personal ideas into *shared concerns and solutions*" (author emphasis).

This is something that Baldwin contributes not only as a writing style, but also as a contribution for ethnography. With his witnessing of the Black American condition, Baldwin made Black American issues, which would otherwise be considered a private, individual concern, a shared, public concern that was inseparable from the nation and its existence. The fate of the country and, importantly, the project of democracy was impossible to determine without also acknowledging Black humanity, suffering, and struggles for complete citizenship.

Drawing on the experiences of the oppressed as a starting point for research and knowledge production is critical for his assessment of the country, his countrymen, and any hope (if at all)

for change. In addition to centering the voices of the oppressed, Baldwin adds another element that is important for ethnographers to remember: you need to make the connections between what you observe in the service of building a collective knowledge that produces sociopolitical changes. Ethnographers who are interested in doing the work of the witness must produce knowledge in the service of pushing democracy toward radical change. Our work must be created for a public that is concerned with mutual human recognition as a critical practice.

To be sure, knowledge production cannot happen accurately without taking account of who you are and where you are in systems of oppression, ranging from your biases and prejudices to the training program where you received your methods training (Reyes, 2022). However, witnessing is more than just about participating in personal reflexivity. There are a number of scholars that have already thought deeply about reflexivity (see Davies, 2008; Lichterman, 2017), but the witness pushes us to go further than that. The witness takes on the task of advocating for a radical change in the country's social institutions. Ethnographers who are interested in this type of work do not only need to take responsibility for the harm we might cause in the service of knowledge production, but also what we stand to lose if we do not relinquish the status quo and the fantasies that protect it.

In order to locate the mechanics of this witnessing work and how it aligns with the oeuvre of ethnographic research that wrestles with the researcher's role in the field, this essay considers the following questions: (1) What techniques does Baldwin employ to identify the responsibilities of witnessing?; (2) Which grounds of witnessing are especially important to him? and, (3) Can anyone be a witness? My implicit argument is that paying close attention to Baldwin's engagement with the imperial orders in the United States and France is important to understand his scholarly— in this case, sociological— self, and his literary self. Baldwin's (re)presentations of the United States and France in *No Name* reflect and magnify the tensions between multiple layers of "outsideness": being an international outsider, a within-nation outsider, and a within-community outsider.

Comparing his observations and experiences in the United States and abroad is useful for ethnographic research because witnessing the interactions between the French and Algerian people, between white and Black people living in the South, and between members of the Black Panther Party demonstrate that reorienting the center to focus on marginalized groups does not "taint" the production of science. My interest lies less with privileging one system of representation over the other (i.e., ditching the "old," positivist ethnography for the "new," critical ethnography). Although those debates are still happening, I am more concerned with extending the genealogy of writing and thinking about racism, as well as traditional understandings of racism, racialization, and how that matters for life outcomes. Ethnography is constantly subject to criticism because of claims that it lacks generalizability, yet it is an effective, powerful, and accessible research method that can explain how and why social problems are so difficult to eradicate. It is especially needed right now.

Indeed, ethnographies are socially conceived products that emerge at particular historical junctures and are formed through a dense constellation of complementary and competing bodies of

and the size of her research sample ($n = 1$). Despite these ludicrous criticisms, Hurston inhabited the role of the witness; she understood that to understand the true scope of this vile institution required speaking to people who survived the violence of the Middle Passage and slavery firsthand. This testimonial text helps us see the critical and historical singularity of slavery's violence, thereby connecting the past, present, and future simultaneously through Koussula's narrative.

knowledge. Very often, the distinction between these bodies of knowledge represents different chapters in the story of this method, including using the method for the purposes of exploitation and colonial expansion in the service of forwarding a global color line (Itzigsohn and Brown, 2020; Johnson, 2020). Indeed, crucial to overturning the normalcy and neutrality of racist framings of people's behavior (very often, but not always, conducted by white scholars) are the stories by people of color whose experiential knowledge of structural racism provides the "necessary contextual contours to the seeming 'objectivity' of positivist perspectives" (Ladson-Billings, 1998, p. 11). In that spirit, I am writing this essay to ethnographers writing, researching, and acting in good faith, who are looking for accessible ways to make sure that they do not produce harm with their work and who have different responsibilities to communities outside of the scientific community.

I present the concept of the witness to not only honor the tradition of the critical work that preceded me, but to also show current and future ethnographers how this position can be a part of their "strategic toolkit" while in the field and representing the data they find from their projects (Reyes, 2020). I argue that the position of the detached, neutral observer is insufficient to address the issues that people are currently facing. I make this argument to touch on a more important part of the ethnographic method: responsibility for pursuing freedom for all people, rather than merely describing the conditions of their circumstances. When it pertains to the study of racism one cannot fully grasp all of the dimensions of the observed problem by merely observing the horrors of the worlds we inhabit from a distance. Indeed, it is difficult to argue that the world needs to be changed, but then tow the line of moral non-involvement for the sake of science. By moral non-involvement, I mean juking the responsibility that you have, in private and public, to change and redefine yourself and using that knowledge and experience so that others might live.

In what follows, I will briefly discuss several intellectual interlocutors that speak to the theme of witnessing, especially Black feminists and transnational and postcolonial feminists. I will discuss how these approaches align with, and depart from, other forms of ethnographic practice within sociology. Then, I will turn our attention to the concept of the witness and how it is useful for ethnographers by conducting an exegesis of *No Name*. Specifically, I will discuss three important witnessing "moments": his observations of the French/Algeria conflict, his observations of Black life in the South in the building civil rights movement, and his relationship with the Black radical tradition via the Black Panthers. As this essay will detail, part of what ethnographic research can tell us is why people commit so heartily to belief systems and practices that not only harm others, but also themselves. Baldwin reminds us that racism endures because of the lies that people delude themselves into believing.

The legacy of witnessing in ethnographic research

I am not the first person to propose that qualitative, and especially ethnographic, research should be conducted differently.

There are many branches of qualitative research that equip the researcher to consider multiple vantage points to understand social problems, such as participatory-action research (McIntyre, 2007) and Black feminist (Collins, 1990, 2000, 2009; Crenshaw, 1991) perspectives. I will pay particular attention to the work of Black feminist and transnational scholars in this essay. An important tenet of the Black feminist tradition is that it is an intellectual tradition that honors previous iterations and theories. It builds upon itself, across time, in order to locate and piece together a language that can accurately represent the experiences, joys, struggles, and power of Black women (Luna and Laster Pirtle, 2022). Black feminism is important despite the attempts to suppress, ignore, and erase the contributions of Black feminist scholars. For instance, Collins (2000, p. 8) lists several examples of suppression, but the one that matters especially for this essay is "paying lip service to the need for diversity, but changing little about one's own practice." By this, Collins draws our attention to how scholars talk about acknowledging the importance of diversity, but making little change to their citation practices and their paradigms to understand today's problems.

In this way, witnessing is part of that paradigm shift that scholars need to do. To take on the task of witnessing involves centering the epistemological insights from those who occupy the status position of the "outsider-within" (Collins, 1986). Individuals who live at the intersection of multiple systems of oppression can use that position to not only resist inequalities of power, but to better understand and observe these systems of domination. Race, class, and gender scholars have long argued that the margin is a place of oppression and resistance from where marginalized groups can cultivate reflexive perspectives (Du Bois, 1903; Fanon, 1961; Rawick, 1972; Collins, 1986, 2000; Crenshaw, 1991). As bell hooks (1984) reminds us, the margin can enable subordinated people to look "both from the outside in and from the inside out," which helps develop a "second sight" (Du Bois, 1903: p. vii), a "mode of seeing unknown to the oppressors." Indeed, being in an outsider-within position can be advantageous, as it enables people to make "creative use of their marginality" (Collins, 1986, p. 14).

It is possible to draw from the lived experience to make sense of the observations, interactions, and "moments" that people encounter, to create theory. One does not, and should not, need to be a distant observer to lay claim to knowledge production. Therefore, to occupy the position of the outsider-within opens new pathways to consider how we demonstrate what we observe and what we can know, with authority. In other words, witnessing can be included within the oeuvre of Black feminist thought. Too often, the voices of Black, feminist, and critical scholars are suppressed, ignored, and dismissed because they depart from the traditional way of doing science. Witnessing from the perspective of Black people opens the possibility to build coalitions that support Black freedom and therefore *everyone's* freedom. It is imperative to reject the short-sighted, zero-sum game that argues that prioritizing Black people's freedom comes at the expense of everyone else's freedom.

The work of transnational and queer feminists is also relevant for discussions of what witnessing can look like, especially when it pertains to troubling and harmful social issues (for both the researcher and the participants). Moussawi's (2021) work examines "bad feelings" in the ethnographic process. Moussawi discusses how to make sense of fieldwork when the researcher encounters negative

feelings that uncover past lived trauma, and how the researcher should move forward when their research causes them to feel “bad.” In her essay, Moussawi argues that bad feelings about one’s research is stigmatized in the academy and feeling bad about research is a response to the rigid boundaries placed around knowledge production. Emotions are framed as illogical, or antithetical to reason; therefore, this serves to marginalize “certain topics, modes of knowing, and scholarship” (79). This is important for witnessing because transparency about troubling encounters in the field and the impact that has on the researcher is a necessary consideration for the execution of a project. It is important to be mindful of what our limitations are and how this will affect how we conduct our work. It is better to be an honest witness than to ignore one’s limitations and potentially misrepresent the people we encounter and the observations we make.

This mindfulness is what Baldwin means when he charges us to consider our personal responsibility in the work. Moussawi is a public witness to a troubling and traumatic subject area; for her, it is not just a matter of executing observations, but taking responsibility for how the work affects her. For Moussawi, the goal is not to produce science for the sake of the academic community, but to do it in a way that does not erase her lived experiences and feelings. Boundaries are important to honor and one’s safety should not be sacrificed to conduct research. As other scholars remind us (Parreñas, 2021; Hoang, 2022), fieldwork is an embodied experience and different ethnographers have to navigate different challenges (either personal experience or hearing others encountering it) such as sexual harassment (Parreñas, 2021; Hoang, 2022), or threats to one’s bodily capital (Hoang, 2022).

Researchers must, to borrow from Reyes’s (2020) conceptualization of “the ethnographic toolkit,” recognize “how both our theoretical traditions and methodological choices are strategically used throughout research” (p. 235). What this means is that researchers who take on the role of the witness must see research participants as humans, not just a means to accomplish an end for the scientific community. For example, in his ethnography of Black youth rappers in South Central Los Angeles, Lee (2016) argues against the use of concepts like “participants,” “research subjects,” and “ideas about in-groups and out-groups” because these terms are inadequate for capturing kinship and other processes of intersubjectivity in qualitative research. These are antiquated terms and ideas that do a disservice to the people who we witness building and changing the social world. The role of the witness builds on these critiques of detaching ourselves to uphold the project of science. The witness can be considered a role to inhabit to make sense of the experiences in one’s country (or other sites of field research), interacting with its inhabitants, and connecting those experiences to the broader, global racialized social system. Again, to return to Reyes (2020, p. 25), because “we have multiple characteristics we draw on and we do not share all of our participants’ characteristics” our methods should reflect those changes, boundaries, and points of intersection.

What is the witness, exactly?

In *No Name*, Baldwin considers the building confrontation between the emerging energy of the Black freedom struggle (beginning with the civil rights movement and ending with the

Black Panthers) and what Ray and Seamster (2016) have termed the “racial progress narrative” about the country and its liberal views of itself. Baldwin takes issue with the delusion that so many white people continue to believe about the country and its progress, and he does hold back in his criticism of this behavior. You might wonder what distinguishes the witness from a scholar-activist or participant-observer. The witness is not a different “type” of scholar, or a different methodological approach; rather, it is an approach to ethnography that is based on a political commitment to pursue freedom for oppressed communities. It is less about clear criteria that demarcate between these different “types” of scholars. Instead, the witness must consider the motivations for entering the field in the first place, and what criticisms she wishes to forward in the interest of building a new world. Thus, while a participant-observer or scholar-activist might enter the field based on their own research interests, because they locate a gap in a research program, or are driven by a desire to see justice in the world, the witness enters the field on behalf of the interests of the people they are in community with, with a commitment to not sensationalize people’s experiences, and to use the lessons learned from experiencing the social to make criticisms about the country’s arrangements. I do not think Baldwin would refer to himself as a scholar-activist or bother with labels in this capacity. Baldwin was constantly careful with how he described himself. In the conversation between him and Giovanni that was referenced at the beginning of this essay, both joked about if there was even such a thing as a “Black writer.” He expressed hesitation toward words that are often used to describe writers, too, such as “artist,” “integrity,” and “courage” and instead was more interested in the meaning and reality *behind* those words.³

Baldwin wanted to use his writing to challenge people to live courageously and pursue freedom. His work reminds us that he identified as a Black man who wanted to speak truthfully about the horrors that his country refused to accept and continued to propagate. Therefore, I do not think that he would equate witnessing with operating as a scholar-activist or a different type of scholar. To be an effective witness does not require identifying with a particular research camp or tradition, nor does it require one to propose immediate solutions to the inequality that one encounters as a field researcher. Often, and especially within the sociological tradition, research studies end with suggestions to change policies or other formal rules to ameliorate the problem(s) of interest. I am not saying that suggesting policy changes are inherently wrong or misguided; rather, it should not be the only orientation that researchers should privilege when confronted with the uncomfortable weight of our research findings. It should not be the only recommend end for research.

In this regard, the witness has a different relationship to history, knowledge production, and the current arrangements that continue to erase the capacity for human thriving. Specifically, the witness resists the urge to frame inequality under the guise of “pervasive presentism,” where the researcher “conveys an image of the social world as being governed by unchanging universal laws and logics of necessity [...] [and] the message is that the present is the same as the past, or that the past is simply not interesting” (Steinmetz, 2018 as quoted in Patterson, 2019). The witness understands that the social world cannot solely be described in the “sociological present

³ See: Baldwin (1962). “The Artist’s Struggle for Integrity.”

tense” (Steinmetz, 2018). To speak honestly about the *durée* of white supremacy requires utilizing the contributions from different disciplines, theorists, artists, and other creatives who are concerned with the weight of the social.

To inhabit the role of the witness is to shift one’s relationship to the project of science. By this, I mean that the witness is not so much concerned with “science” for the sake of protecting its supposed sacrality, but using scientific methods— in this case, observations— for the sake of humanizing people that live and survive within oppressive systems. In *No Name*, Baldwin (1972) refers to himself as a “public witness to the situation of Black people.” His words entail a certain form of responsibility that recognizes the duty to represent “the voice” of a diverse community in the public— and using that platform to criticize any claim to the narratives about the country that suggest its hands are pure, clean, and without fault. At the time that Baldwin wrote this book he was known, nationally and internationally, as an esteemed writer and critic of American race relations. Producers in Hollywood asked him to write a screenplay for a film about Malcolm X, and later a similar film in the wake of Martin Luther King’s murder. He declined both offers because he did not want to be an accomplice in “a second assassination” of these men (50). This refusal is important. His refusal tells us that not all representation of your community members, especially those with notoriety, can be considered an honor.

It might be tempting to argue that Baldwin was merely overreacting and that the Hollywood producers did not intend to offend him or, importantly, Malcolm’s legacy. Sociologists are trained to avoid arguing about intent because to do so would be considered a moral arrangement, and locating intent would mean that we would have to confront different explanations for why, in this case, the “race problem” mattered for that particular social situation. Nonetheless, this has not prevented scholars from trying to argue that intentions matter in the day-to-day experience in the United States. We have already seen the damage of assuming intentions about the “faces at the bottom of the well” (Bell, 1992). One need only to read the Moynihan Report and subsequent articles that discussed the functionalist “culture of poverty” thesis that continues to plague our political economy (Small et al., 2010). The point here, however, is that regardless of intent, you cannot evade responsibility for your actions. You *are* responsible for the words you produce and the potential responses that the audience of these words will conjure upon interacting with your words.

Baldwin understood this tension, especially as he grew in fame and popularity as a writer in the American context. Nonetheless, this change in status did not protect him from confronting the deeply unsettling ways that his country and countrymen continued to choose their own destruction, and therefore ensure the marginalization of his community. It is here where *No Name* takes a different, more apocalyptic tone, than his previous texts, such as *Fire*. In the latter text, Baldwin expresses hope for change and belief that his country can turn a corner toward progress. However, ten years later, he provides a different, more refined argument about the *durée* of the racial order in the United States and, as we will see soon, the rest of the world. Here, we turn our attention to the first lesson: how witnessing as an international outsider bears lessons for connecting the domestic racial order to a global racial order.

Lesson 1: connecting empire to the global racial order via the international outsider

Before Baldwin arrives in France, there was already a decolonial project underway in the country. The Vietnamese are fighting against French occupation in the First Indochina War (1946–1954). This led to a standoff at Dien Ben Phu in 1954, where the Viet Minh overwhelmed French forces and force the French to retire their efforts to maintain power over what was then known as colonial French Vietnam. Almost immediately, the French become entangled in the Algerian War (1954–1962). Baldwin’s original reason to retreat to Europe was to escape the vice grip of antiblackness in the United States. He is convinced that the country and its social institutions will kill him, or that he will kill someone. Yet, he notices a deep hostility toward Algerians in France is well underway. He begins to make sense of his position as an American citizen and Black man within this tapestry of colonial relations between France and Algeria. He is *not* an insider or member of either community. In fact, he arrives in France penniless, a stark status shift from his experience in the United States. There is no limo to pick him up from the airport and take him to wherever he wants to go in the city. Consequently, he finds himself living “mainly among les misérables, and in Paris, les misérables are Algerian” (56). Indeed, he finds a community in sharing status with those whose faces were also at the bottom of the well (Bell, 1992). Because he lived with les misérables, or in Fanonian terms “the wretched,” he observes an incredibly rich life of interactions between Algerians living in France, and Algerian/French citizen interactions to make sense of his own marginalization, and position, as a Black American (Fanon, 1961).

As a result, he sympathizes with the plight of the Algerian community because they are treated similarly to his community in the United States. His observations of the Algerian condition in France leads him to conclude that when empires are threatened and confronted, the response is one of greater force, as it can no longer pretend to justify itself. In fact, the first time he uses the term “witness” in the text is when he observes the French police exert violence toward an unassuming Algerian man by throwing him through a glass door and leaving him in the street. He writes (Baldwin, 1972) that he “witnessed a murder, or nearly witnessed an attempt to do that.” According to his observations, the French were hurt that “their stewardship should be questioned, especially by those they ruled.” Even though the Algerians had nothing to do with the defeat in Vietnam, Baldwin notices that the French police viewed Algerians as a threat to their authority and, largely, the authority of the French empire. He not only observed this through interpersonal interaction. He was not content with merely observing the violence in the country to which he was an outsider. He also pairs his observations with content analysis of *Combat*, a journal run by Albert Camus to see how Algerians are framed in French media. He makes his observations of the French/Algeria conflict not for the sake of scientific ambitions, but to make sense of the *global* racialized order that all of us are a part of. His study of these events led him to a larger point: colonialism is not just present in France, but it affects all non-white people around the world.

Here is the lesson that is instructive for ethnographers: through his observations of social interactions and content analysis he collapses the demarcation between “comparison groups” across the colonial color line and instead gathers empirical data from the vantage point of socially marginalized groups. Indeed, there is enough explanatory power about the dimensions of inequality that can be observed within a socially marginalized group, instead of assuming homogeneity within this group or that the outcomes of this group need to be compared to the experiences of members from dominant groups. He utilizes his observations to leverage the act of witnessing toward social justice. He walks with the Algerian people, lives with them, eats with them— this labor of participation provides him a language to reflect the Algerian people’s physical, social psychological, and cultural condition. Notice, though, that this is a slight departure from participatory-action research. Baldwin does not collaborate with the Algerian people to “develop research questions, methodologies, and analyses that empower and liberate communities” (Hayes et al., 2022, p. 102).

Nonetheless, his observations challenge the audience to outline the afterlives of colonialism and the ever-present need for empires to assert their dominance over countries and people they have determined to be inferior. Baldwin does not include interviews with French police officers or citizens to get “both sides” of this conflict. Indeed, “the Algerians were not fighting the French for justice [...] but for the power to determine their own destinies” (Baldwin, 1972). His status as a witness is not to pursue distant objectivity, but to make a moral commitment toward honoring the Algerian people as theorists of their own condition, rather than viewing them through the eyes of the dominant class. The longer Baldwin stays in France, the more connections he makes to his own experiences as a Black man in the United States. He goes on to write that it is “strange to find oneself, in another language, in another country, listening to the same old song and hearing oneself condemned in the same old way” (Baldwin, 1972). This reflection draws a comparison between French colonialism and the vestiges of institutional and systemic racism in the United States. At the same time, he is also deeply aware of how living in France isolates him from the very people (and problems) he wants to speak to, connect with, and protect: Black Americans. This desire to be in community back home, in part, allows him to see how insulated he was while witnessing the Algerian/French conflict because what was happening to them “did not appear to be happening to the blacks” (Baldwin, 1972).

While he could understand and support the Algerian independence project, he could not fully detach himself from the United States cultural frame. He concluded that he was, “operating, unconsciously, within the American framework, and, in that framework, since Arabs are paler than blacks, it is the blacks who would have suffered most” (Baldwin, 1972). He believes that he needed to return back to the United States. In his own words: “Everybody else was paying their dues, and it was time I went home and paid mine” (Baldwin, 1972). Now, we will turn our attention toward how his responsibility as a public witness manifested after his return to the United States, specifically as a within-nation outsider.

Lesson 2: paying one’s dues via the within-nation outsider

Baldwin returns to the United States in 1957 in the midst of the growing civil rights movement. He misses the familiarity of the country— the sounds of taxis in New York City, the familiar places, his family, and, yes, even the food. However, underneath this nostalgia is a recognition that his work is unfinished in the United States. While in Paris, he operated as a witness for the Algerian condition in the French colonial regime. Now, he must turn back to his own country to engage in the unfolding institutional and systemic racism of his birthplace. This colonial comparative analysis profoundly reshaped Baldwin’s prose. Upon returning to the United States, he changes his language; Baldwin begins to frame Black Americans as a colonized people, and white people as colonizers. This language shift is influenced, no doubt, by witnessing the surveillance and subordination of the colonial regime in France.

For Baldwin, paying one’s dues is an important burden to carry if one is to conduct the work of the witness. To pay one’s dues involves developing deep, interpersonal intimacy with the community/communities that you are walking alongside and taking careful attention to describe one’s people in humane (rather than scientific) terms (Du Bois, 1903). By humane terms, I mean describing Black people as complex, beautiful, and fiercely determined people, rather than hapless victims, unintelligent recipients of white supremacy, or individuals who are “cultural dopes” (Lynch, 2007). Instead, Baldwin’s witness aligns with what the late Bell (1991, 1992) refers to as racial realism— a mindset or ideology that describes how Black people navigate the terrain of unrelenting white supremacy on the interpersonal, institutional, and systemic levels, and sustain themselves in the midst of this reality. The racial realist accepts the permanence of racism as a necessary staple within the United States, but does not wallow in this reality; instead, the realist seeks (and imagines) alternative avenues of being that fall outside of the periphery of the status position that the country has relegated her to. The racial realism approach is useful here because it provides a broader articulation of the Black experience that exceeds white sociological framings of Black life— as colorless, or merely a site of permanent exploitation. With this turn in *No Name* Baldwin’s observations help us understand the colorful moments of “Black placemaking” (Hunter et al., 2016). This is an aspect of witnessing that is important to him as he turns his attention to the South in particular.

Baldwin is commissioned to write a newspaper article on how Black people are treated in the South. The assumption was that Baldwin would write about the “typical” Black experience under Jim Crow and that this work would help solidify him as a voice in the movement. In contrast, Baldwin approaches this writing project with the understanding that the troubles that face the South are not unique to that region. The origin of the so-called “Negro problem” is both an international problem manufactured by the global racial order and one that emerges from within private life. In the U.S. South, for example, Black people have long been subject to the whims of white people who think about them as “mammies,” “magical Negroes,” and as hyper-sexual beings— all of which shapes

Black bodies and lives into beings that can be controlled in all arenas of the social. To this point, Baldwin writes that the way toward redemption is through the South *not* the North. While the North believes itself to be superior to the “backwards” ways of the South, it did so as a means of scapegoating the nation to evade its own complicity in white supremacy.

In 1957, Baldwin arrives to Little Rock, Arkansas and is confronted with his lack of familiarity with Southern social mores and taboos. He was, after all, a city boy and while growing up in Harlem was not easy, it was distinct from life in Little Rock. He initially becomes overwhelmed with dread and concern for Black people there. More specifically, he fears for the lives of the children who were sent to integrate the school in this city. He fears for their safety, for their humanity, and the *inhumanity* that white Southerners portrayed because of their hatred for Black Americans.

While on his assignment, Baldwin conducts observations of the racialized interaction order that narrates Southern social life. He enters a local restaurant through the front door and is immediately confronted with unwritten social cues suggesting he erred in his entrance. He writes, “every white face turned to stone: the arrival of the messenger of death could not have had a more devastating effect than the appearance in the restaurant doorway of a small, unarmed, utterly astounded black man” (Baldwin, 1972). The waitress, and a white man passing by on the street, remind him of his place: “right around there, boy. Right around there.” Baldwin is fearful and angry and recognizes that his life could very well be at stake for daring to enter the restaurant as another citizen would. More importantly, though, he realizes that, through the menaced glares, coolness, and shock from the white people, these two people believed that they were doing him a favor by reminding him of his place. This social repositioning surprises him, as Baldwin expected that these people would express pure resentment. As he notes (Baldwin, 1972): “[that man] was, indeed, being as kind as can be expected from a guide in hell.”

Much like Baldwin’s time in France, his reflections reveal that although he shared similar experiences of racialization with Black folks living in the South, he was unfamiliar with the plight of Black people in the South. That being in a place and of a place was also true internally within the United States. At this realization, Baldwin writes that his, “role was to do a story and avoid becoming one” (Baldwin, 1972). It is easy to assume that he is arguing that he should be a detached, neutral observer and not interfere with the site he is investigating. To the contrary: this quote illustrates that he is acknowledging his status as a *within-nation outsider*, but this status position does not preclude the possibility of making sense of Southern Black life. We know this because he does not represent Black Southern life from the perspective of white Southerners. He finds alternative ways of being that exist outside of the imaginations of white supremacists.

Despite the weight of Jim Crow segregation in the Deep South, Baldwin locates a vibrancy in Black Southern social life. Here is where his position as the Black witness is advantageous. He observes that the hotel where he is staying is also a gathering place for the Black community. It is a place where self-delusion comes to die—where the stoic Baptist preacher can sit across from “the town’s loose and fallen ladies and their unstarched men” (Baldwin, 1972). This point about delusion is important: a

key theme throughout Baldwin’s writing is the prevalence of the dishonesty that Americans continue to believe about themselves. The dishonesty about how they arrived here, how the country emerged within the world, and even what drives people to behave in the way(s) they do. Racism, in his view, is a project rooted in delusion that is accompanied by structural support for those lies. The South, Baldwin argues, is a place where one—Black and white—cannot hide behind that delusion. It is in your face, unlike Northerners who believe they are superior to the ways of the South. The point of the duality of Baldwin’s observations in the South—the weight of white supremacy on the one side, and the freedom and joy that Black people experience with each other on the other—demonstrates the Du Boisian observation of the veil (Du Bois, 1903). Black folks in the South have a second sight to not only see white people as they are, but to also see *each other* fully.

The final observation he details in this section is the social and emotional costs of nonviolent protest behavior in the South. He describes two pastors, and one of whom is a grocer. He marvels at the hypocrisy of the American democratic myth: that harmony and unity are at the center of this project. If this was the case, then Reverend D. (the grocer) would not need to arm himself and his children while they watch over his grocery store at night, or the Reverend S. would not have bullet holes riddled in the basement of his church. Both men were passionate about their religious convictions and committed to registering Black voters, which, of course, brought violence to the front steps of their homes and churches. Baldwin expresses sadness as he speaks about the frankness of nonviolence. Today, we have the fortune of merely recognizing (and in some cases memorializing) the nonviolent approach. Baldwin’s observations are visceral—he writes (Baldwin, 1972) that his observations of Reverend D. made “the concept of nonviolence real to me” and that the concept of nonviolence “entered the realm of individual and above all private choice and I saw for the first time, how difficult a choice it could be.”

I want to pause here because this point is relevant. The observation of nonviolence should not be viewed within a vacuum. I do not wish for us to conclude that nonviolence is a “natural” response to white supremacist violence or ignore the deeply sacrificial aspects of choosing nonviolence in the face of unrelenting evil that wishes to exterminate your life. Baldwin’s framing of this behavior is from a position of deep interpersonal intimacy. Therefore, as he draws near toward this community and its various leaders, his words should move us toward a position of reverence for these people, not pity or truncated, hollow “respect.” Baldwin cautions the reader to not think of nonviolence as a simple decision. Nonviolence is a costly commitment. The loss of property, status, and employment affects an entire family or community system, not just individual community members.

While the *observation* of nonviolence, and the various backstage moments that lead up to the decision to organize for the right to vote happens on the interpersonal level, the connection to the larger pattern of macro-level discrimination should not be lost. It is fitting that Baldwin finishes the first half of the book in the South because, as other scholars of the South detail (Laymon, 2018; Foster, 2020; Wright, 2020): as the South goes, so does the country. The South is not another society that is removed from the North, Midwest, and the Western United States. It is just as much a part of the country as

the city that Baldwin claims. Indeed, he understands that “the spirit of the South is the spirit of America” (Baldwin, 1972). To reject and dismiss the South is to reject the genius that built the nation.

In the next section, I turn my attention to the lessons that Baldwin provides in the second half of the book. In this section, he teaches the readers about witnessing the pangs of mass incarceration and its deleterious effects on Black people, their lives, and the Black radical tradition from the position of the within-community outsider.

Lesson 3: representing the wretched via the within-community outsider

To exist as a witness from the position of the within-community outsider means accepting that even within your own community, you might face ostracization. It is a status position that might result in loneliness. Secondly, this position in the field opens new pathways for the ethnographer to consider “the shape of the wrath to come” (Baldwin, 1972). Baldwin, borrowing from the Christian tradition, deploys this language to speak of the final judgment. This is a striking departure from the liberal optimism in his earlier work, especially in *Fire*. Baldwin’s apocalyptic “turn” reflects dissolution—indeed, condemnation—with the American experiment that runs on the wheels of (racial) capitalism (Robinson, 1983). Rather than trusting the individual agency of select people, he turns to an international framework (which he then uses to make sense of his home country) within which individual agency is swallowed up and co-opted by greed and capitalism. Baldwin observes that, without economic justice, the legal gains of the civil rights movement were toothless for many—if not all—Black Americans. He notes that the civil rights had been “rendered moribund” (Baldwin, 1972). Baldwin is not the only Black writer who is thinking about these ideas, though. Other Black writers and artists such as Toni Cade Bambara, Amiri Baraka, Audre Lorde, Sun Ra, and Malcolm X also inhabited an apocalyptic frame to criticize the racial inequality built into America’s commitment to capitalism.

Genuine solidarity with the Black radicalism of the late 1960s replaces the liberal optimism he expressed in his earlier writing, and this change is likely a consequence of his remarkable engagement with the Black Panthers. Although he sees the co-opting gimmicks of landlords, jobs, corporations, and politicians in most of the activity in or directed toward the Black ghetto, he finds the same sort of community in the Panthers that he found in the parties in the South and the banlieues in France.

What does this shift in belief in the country mean for political action? Baldwin poses a dilemma: “If [the excluded] attempt to work out their salvation—their autonomy—on terms dictated by those who have excluded them, they are in a delicate and dangerous position, and if they refuse, they are in a desperate one: it is hard to know which case is worse” (Baldwin, 1972). Baldwin suggests that both reform (working within the “terms dictated” by those in power i.e., the pursuit of civil rights) and revolution (“if they refuse,” i.e., separatism, Black Power) are too risky (“delicate and dangerous” or “desperate”). This observation marks him as a within-community outsider. Because he was friends with Malcolm X and befriended members of the Black Panther Party, some civil rights activists did

not want to associate with him. Conversely, some members of the Black radical tradition did not like him because he also humanized the civil rights struggle and associated with King, which they believed marked him as a sympathizer with reformist solutions. Although his adjectives are vague in this passage, he is clear that the excluded have little freedom to practice their agency and achieve autonomy. He acknowledges that the brunt of capitalism forces the most excluded people to seek alternative ways to reclaim their autonomy.

What does that mean for change, then? He answers this question from two perspectives. From the perspective of the powerless Baldwin writes, “for power to truly feel itself menaced, it must somehow sense itself in the presence of another power—or, more accurately, an energy—which it has not known how to deny and therefore does not really know how to control” (Baldwin, 1972). He elaborates what this cryptic “energy” that power can neither deny nor control might be, and how to acquire it. We must attend to “the people who are the most spectacular recipients of the benefits of this prosperity [that costs millions their lives]” (Baldwin, 1972). In this view, I argue, he is acknowledging that organizations like the Panthers are an exemplar of that counterforce to racial capitalism.

From the perspective of the well-off, however, he notes that eradicating capitalism would cost them too much. He writes that the well-off, “cannot, or dare not, assess or imagine the price paid by their victims, or subjects, for this way of life, and so they cannot afford to know why their victims are revolting. They are forced, then, to the conclusion that the victims—the barbarians—are revolting against all established civilized values” (Baldwin, 1972). As a result, these people “desperately seek out representatives who are prepared to make up in cruelty what both they and the people lack in conviction” (408). Because the well-off refuse to understand the suffering that motivates people to riot, they respond by distancing themselves from “the barbarians” and electing politicians who vow to crack down on the black underclass, such as Richard Nixon, who campaigned, in 1968, on the promise of “law and order.”

Inhabiting the role of the witness allows him the space to align with and critique the responses to oppression and marginalization. He *does* take a position in his assessment of the Black radical tradition, civil rights, and white supremacist counter-behavior. Similar to other sociologists who conduct ethnographies of capitalism and how it manifests in schools, the racial politics of desire, and the criminal justice system (Hoang, 2015; Clair, 2020; Drake, 2022). Baldwin does the work of analysis but he is not so much concerned about proposing an easy solution. Notice that he does not propose new policies or suggest individual reform toward the racialized social system. Another energy entirely—that is not beholden to the cruelties of racial capitalism—is needed.

This disavowal and cruelty “is a formula for a nation’s or a kingdom’s decline” (Hoang, 2015; Clair, 2020; Drake, 2022). The disavowal prevents the powerful from being able to deny the energy manifested in riots. The election of politicians like Nixon only intensifies this undeniable and uncontrollable energy. In the end, “the victor,” by which Baldwin means the United States, will “become the prisoner of the people he thought to cow, chain, or murder into submission” (Baldwin, 1972). Here is why the apocalyptic turn as a within-nation outsider is useful for ethnographers. He admits that there are hosts of people who are

unwilling to change, too, who *enjoy* experiencing the fruits of exploitation at the expense of their fellow countrymen. This is a routine that cannot last for long, in his eyes, and is a formula for the decline of an empire. The excluded must begin “to forge a new morality, to create the principles on which a new world will be built” (Baldwin, 1972). This is what the Panthers and civil rights activists were trying to do with their mobilization, and this is why he turns to apocalyptic language to solve the problem of individual agency. If racial capitalism was the problem, nothing short of its undoing would do. This is a task that could not be solved by changing individual dispositions. Therefore, Baldwin calls for people to wait, endure, and plan for the radically new world that is inexorably coming.

Conclusion: can anyone be a witness?

Throughout the iterations of the paper, my colleagues (and reviewers) asked me multiple versions of this question: given these terms, can anyone be a witness? The answer is not just a simple “yes.” I am reminded of Toni Cade Bambara’s charge: what are [you] pretending not to know today? One cannot be a witness and still hold fast to the seductive lies of American progress. By this, I mean that if we continue to believe that the United States has turned a corner toward some ideal version of progress, we are falling prey to the same delusions that Baldwin wrote about just over fifty years ago. For Baldwin, the sense of responsibility for bearing witness was driven by his recognition of, and refusal to suppress, the fundamental tension emerging, as Leeming (2015, p. 304) puts it, from “beings within himself— the child dancing through life, the intelligence outraged at the nature of things, the madman often blowing the house apart.” *No Name* is a testament to the indivisibility of the personal and the political (and, for our purposes, the distinction between a researcher and a human), as the 1960s snowballed into the unknown futurity of the 1970s. The words shared here still resonate for our moment today.

One must be willing to accept that doing good science does not require a distancing between your identity as a researcher and your identity as an agent. Importantly, one (and one’s peers in the scientific community) must accept that refusing to do this distancing does not mean that one’s work is “non-scientific” so that one can be a participant in building freedom for all people. This includes being knowledgeable about one’s positionality in the field, carefully considering how one interacts with the members of various communities while in the field, refusing to homogenize those members, and to consider the research questions and topics of interest. Importantly, this means having the intellectual humility to defer to marginalized scholars, and members outside of the academic community, as experts and knowledge producers, not as an aside to mainstream scientists. If you accept these terms, then one can operate as a witness.

Additionally, we cannot do effective ethnographic work and ignore the significance of race, gender, class, and sexuality as continuous and stable forces of the social world. These systems of domination are not just variables within our models or details that distort our ethnographic observations— these systems are part of the landscape of inquiry and should be treated as fundamental parts of everyone’s lives. Even if a study is not explicitly about race,

gender, or class, those elements are there. One cannot be a witness and remain “strangely hesitant” about if these social facts continue to affect our life outcomes and how these social facts are upheld by structures on the macro, meso, and micro-levels (Du Bois, 2000). One’s citation practices should reflect this understanding, for instance.

Another question I was asked multiple times is, is being an outsider necessary to be a witness? The multiple layers of “outsideness” as articulated in *No Name* is instructive for ethnographers to keep record of their position(s) in the field and how those positions allow us ways to see—and importantly— not see. Even Baldwin did not cover the full scope of interactions in these different contexts. Even within this provocative book there is terrain that he did not cover. The charismatic, grassroots leadership of Black women was central to the success of the civil rights movement, but was not a point of emphasis in the text. While Baldwin mentions leaders like Martin Luther King, Jr., Malcolm X, and Fred Shuttlesworth, he fails to mention Fannie Lou Hamer or Angela Davis. Baldwin still upheld the responsibilities of witnessing, but he was limited in his insights. There were limitations in the execution of this work. Being an outsider to a community is not a requirement to do the work of witnessing. However, seeing the world from a position of marginality (epistemologically, demographically, and socially) *does* help with doing the work of witnessing because it uncovers what mainstream accounts of inequality often take for granted or ignore as a social fact. The contributions from Black feminists are noteworthy because they cannot afford to ignore race, gender, and sexuality as social forces in their lives. The work of scholars such as Zora Neale Hurston, Nikki Jones, Kemi Adeyemi, Karida Brown, and Mary Pattillo provide rich ethnographic insights that provide answers to questions that are too often neglected by mainstream sociologists. Instead of pursuing generalizability, the work of these scholars reminds us that what we call the “marco” cannot exist without individuals and the communities that they inhabit. The inequality that we regularly see is not “natural” but requires people to buy into it and believe it, which also means that we have the capacity and obligation to disrupt those systems as well.

Finally, I will return to the apocalyptic future point that I shared in the paper’s abstract and discuss how Baldwin’s lessons for witnessing matter for practitioners and ethnographers. We must forego the liberal delusion that the nation has encountered significant progress. Perhaps it might be best to dismiss the term progress altogether, especially if it is used in vacuous terms. As Spillers (2022) reminds us, whenever there is a “first” of something (e.g., first Black president or vice-president), that means it has arrived too late. This is not to say that *No Name* is devoid of hope, though. Baldwin does express hope, but differently from the “reasonable Black and whites” that he mentioned in *Fire*. In *No Name*, he expresses hope in the possibilities of the revolutionary forces that are fueling movements within the Black radical tradition. This is not the same as pursuing specific policy goals that the civil rights movement tried to accomplish. Instead, hope lies in the recognition of the Black radical tradition’s potential for rupturing current social arrangements that are grounded within the teleological myths of American racial progress.

The inspiration for the title of the text comes from the Book of Job. Baldwin uses the passage from which it comes to articulate the fate of the wicked. The passage prophesies the annihilation of any memory of wicked people, which I believe he meant as a layered warning to white Americans and the global racial order. The threat of the fire next time— as something that would come if interracial coalitions were not formed— did not hit hard enough. Ten years after its release, Baldwin returns to us with yet another warning, and refusing to put hope in his white countrymen. The same year that this book was published, Richard Nixon's "law and order" approach devastated and disrupted the lives of millions of Black Americans. The conditions for the Black underclass are worse than they were in the 1970s. Mass incarceration continues to solve the problem that Baldwin observes: "that this country does not know what to do with its black population now that the blacks are no longer a source of wealth" (432). And with the emergence of movements like the Movement for Black Lives in response to the "plunder" of Black lives (Coates, 2015), we are still witnessing how white Americans continue to elect "representatives who are prepared to make up in cruelty what both they and the people lack in conviction" (432). If we take a look at the expulsion of two Democratic State Representatives in Tennessee, the rise of moral panic about critical race theory and the subsequent legislation in Florida to make it illegal to teach accurate U.S. history, and attempts by judges to close libraries that house banned books, it is clear that we need to continue to take up the challenge of witnessing. The work of cultural criticism must continue in order to participate in the ongoing pursuit of freedom.

Data availability statement

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

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Author contributions

The author confirms being the sole contributor of this work and has approved it for publication.

Acknowledgments

The author would like to thank Victoria Reyes, Theresa Rocha Beardall, Magda Boutros, and Walton Muyumba for their comments on this paper at its various stages and for their consistent encouragement to think beyond the text to locate Baldwin's important contributions as a sociologist.

Conflict of interest

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OPEN ACCESS

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RECEIVED 10 February 2023
ACCEPTED 11 September 2023
PUBLISHED 28 September 2023

CITATION
Owens LL (2023) Encountering deception in
virtual spaces: guidelines for virtual
ethnography.
Front. Sociol. 8:1163560.
doi: 10.3389/fsoc.2023.1163560

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Encountering deception in virtual spaces: guidelines for virtual ethnography

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This Perspective Essay draws from an experience of deception in virtual fieldwork and considers implications for those designing methodologies for virtual ethnographies. As qualitative field work increasingly takes place within virtual spaces and through virtual means, researchers are faced with critical dilemmas in the processes of data gathering and verification. One of these dilemmas concerns ensuring data validity and facticity if encountered with research subjects who are deceptive about their identity, experiences, or relationship to the field of research. This Perspective Essay offers specific guidelines concerning articulating the nature and possibilities of deception in virtual spaces, identifying deceptive data, and what to do with deceptive data in order to maintain data validity and transparency.

KEYWORDS

deception, virtual, ethnography, research methodology, data transparency

Introduction

Entering the realm of virtual research during the widespread social distancing measures and shutdowns related to the Covid-19 pandemic beginning in 2020, I and many others began the process of adapting research and methodologies to primarily virtual contexts. During that time, I encountered a woman named Colleen¹ who I was able to ascertain was taking on a deceptive identity in order to participate in research studies. This Essay builds on a previous Article written about my experience with Colleen by exploring some of the methodological implications of intentional deception for ethnography conducted in virtual spaces. In writing it, the intended audience are first time ethnographers navigating virtual spaces. Accordingly, I provide a series of guidelines for each stage of the research process, including the research design phase, the data gathering and analysis phase, and the reporting phase.

Virtual methodologies present many of the same challenges and concerns as in-person methods; however, virtual methods also introduce certain new challenges or change the nature of old ones (Owens, 2022). Although virtual and physical spaces can often seem to be analogous, virtual spaces can both shield and reveal in ways that physical spaces do not. Some of the features of virtual space may make it more vulnerable to deception. For example, the liminal spaces, points of transition and waiting, of virtual spaces are different than in physical spaces. Many of the physical spaces where observations take place are shared in-person, but not virtually, changing the research experience (See, e.g., Gorski, 2021). These moments and spaces are one way in which the researcher orients themselves, gauging the authenticity and complexity of those

¹ Colleen's Name has been changed. A longer form article written on Colleen is also available as an open source from Sociological Methodology at <https://journals.sagepub.com/doi/full/10.1177/00811750221106777>

whom they are interacting with. Additionally, with virtual space, the barriers to entry are lower for those being observed, perhaps meaning that there is a greater risk of deception in virtual space.

Virtual research presents many advantages as well. For ethnography, without the burden of travel, more observations with more research subjects may be conducted in a shorter amount of time. For ethnographic interviews, scheduling may be easier if research participants do not need to commit to traveling physically to an interview location. A wide range of groups are accessible to a wide range of researchers in comparison to in-person ethnography, and the possibility of multi-sited, global ethnography is made real. Without the burden of travel, more observations with a single research participant may also be easier to conduct, making ethnographic methodologies more accessible for researchers and participants. As well, encountering the inner lives and spaces of research participants through virtual communication methods can add to the depth of data.

The possibility of deception is particularly salient in terms of the validity, accuracy and transparency of online research. While deception can take many forms, for the purposes of this paper I focus on deception that is intentional on behalf of the research subject. Intentional deception in virtual spaces can take many forms. For example, it can involve deception about one's relationship to the research, as was the case with Colleen. It can involve deception about one's own identity in online spaces. And it can involve deception or embellishment about one's story or experiences. This Essay primarily provides some practical methodological guidelines around intentional deception in virtual ethnography.

Encountering colleen

I've documented a more expansive description of how I negotiated my relationship with Colleen in a preceding article (see Footnote 1). Here, I summarize this relationship for the purposes of giving context to the rest of this Essay. I first encountered Colleen in an email when she responded to a Craigslist ad recruiting residents of New York City public housing for an interview-based research study. Initially, I experienced Colleen as an ideal subject. She communicated clearly and was verbose in her responses. For example, whereas most respondents answered my initial written questions as briefly as possible ("yes" or "no"), Colleen gave detailed answers. However, it was only a few minutes into our initial synchronous verbal conversation that I began to suspect that Colleen did not have the experience that she claimed and was not likely to have ever been a resident of New York City public housing. When asked questions in 'real time', Colleen could not adequately fabricate answers to questions like "What do your neighbors do for a living?" (Answer: "Snitches get stitches.") or "What is your neighborhood [reportedly midtown Manhattan] like at night?" (Answer: "Very scary"). Her answers were sufficiently implausible that I politely ended the interview quite early on, paying her the promised incentive fee but marking her data as likely deceptive and separating it from other data. At this time, I strongly suspected that Colleen was being deceptive, but I did not yet have definitive 'proof' or admission of deception. I was also unsure about what to do with Colleen's data and what my responsibility was in reporting it to future readers of related work.

The second time I encountered Colleen was in her answer to an advertisement attempting to recruit preliminary interviewees for a

second, unrelated research study. In her response, Colleen used the same unique 'real' (legal) name, and I was immediately able to flag the email. This time, the data she gave was fundamentally at odds with the data that she had previously given. As I now had confirmation that Colleen was engaging in deception, I was emboldened to try to engage her in a conversation about how and why she was doing what she was doing. I wrote her back with a direct request to speak to her about how she went about, in my words, "posing as someone who meets criteria for these [research] studies." She also indicated that she was happy to speak with me about her "story."

Colleen described herself as a people pleaser who was able to intuit what researchers wanted to hear in interviews. She felt that she had been very successful earning an income through participation in research studies that paid incentive fees and were completed online so that she did not need to travel. At the time we spoke, she reported that I had been the only researcher to 'figure it out' (though I suspect others did, perhaps just not confronting her) and that she had participated in 'hundreds' of research studies. Colleen did not change every aspect of her persona for the research studies, only the relevant ones. For example, Colleen did not have a husband at the time that we spoke, though she indicated that she did, describing him as "the sexiest Puerto Rican guy" and basing his characteristics on a celebrity that she admired. When I asked her about how she met her husband in the initial interview, she was able to give a detailed, plausible, and gushingly romantic story. I would have had no reason to question this part of her interview, should it have existed in a vacuum.

However, Colleen was less able to answer specific questions that focused on less universal subject matter, instead relying on cultural tropes in those circumstances that did not resonate as authentic. Over email, for the site and parameter specific answers to the questions I had asked initially, Colleen had had the time to carefully consider her answers. For example, she had conducted research via the internet to find the specific name of a public housing complex; however, in the midst of a real-time, synchronous conversational exchange, she did not have time to conduct this outside research.

In our conversation, Colleen indicated that when selecting studies to participate in, she sought to avoid research studies that involved complex knowledge, triangulation of data, or detailed storytelling about subjects she did not have personal knowledge, such as medical conditions. Instead, Colleen sought to participate in studies involving more universal experiences which were also highly individualized (i.e., relationships or work), or experiences which could not easily be triangulated or disproven. For example, in order to provide a plausible rendition of experience with a specific medical condition, one would need to become intimately familiar with the symptoms, treatments and communities of a condition. However, a rendition of an experience working is an almost universalizable experience which is, at the same time, highly individualized and difficult to confirm.

Colleen's success deceiving virtual researchers has methodological implications. Colleen was encountered in the context of virtual interviews, and considerations about incentive fees, recruitment, interview structure and design are immediately brought to bear. However, other virtual methodologies are also implicated and the following section will consider some of those implications for virtual ethnography.

[A few] guidelines for virtual ethnography

Colleen highlights the fact that virtual spaces do facilitate those who wish to deceive in ways that physical spaces do not, and that ethnographers operating in virtual spaces should expect to encounter deception. Below, I discuss three sets of guidelines for researchers entering into ethnographic study of virtual spaces which emerge from my own experience in virtual fields of research.

The research design phase: articulating the nature and possibilities of deception in virtual spaces

The performance of social life in online spaces may be easier to creatively maintain or manipulate, which makes understanding deception in virtual spaces complex. Various forms of anonymity in virtual space, including around data privacy, also complicates the concept of deception. Whereas data verification and “questioning and corroborating” data is generally a part of ethnographic research (Murphy et al., 2021, p. 50), virtual spaces, perhaps more easily than physical spaces, allow individuals to obscure, invent, enhance and borrow identities, experiences and modes of interaction. The motivations for this vary and include fears about personal safety or risk, and, at times, may also represent attempts at self-invention or experimentation. Further complicating this, participant anonymity or self-enhancement in virtual space is sometimes socially normative or considered best practices.

In Sociologist Forberg's (2021) virtual ethnographic work on extremist conspiracy theorists, part of his data gathering required ‘lurking’ anonymously in virtual message board spaces. There, anonymity or semi-anonymity was a common practice as a risk management strategy to prevent doxxing. As well, in the community he was observing there was also a running suspicion about infiltration of the space by ‘fake’ community members (2021). Contrasting further, in Sociologist Julia Goldman-Hasbun's study of hate speech in virtual message boards, total anonymity was the norm and basis for the kind of expression the researcher was studying (2022). On a totally anonymized message board or in a forum where anonymity is common, it may be difficult to confirm deception when an anonymous user baits or spams a virtual space (or is perhaps not even human) and when a user is authentic (Goldman-Hasbun, 2022). Goldman-Hasbun's work makes clear, one cannot assume that anonymous posts are not spamming the board. At the same time, the use of real names also comes with its own risks, for example introducing an observer effect or putting the observer in jeopardy. This does not necessarily invalidate the data gathered in these spaces, it is merely one methodological consideration around a particular field of interaction and observation. Before getting in the field ethnographers should ask themselves what kinds of threats to data validity might exist in the virtual worlds they are entering.

Other virtual spaces may not put value in anonymity, such as those of social media influencers, where anonymity is not as often strived for. However, such spaces are also susceptible to fraud or deception, such as through fabrication of experience. There may also be meaningful differences between individuals' virtual and non-virtual personas. Motivating factors and dynamics in virtual space vary, and there is, for example, a great deal of financial reward for those who are able to attract a following in social media. Sociologist Forrest Stuart

conducted an ethnography which linked virtual and physical personas, comparing online personas of ‘micro-celebrity’ musicians who identified with and reproduced online representations of urban gangs with observations made in the physical:

“I came to realize that a responsible account [of the online personas] required directing even more attention to the agency and ingenuity of these young people as they try to cash in on their stigma. It also forced me to continually remind readers that these online performances are often just that—performances.” (Stuart 2020, p. 216)

Stuart found that oftentimes online personas were exaggerated, for example, with individuals claiming online to have engaged in much more violence than was actually the case, though both personas influenced the other. Sometimes, individuals would try to live up to or maintain online performances in physical spaces in some ways, such as impressing the romantic partners in their lives. This deception was both intentional (the research subjects knew they were not being truthful about the violent acts not committed) and reflective of meaningful social processes. Unlike Colleen discussed in the preceding section, who momentarily took on personal attributes in order to conform to a study's expectations without any personal knowledge of the field of research, the subjects of Stuart's research meaningfully engaged with and navigated between both personas and fields. Identifying this deception and triangulating the data was meaningful for Stuart's analysis, and at that point the ‘deception’ itself became an object of study. Nonetheless, one might imagine a very different analysis and/or set of research questions if Stuart had studied only the online personas.

How does one identify intentional deception and its meaning for data validity while also acknowledging that research subjects often have complex motivations and views? If attempting to identify deceptive data or planning for the possibility of deceptive data, a researcher may ask themselves:

1. *Does the virtual space contain the ‘whole story’? If not, what ‘part’ does it reflect?* By studying one virtual space in which his subjects expressed themselves and comparing it to data gathered in-person, Stuart found that the virtual space contained an element of performance whereby subjects would enhance their accomplishments for the benefit of the virtual ‘audience’. Ascertaining which part of the ‘story’ that the virtual space contained, was key to Stuart's interpretation of the data. For every online space with a public, accessible space, there will also be data which are harder to access, e.g., from private messages to proprietary algorithms. Obviously, this is analogous to in-person spaces--researchers are almost never privy to every private conversation or knowing glance-- but it can help to think of all the ways one is not gathering data in a specific space.
2. *Many times, virtual spaces are created with specific intent, how does the intent shape the space and what occurs there?* For example, sociologist Robards (2017) studied a virtual forum (a subreddit) created for cis men identifying as straight to access gay porn and identity affirming interaction (2017). In the space, users and moderators encouraged non-judgmental discourse around fluid sexual identification. In other words,

the space was created with a specific intent which was reflected in the way the space was maintained (and, then, constrained). One might imagine a very different discourse in another forum which was created with a different intent. Whereas analysis of either space could perhaps provide interesting insight into sexual identity or the ideologies around specific norms or practices, either space would also likely be limited in the types of research questions they might reasonably speak to, given each's specificity and constraints.

3. *In what ways can the data be feasibly triangulated? Has this been planned for in the research design, questions and analysis?* Each study will likely have its own unique considerations around triangulation. A few methods of triangulation are discussed in the next section but for now the point is that it is at the phase of designing research that the ethnographer should think about how to go about collecting data such that data can be triangulated.
4. *What motivates, directly and indirectly, interaction in the virtual space?* For Colleen, money was a motivating factor. For many participating in social media spaces, influence, connection, popularity, and also income can be motivating factors. In such spaces, the norms of a virtual community can also motivate (or not) interaction in virtual space.
5. *Are you making any assumptions about data validity in the research design?* For example, there are inevitable risks in assuming that those anonymously posting on message boards are not merely spamming the board (or that they are not bots). For Goldman-Hasbun, assuming this risk and the validity of the data was central to the research questions they were able to explore. At the same time, there are also risks in relying on 'real' names and likenesses, as this does not preclude deception. Before getting into the field, ethnographers should ask themselves what kinds of threats to data validity might exist in the virtual worlds they are entering.
6. *Are there aspects of the research field which make it more or less vulnerable to deception?* In [Forberg's \(2021\)](#) study, there was an interplay between a norm of anonymity and an expectation that the space could be infiltrated by imposters. Driving this, at least in part, was that the space was dominated by individuals with extreme and politicized ideologies, which was thought to risk attracting those who did not share extreme beliefs. A norm of anonymity both reflected an assessment of this risk (in [Forberg's \(2021\)](#) words, that individuals using legal names could be doxxed), and a vulnerability to deceptive participants who for whatever reason might pretend to share the underlying beliefs of the space.

The data gathering & analysis² phase: identifying deceptive data

Confirmation and triangulation of data can be accomplished in several ways, both while gathering data and in analyzing the data. Some

sociologists have hired independent 'fact checkers' to double check their work, other methods of triangulation include verifying data independently, cross-checking multiple sources or mediums of data, verifying data across a number of interactions with the same subject, or cross-verifying data with different research subjects. Methods of triangulation will vary by study and possibly as the researcher learns more about the field. Colleen claimed to be a public housing resident, though she was not one. I set forth some guidelines based on the ways in which I identified Colleen as being deceptive about her qualifications and experience, as well as some methods for understanding deception which I adopted after my experience with Colleen.

1. *Compare data across research subjects.* I was able to understand that Colleen was most likely not who she said she was when I asked her standardized interview questions which she then answered implausibly. For example, I asked her how she acquired her public housing apartment. To this question she answered that she had filled out some paperwork and received the apartment within a few weeks. In comparison to the range of other stories I had heard--the apartment had been in the family for generations, they'd waited on lists for years and years, they'd come from a shelter--her answer was outlandish.
2. *Generate data to be triangulated independently.* After my experience with Colleen, I also further built in deception checks for interview participants, for example, asking them in synchronous conversations questions like "What is your favorite restaurant close to your home?" or "Where do you shop for groceries?" as this would produce data that could be easily triangulated by searching to see if that place indeed existed and was nearby a stated residence if a subject was able to produce a plausible, timely answer.
3. *Cross-verify stories.* After interviewing the 'real' Colleen, I became curious if the stories she was telling me were *really* real. I was able to cross check her stories with sources I found online in order to verify important parts of them. For example, I looked for anything related to her reported work as a university instructor, finding documents such as a press release about an award she had been given at the university she worked at, and a program for a conference.
4. *Require conversational synchronicity when possible.* Colleen was able to easily fabricate answers to questions that were not asked synchronously, in synchronous conversations she had more trouble.
5. *Take multiple observations.* It stands to reason that I would have been able to gather more insight into Colleen's fabricated identity across multiple interviews or observations. In an online ethnography, observations across time that yield cohesive, if evolving, narratives can confirm data validity or facticity.
6. *Take note of outliers.* Taking notes or coding for extreme views may help an ethnographer identify deception. Many of the stories that Colleen told were outliers--they were extremely different from the stories I had heard before responding to similar questions. In my own virtual ethnography of adoptive parents, I observed for more than two years parents who actively participated in social media-based networks. These parents existed across a range of beliefs about adoption as a moral good (or evil). In my notes and analysis, I noted where parents held

² Both this section and the next discuss the "analysis" stage. This acknowledges that analysis happens both alongside data collection and prior and alongwith the writing process.

extreme views about adoption (i.e., rigid ideologies that adoption was ALWAYS evil or ALWAYS good), and focused my study primarily on parents who were somewhere in the middle of that ideological spectrum. I did not discard the ‘extreme’ data, but I chose not to focus on it, and noted when I used data from ‘extreme’ research subjects. In the community, where extremism was *not* the norm, provocateurs with extreme views could sometimes seem more like ‘trolls.’ Whether or not the extreme subjects were deceptive, and I cannot say whether they are more likely to be deceptive or not, it did help me to make sense of my data to sort it that way.

I want to be clear that not every field of research would be able to accommodate these specific methods, and I am not suggesting one try to do so. Additionally, there are many traditional methods of verification of participant identity which require consideration according to individual research paradigms, for example asking for a legal ID. This type of identity verification might be important for some types of research, but could have negative effects in others. For example, in a study of public housing residents, a researcher might initially consider asking for a lease in order to verify identity. However, asking for a lease does not necessarily serve that purpose as thousands of ‘real’ public housing residents live in public housing off-lease. Asking for a lease might unnecessarily, and to the possible detriment of the study, overly-narrow a pool of research subjects. There is also the matter of rapport building--asking to see a lease also has the potential to alienate research subjects who are at times wary of revealing too much personal data online and could seem stilted when developing relationships with research subjects. Snowball sampling might also be a way to both verify identity and recruit research participants who are less likely to be deceptive if studying people who are part of specific communities. However, consider as well that research subjects might either be wary of identifying other individuals involved in sensitive communities or may pass on the contacts of other deceptive individuals.

The analysis & reporting stage: what to do with deceptive data

The issue of data validity and facticity is central for ethnographers, including practical matters around the handling of data and how to share invalid deceptive data with readers. So too, are questions around data transparency as an indicator of legitimacy and rigor in ethnographic study. Therefore ethnographers must consider what they will do with any deceptive data if collected when it comes to analysis and writing up of results.

Colleen was deceptive, though in my first interaction with her, I could not confirm that she was deceptive. Rather, I only could suspect she was because of the implausibility of her answers to my standardized questions. While the waste of time (and money, I did send her the promised incentive fee) was an annoyance, I did not accuse her of deception in our first interactions as I felt it would be unprofessional to do so. When my undergraduate Sociology students first read a draft of my article about Colleen, this is the thing that deeply troubled them. Shouldn't I have confronted Colleen about her deception and let her know that what she was doing was immoral? Generally, I would strongly caution ethnographers to avoid emotional confrontations or unreflective accusations of deception in the field, as I imagine it would not be productive in most cases. If an ethnographer feels that an interaction or observation might be based

in deception, it might be worth reflecting first on ‘why’ before making further decisions about how to deal with or further confirm deception.

Data transparency requires that ethnographers make transparent “how they know what they know” so that other scholars may judge for themselves the ethnographer’s claims (Murphy et al., 2021, p. 52). I suspect that many times, deception will be highly suspected but not absolutely confirmed. Even without confirmation, I felt that it would be wrong to include Colleen’s data in an analysis. I segregated her data, marking it as based on deception, and decided Colleen’s interview should not count toward my interview counts. Nevertheless, I also felt it was important to keep Colleen’s data and to deal with it as deceptive rather than pretending as if it did not exist (i.e., simply disposing of it). Simply burying deceptive data under the rug would not meet the ideals and requirements of data transparency. Neither would be the use of data without any thought to the possibilities of deception. Furthermore, how one handles and is transparent about deceptive data helps an ethnographer build trust with readers concerning their data more generally by giving the reader access into how thoughtful a researcher is around questions of data validity.

I’ve not come across many sociological studies that specifically discuss contact with intentionally deceptive participants, possibly because data that comes from interactions with those who intentionally deceive is thought to be not useful analytically. However, sociological studies do commonly note instances in which the accounts given by research participants vary from the researcher’s own experience. Data transparency requires that readers may analyze a researcher’s interpretations and consider other explanations as well as that researchers might ultimately return to data and re-assess it (Jerolmack, 2022). Ensuring transparency, while also ensuring data validity, might require the researcher to consider the following in terms of deceptive data when conducting data analysis:

1. *Reflect on whether suspected deceptive data can or should be further confirmed as deceptive.* When I first interviewed Colleen, I did not feel it was necessary to additionally confirm that her data was deceptive before treating it as deceptive because her answers to interview questions were such outliers.
2. *Segregate the deceptive data in analysis.* Instead of disposing of Colleen’s data, I clearly marked it and segregated it from other data.
3. *Retain your deceptive data as you do your non-deceptive data.* Deceptive data is still part of the dataset no matter how one treats it. Transparency does not necessarily require retention of data, though increasingly, data transparency requires that researchers not destroy their data (Murphy et al., 2021).

Additionally, researchers should also address deceptive data when writing results of research studies:

4. *Attend relevant considerations of the anonymization and redaction of deceptive data.* The ramifications of identification for deceptive participants may be different than those who are not deceptive. In a hyper-punitive ‘cancel’ culture, those identified as deceptive could be subject to far reaching and unpredictable consequences. This may be a consideration in studies where real names are used.

5. Address deceptive data in methodological explanations sections of research articles, parentheticals or endnotes, including why it is suspected (or confirmed) as being deceptive or invalid. This is one way to build trust with readers and aligns with the principles of data transparency. Giving your reader as much information as possible helps the reader evaluate your claims and to better understand 'how you know what you know'. As well, disclosing such information may help your reader better understand the social context of your study and those in which you generalize findings. Understanding deception that occurs in one study also may help the reader to understand more about what conditions produce deception.

As well, researchers should not assume that all deceptive data is without value. Facticity is important in ethnography, though not always paramount, and at times accounts which cannot be corroborated can be valuable to research (Murphy et al., 2021). At various times, ethnographers have chosen to foreground their own observations over the accounts of the perceptions of others, and other sociologists have chosen to highlight the perceptions of their subjects, even when they conflict with their own (Murphy et al., 2021). Inevitably, decisions as to whether or not to include data in analysis, or whether to foreground it or not, will require application of the researcher's judgment.

Ultimately, with Colleen, the decision to set her data to the side was a simple one--she was intentionally deceptive about the essential qualifications for the study, was motivated to participate by financial reasons, and had no personal experience with the field of study or anyone in it. However, without confirmation of this (only an understanding of her data as implausible) the initial decision to set her data aside was also a judgment call. The universe is eminently complex and a researcher cannot know everything. For example, one might imagine an alternate universe where Colleen *actually had* received her apartment after a few weeks of waiting. For example, off the cuff, she could have been illegally subletting and using a lightly invented cover story because she did not want to risk the illegality of her housing situation coming to light, or she could have been a criminal mastermind who engaged in identity theft to acquire the apartment, or a hacker who put herself at the top of the waiting list. Nonetheless, even considering a myriad of creative possibilities, I made a judgment that her data was, on the whole, implausible.

Of course, I eventually received an external 'confirmation' that Colleen was being deceptive and admission from Colleen herself that she was deceptive, but this is unlikely to be the case in most instances of deception. The principles of data transparency acknowledge the necessity of the exercise of judgment, while also safeguarding it by allowing others with different perspectives and bases of knowledge to assess whether the case is convincingly made (Murphy et al., 2021).

Conclusion

While working through the peer review process originally while writing about Colleen and virtual interviewing (Owens, 2022), one reviewer was deeply concerned about how the presence of fraud or deception might seem to invalidate or discredit virtual interviewing as a methodology. I do not share this concern about virtual interviewing or other virtual methodologies. If anything, my experience with Colleen, anecdotally at least, shows that identifying

and contextualizing deception in virtual research is readily possible and possibly valuable.

In this essay, I take the liberty of identifying prescriptive guidelines concerning deception which researchers might ask themselves or choose to implement when designing methodology for ethnography of virtual spaces. I want to reiterate the (perhaps) obvious that each virtual space will be different, and will present its own considerations, advantages and disadvantages for research and research questions. This essay is a provocation for research designers, and is not meant to indicate that certain virtual spaces are valuable or invaluable in terms of the data they may produce.

The various challenges of virtual ethnography, as well as its promise, are known. The centrality of the virtual in social life is clear, and Sociologists must include virtual spaces in their work to continue to shed relevant light on social processes. Virtual ethnography presents extraordinary possibilities, even while "methodological training [for virtual ethnography] is still in its infancy" (Urbanik and Roks, 2020, p. 213). Promisingly, the 'natural experiment' of ethnographic methodology in the years following the Covid-19 pandemic has been an excellent opportunity to understand more about the risks and methodological differences between in-person and virtual ethnographies. It is imperative for researchers involved in these studies to memorialize the methodological puzzles they encountered over the course of their research, including how they sought to confirm data. To do so will further establish the gold standard status of virtual ethnography in terms of methodological rigor as the discipline progresses further into the study of virtual social life.

Data availability statement

The data analyzed in this study is subject to the following licenses/restrictions: confidentiality. Requests to access these datasets should be directed to lisalucile1@gmail.com.

Ethics statement

The studies involving humans were approved by Institutional Review Board, Columbia University. The studies were conducted in accordance with the local legislation and institutional requirements. The participants provided their written informed consent to participate in this study. Written informed consent was obtained from the individual(s) for the publication of any potentially identifiable images or data included in this article.

Author contributions

The author confirms being the sole contributor of this work and has approved it for publication.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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OPEN ACCESS

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RECEIVED 20 January 2023

ACCEPTED 25 October 2023

PUBLISHED 16 November 2023

CITATION

Goldensher LO'D (2023) Problems of knowledge, problems of order: the open science field site.
Front. Sociol. 8:1149073.
doi: 10.3389/fsoc.2023.1149073

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Problems of knowledge, problems of order: the open science field site

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Ethnographers can and should not just *do* or *not do* open science, but *study* the push to share data, instruments, and other research materials as an important moment of change and contest in contemporary knowledge-making and knowledge politics. Following ethnographers of science and technology who have demonstrated the analytic opportunities afforded by moments of scientific controversy, we should treat the places where these calls are made, debated, and taken up as important field sites for ethnographic inquiry. Whenever and wherever the sharing of data, instruments, and research is discussed, planned, done, measured, judged, or regulated, there are powerful claims, visions, and action concerning what makes for facticity, legitimacy, and credibility in both research and politics. From these sites, I argue, we can observe changes to disciplinary and popular understandings of epistemic virtue, or what makes for reliable, factual, or adequately transparent knowledge production. Attention to these sites can also yield important perspectives on the ways that visions of proper research conduct are imbricated with visions of governance. I argue that turning ethnographic methods to studying the open science movement can enable us to do timely scholarship about shifting understandings of facticity, knowledge, information, and governance.

KEYWORDS

open science, data, ethnography, epistemology, democracy

Introduction

In October of 2022, I sat in a library workshop designed to train faculty at my university in practices of “Open and Reproducible Research.” I toggled back and forth between my Zoom screen and the Center for Open Science (COS) project workspace I was learning to use. Our instructors emphasized that the aim of the workshop was to teach us how to apply a set of valuable practices to our own research routines. In the first module of the curriculum we'd be following, we'd learn data management principles and practices, and we'd become familiar with some of the repositories in which we might store data. In the second module, we'd learn how to find and create reproducible research protocols and methods. We'd learn about how and why to engage in newer practices associated with open science, like peer review of research protocols. We'd learn to share protocols and check them for transparency according to various guidelines. In the third module, we'd learn about how to organize research collaborations—using the COS tool, around which I was clicking. Lastly, we'd learn why and how to share research materials. The workshop promised better research through these principles and tools: “Both of us,” one of our instructors told us, “just want the best research that can be done in the best way.”

The best research, done in the best way. As we began our first module, the idea of an optimal, open, and optimally-open way to do research continued to echo. Data management, our

instructors defined, “looks like having well-documented and well-organized data, ideally in public repositories.” Storing our data in a personal Dropbox or Google Drive was, they affirmed, better than storing it locally on a laptop, but “what you really want,” the leader told us, “is an archive, some kind of repository that is maintained by librarians, or archivists, or other people who are interested in long-term preservation.” I began to recognize that I was not just in a workshop about technical questions of data storage and retrieval. Instead, I was in a conversation about what makes for good, even ideal, knowledge-making: how long artifacts should persist, where they should reside, and who should manage and see them. Behind the nuts and bolts of file storage lay a set of rich claims about norms, values, and epistemic virtue (Daston and Galison, 2007).

Like the ethnographic “reckoning” (Murphy et al., 2021) with power and representation that yielded new standards for analyzing positionality and reflexivity, contemporary calls for conventions like data- and code-sharing or advance registration of hypotheses and research designs mark an important moment of change in knowledge production. How should ethnography respond to this push for “open science”? In this article, I argue that we should take these claims about the best way or the right way to do research as objects of analysis. Ethnographers should examine the places where open science is defined, made, evaluated, and practiced as field sites. Here, ethnographers can locate contest and struggle over epistemic virtues in contemporary knowledge politics and governance. Whenever and wherever the sharing of data, instruments, and research is discussed, planned, done, measured, judged, or regulated, there are powerful claims and visions about what makes for facticity, legitimacy, and credibility in both research and politics. We can certainly track these contests at the same time as we ask and answer questions about which practices make ethnographic data transparent in the ways we wish it to be, whether and where data should be stored, and to whom it should be made available (Reyes, 2018). By turning ethnographic methods toward the debates, uses, and regulation of open science, we can and should find potent analytic opportunities to study the relationship between ideals of knowledge-making and visions of governance.

In advancing this argument, I reflect on both my own experiences in my university’s very first workshop¹ training faculty in practices of open science and on broader policies, standards, infrastructures, and organizations of open science. I use this workshop as an entry point through which to consider the networks of actors surrounding the matter of open science—from scholars to philanthropists, software

developers to publishers—where ethnographers and our collaborators in disciplinary locations from human-computer interaction to comparative historical sociology can to ask important research questions about facticity, legitimacy, and epistemic virtue.

Open science, an intellectual movement whose emergence is typically dated to the early 2000s, means different things to different people (see Borgman, 2012). Actors have advanced a wide range of motivations, definitions, accounts of practices adequate to those motivations and definitions, and even positions on what terms to use, such as open science (Nielsen, 2011; National Academies of Sciences, Engineering, and Medicine, Policy and Global Affairs, Board on Research Data and Information, and Committee on Toward an Open Science Enterprise, 2018), open research (Nosek et al., 2015), and open data (Chauvette et al., 2019). Some, especially those who call for science to be opened in order to facilitate validation of existing scientific findings, emphasize the possibility of replicating scientific experiments—and the sharing of research protocols and data that would enable replication (Open Science Collaboration, 2015). Still others emphasize the sharing of publicly funded research with wider publics through open access to publications; others the facilitation of easier and faster collaboration across disciplines, institutions, and between career scientists and non-academics; others new measurements of the impact of a piece of research. Projects from data sharing and open access publishing to citizen science and nonacademic participation in research have gone under the name of open science (Fecher and Friesike, 2014). The notion that it is ideal that something be intentionally put in a public place or made accessible, however, is at the heart of many of the most commonly-circulating definitions: open science, according to one of its central proponents, is “the idea that scientific knowledge of all kinds should be openly shared as early as is practical in the discovery process” (Nielsen, 2011). Most commonly, the terms “open science” and “open research” are used to invoke the sharing of not only knowledge products, but the raw materials of their creation (see Nosek et al., 2015; Open Science Collaboration, 2015), such as protocols, data, and instruments.

Why should ethnographers study the push for open science? After all, psychology, not ethnography, is the field in which questions originally arose about how and why to make the raw materials of research available (see Maxwell et al., 2015). This article opens by considering this question, noting the ways that ethnographers have found rich sites for exploration in open science projects, data-sharing practices, and scholarly conversations about—and understandings of—transparency and open data. I then suggest a new focus for empirical studies of open science projects and calls for open science. I argue that ethnographers and other scholars of scientific knowledge should study the sites that are defining and making open science because calls for open science are calls to shift practices of inquiry and knowledge construction—and they are calls to shift the norms that guide and are used to assess these practices. Treating these as field sites would allow ethnographers to bring transparency to what has been called the “transparency movement” (see Elman et al., 2019). Turning to justifications for data-sharing from in and beyond the workshop I attended, I show how calls for open science often contain powerful and novel claims about what research practices make for reliable knowledge. Whether or not open science calls suggest that researchers should aim for verifiability or reproducibility, they are, as I show, calls to do different things in the process of making facts and drawing interpretive or analytical conclusions: to plan research in novel ways,

¹ While this article encourages ethnographers to undertake systematic studies of sites like this workshop, I did not myself do so. Because the descriptions I offer here are reflections on my experiences and the questions I later asked, not findings from systematic inquiry, my IRB determined that this project is not research involving human subjects and therefore did not need IRB review or approval (see VT IRB determination number 23–659; for more on exclusion from IRB review, see Lederman, 2004, 2007). However, I still checked in ahead of time with workshop organizer/trainers to let them know I was considering writing about my experiences in the workshop and to ask if there was anything they’d like me to keep in mind or to do or not do as I participated and wrote. On the day of the workshop, I included in my introduction to the group a note that I planned to write about the workshop, but not to identify anyone by name.

to collect and record data differently, and to share and store the materials of research in new arrangements and new institutions. As sociologists of scientific knowledge have long demonstrated, when those who do the day-to-day business of getting knowledge-making done begin to do so differently, important social and cultural work is going on—and ethnographers have the right methodological tools to examine the shifting practices through which that work unfolds. Through a reading of STS literatures demonstrating how studying moments of dispute and change in scientific practice can yield important insights about epistemic virtue, or understandings of how scientists ought to behave, I show that calls for open science are more than just suggestions that researchers to try out something new in the process of making knowledge. They are also calls for scholars, publics, and policy-makers to use novel standards to recognize true, authoritative, trustworthy, or reliable knowledge. And because shifts to epistemic virtue are moments when moralities, understandings of the right relationship between science and state, and visions of social authority are contested and renegotiated, these calls offer important opportunities for us to consider classic questions about the relationship between knowledge and order.

I then turn to these standards and to the infrastructures in which they are embedded. Calls to share data and research materials are linked to important changes to standards and conventions through which institutions discern the difference between spurious knowledge and reliable knowledge that should be circulated, supported, cited, and used to make decisions. I show that because practitioners often frame open science as a kind of democratic good, especially for use in governance, ethnographers can investigate these standards not only in scientific and research communities, but also in policy-making. Open science initiatives matter, in other words, for changes to the ways that authorities within and beyond the scientific or scholarly community recognize facticity and legitimacy. This makes calls for open science—and the policy changes that have come in response—key moments in both scientific and political constructions of truth and reliability in information. Taking metrics and curricula from my own discipline and from the workshop I attended as an example, I offer some initial suggestions about questions researchers might ask as we follow open science into policy and politics. In the article's conclusion, I suggest that ethnographically studying claims about what makes for good knowledge in science and governance can both catalyze timely research about the politics of facticity and more rigorously inform ethnographers' own data-sharing practices.

Defining and justifying: ethnographic studies of open science

What does it look like to study open science ethnographically? Ethnographers and other qualitative and interpretive social scientists have found open science to be a fruitful object of inquiry in a number of ways. Some study open science projects as sites from which to examine classic problems in research areas wherein ethnography is already a commonly-applied tool. In this approach, an open science project like a distributed computing partnership can be a chance to investigate the techniques and technologies used to know and manage working at large scale—by, for instance, by observing the metrics and tools technical specialists use to make estimates (Ribes, 2014). Likewise, a gathering focused on open-source software

documentation can afford an opportunity for ethnographic investigation of documentation as a social and organizational practice (Geiger et al., 2018). Open science projects can be cases of more general phenomena, and analysis of open science can take an approach that emphasizes continuity with existing scientific or technical practice.

Other studies turn more directly toward open science, treating matters such as openness, data sharing, or data reuse as objects of analysis. Some take up conceptual problems raised by calls to open science: which elements of what might count as data are “signal,” which are “noise,” and how does variety in the ways different researchers assign those classifications to the same objects matter? How might use of data be analytically distinguished from reuse? What is the relationship between one or more datasets and one or more pieces of published research? Investigators of these problems note that there are rarely single or simple answers to these epistemological and phenomenological questions (Leonelli et al., 2016; Pasquetto et al., 2017; see also Borgman, 2012; Wynholds et al., 2012). These definitional matters translate directly to practice, and when researchers ask scientists who advocate for or understand themselves to be doing open science, they find great disagreement over not only what openness means, but what goals should drive open science and what scientists should do in opening their work (Grubb and Easterbrook, 2011). Scientists' senses of when and how it is appropriate or important to either share data and research or try to replicate or reproduce a study are shaped by a range of institutional, technical, and policy factors—disciplinary career pressures and cultures of competition, varying computational demand in research and availability of data repositories, and the constraints on sharing introduced by disparate ties to industry (Levin et al., 2016).

In my workshop, disciplinary variation in what it might mean to do open science was front and center. “Please stop us,” the facilitators told participants at the start of the workshop, “if you are like, ‘hey, how does this make sense for social science, or for qualitative [research]?’” They had shared a short definition of open science: “open as much,” they glossed, “as is feasible, while being protective and careful.” Answers to the question of what could be feasibly shared, they noted, might be motivated differently in different disciplines—the business school, for example, might embargo information in order to protect intellectual property, while social scientists might aim to protect human subjects from identification. For some researchers, describing one's methodology in terms detailed enough to enable reproduction might mean specifying amounts of a reagent—and for others, a number of interviews.

When researchers examine the range of enactments that go under the name of openness, they also find more than disciplinary disagreement over what protectiveness, being careful, and feasible opening might mean—and over the question of whether a maximalist, as-much-as-possible approach is even the right one to take. Disagreement over the question of what makes for careful or protective opening is closely linked to the question of how to define data and its situatedness. Designers of the Platform for Experimental Collaborative Ethnography, for instance, have suggested that digital tools developed for the natural sciences must be redesigned in order to represent the embeddedness of ethical matters in a given ethnographic data object (Fortun et al., 2016, 16). Those who study open data across political economic contexts, meanwhile, find that definitions focused narrowly access to data resources fail to account for the unequal conditions

under which those data might be taken up or re-used (Bezuidenhout et al., 2017).

If narrow definitions of data, data use, and data circulation can fail to deeply account for the imbrication of data with people, communities and their “ethical and political imaginaries,” unequal distributions of resources, and histories of exploitation and extraction, then maximalist approaches and mandates to share data can “retrench historically exploitative relations of knowledge production,” too (Okune et al., 2022, 2–3). Disagreement over what it might mean to be careful or protective in opening data is also linked to the difference, as the coordinators of the Open and Collaborative Science in Development Network put it, between approaches that “purposefully acknowledge and seek to redress power relations within a given context” (Okune et al., 2018, 14) and those that do not. The OCSDNet team, for instance, review cases from an international disaster management collaboration between academics, technology developers, and disaster response officials in the Caribbean; an online biological collections-sharing system in Brazil; and a South African Indigenous community’s discussions with NGO and university-based climate change researchers. They find that when projects treat data as fundamentally situated and define “being careful” as aiming to disrupt exploitative arrangements, open science often does not look like putting something into a public place. Instead, it might look like designing deliberately inclusive infrastructures: a shared vocabulary for collaboration, a data and research framework that accounts for variety among participants in the degree to which openness matters or is desirable, or a contract guiding the terms on which a community might negotiate with future researchers who hope to “open up” their local knowledges (Okune et al., 2018, 8).

Not all calls to share about data, meanwhile, call themselves open science. Ethnographers and other researchers whose methods involve sharing social space with research participants have consistently demonstrated how race, gender, and sexuality matter in the process of making not only knowledge, but data. Researchers’ identities and appearances “structure,” as Tey Meadow puts it, all aspects of the “significant interpersonal relationship[s]” that occur in field research (Meadow, 2013, 467). They are “tools” or “resources” that researchers mobilize, sometimes by “amplifying” and sometimes by “minimizing,” sometimes “strategically and actively” and sometimes through “management of others’ reactions” (Meadow, 2013, 476; Schultz, 2019, 184). The conversations enabled and precluded by researchers’ mobilizations of these identities and appearances leave both presences and absences in the resulting datasets. Traces of those relationships and those identities are also embedded in individual pieces of data, as Ann Morning shows in her descriptions of how interview participants cited their perceptions of her race as evidence in the arguments they made in response to her questions (Morning, 2011, 108). Datasets are also shaped by violence that occurs during their making, and as research about sexual harassment and assault in fieldwork has demonstrated, that violence is often omitted by convention from narratives of data gathering (Hanson and Richards, 2017). Different kinds of openness from data-sharing—what Hanson and Richards call “open discussion” of sexual violence (Hanson and Richards, 2017, 601; Su and Phi Hong, 2023), or what Victoria Reyes describes as “being open about the who, what, where, when, and why of data collection” (Reyes, 2019, 187)—are often invoked by ethnographers who call for accounts of the social shaping of field research.

These definitions of openness are not the ones institutionalized in policies such as the Biden administration’s 2022 directive requiring open access publication and data-sharing in federally-funded research, or analogous European Plan S directives for research funded by European public grants. Likewise, these definitions—and these practices—were not the ones we learned about in the workshop I attended. Even across the range of disciplinary contexts we discussed, some key inclusions and exclusions persisted: the audience for open science was made up of professional researchers, not groups of collaborators including both academics and non-academics. The workflows for opening our research that we reviewed—from registering hypotheses and research protocols in advance to archiving data—all involved putting some research material into a public place, rather than depicting data provenance and associated ethical matters or designing infrastructures to disrupt dispossession. Of course, training professional researchers in the skills necessary to comply with policy protects their ability to do their work without penalty. Yet these inclusions and exclusions legitimate some definitions and enactments of openness, while obscuring others.

In so doing, however, they offer ethnographers rich opportunities to observe struggles over what openness means, what it should mean, and how those meanings might translate into practice. Expanding on maps of definitions of open science that are in use, ethnographers might also trace the paths of divergent definitions and track conflicts between definitions and their disparate implications. When, where, by whom, and with what consequences are disparate definitions of open science, data, use, and reuse deployed? When and where are definitions that are and are not attuned to dynamics of power and exploitation taken up, or replaced with others? Which definitions appear in didactic or regulatory settings, and which are absent or omitted? What happens in moments when multiple definitions collide?

As open science policy continues to proliferate, opportunities for policy ethnography do as well. Analysts have suggested that empirical findings about scientists’ understandings of open science can shape a policy landscape whose guidelines and mandates are new and shifting (Levin et al., 2016, 139), and that findings about what happens *after* data are shared can answer key questions about whether mandates are actually leading to the reuse they hope to foster (Pasquetto et al., 2017). Empirical study of what researchers and nonacademics do with guidelines and mandates can not only yield findings that help refine policy about open science, but can also help us understand how knowledge-making is happening in a moment of major change to the backdrop of rules against which researchers’ day-to-day work unfolds. Knowing that rules and guidelines have unintended consequences, we can also ask another set of ethnographic questions about open science. How do researchers actually navigate the spaces and contradictions between enactments of openness legitimated in policy and others? What do researchers and their collaborators outside of universities do, in the process of their work, with the mandates and guidelines they receive? Do they wholeheartedly adopt them, mimic them even in their absence, flout them, strategically define their data in conversation with them, ignore them, challenge them? If a study of a distributed computing or open-source software project can analyze open science as a case of a broader social and cultural matter like collaboration and documentation, asking questions like these can treat the open science field as a site for the examination of an urgent, and much broader, social and cultural shift: changes to—and struggles over—what makes for good knowledge.

Suspicion and facticity in calls for open science

Beyond open science's legitimation by policy, legitimacy matters in another way: calls for open science are often closely linked to calls to recognize some knowledges as legitimate or credible, and others as illegitimate. About a quarter of the way through the workshop I attended, we had learned some technical skills that might be sorely needed for the more disorganized among our ethnographic colleagues—myself surely included—regardless of whether we plan to share data, codes, or research protocols. We had linked our workspaces to an external drive, learned to name our files according to legible conventions and clear directory structures, and reviewed dictionary structures through which to define our data. As a new section of the workshop on reproducible methods² and registrations began, our instructors defined some terms. Reproducible methods and registrations, they glossed, meant “ways to make it so other people can do the same thing as you.” Our instructors suggested we let go of the idea of “getting the same results” as central to reproducible methods. What is most important, they told us, is giving other researchers enough information to enable them to do the same thing you have done, or to understand how you arrived at your result.

Why share what you have done? Our instructors offered a few justifications: they pointed out that thinking through what someone else would need in order to understand one's research process also produced robust reminders to one's future self. They flagged equity problems, pointing out that if methodological descriptions are contained only in paywalled research articles, scholars without access to the journals in question cannot learn from the innovations of their peers. They emphasized the possible career benefits of sharing one's protocols: one's work is more likely to be read, cited, and included in future research if one's protocol has been seen. They noted the gaps in the availability of what future researchers would need to build upon a study's findings that many articles' methods sections include: in a randomized controlled trial, for instance, it will be difficult for future researchers to follow-up on a comparison of an intervention to “standard methods” if they find themselves wondering just what those standard methods were. And they reported a statistic often cited in open science discussions: that somewhere between 40 and 90% of studies are not replicable due to incomplete reporting of methods.

This figure is at the heart of one of the most common claims uniting often-disparate actors in the open science movement: that “science in practice is problematic” (Brezna, 2021). The problem in question is often identified in terms that equate, or at least associate, ethicality and dependability with the ability to review the raw materials of research and produce the same results as a published paper. For commentators identifying a “crisis of reproducibility,” as

Sabina Leonelli writes, “failure to reproduce results...indicate[s], at best, problems in the design and methods used by the researchers in question, and at worst, a fundamental lack of credibility for the knowledge thereby obtained” (Leonelli, 2018, 2). One sociologist, for instance, described a “crisis” of science that is “less reliable, reproducible, and ethical than policymakers, the public, and other scientists expected or previously believed” (Brezna, 2021, 2). Calls for open science, including some calls in my own discipline of sociology, often show their close associations with the related phenomena of “replication crisis” (Maxwell et al., 2015; Shrout and Rodgers, 2018) and “credibility revolution” (Vazire, 2018) by opening with accounts of scandal in psychology, economics or political science. Their citations of exposures of fabricated results (Broockman et al., 2014) and falsified and selectively reported data (Herndon et al., 2014; Kotlikoff, 2018) raise a common specter (see Brezna, 2021): *what if researchers cannot be presumed to be acting in good faith?* By citing disciplinary conversations in which analysis of the hazards of common research practices like “p-hacking” (Simmons et al., 2016) was closely followed by revelations that large swaths of a discipline's published literature contain spurious, impossible-to-replicate findings, they raise another (see Engzell and Rohrer, 2021): *what if none of this is true?*

Ethnographers might well recognize these concerns. Despite disavowals (see Tsai et al., 2016; Freese and Peterson 2017, 159; Murphy et al., 2021, 43) of replicability or reproducibility as a goal of open research practices in ethnography, calls for open science are haunted by this sense that in order to know that facticity has been achieved and that malfeasance is not occurring—that researchers are not lying in their reports of facts, analytical conclusions, or interpretations—we need more than trust. In the specific world of ethnography, as Murphy, Jerolmack and Smith emphasize, calls for data-sharing have also often been justified with reference to a sense that ethnographers may not reveal enough about our methods and motivations, and that our disciplinary conventions of anonymity and fieldnote privacy “arouse suspicion that the researcher may have something to hide” (Murphy et al., 2021, 42)—and that something may be misconduct or fabrication (see Singal, 2015). Without being shown where researchers went, who they talked to, and why, this line of reasoning suggests, we cannot really know that what they claim is true or that they are trustworthy actors. Echoing concerns that science faces a “crisis of replicability,” the sense that ethnographers might not be adequately achieving facticity haunts many calls for open ethnographic data.

Replication and reproduction have been critiqued as either universal guiding goals of opening science or universal measures of reliability in research—indeed, reproducibility itself has different meanings in different contexts (Leonelli, 2018, 3–4). And many calls for open science suggest that more revelation of scientists' tools and raw materials will improve research in ways that have little to do with replicating, reproducing, or demonstrating the steps of research in service of credibility. In addition to the equity, discoverability, and research development benefits my workshop highlighted, others argue that sharing data and research procedures will just make for faster, more accurate, more creative, and more advanced science. Using an analytic tool another team has already road-tested, the reasoning goes, simply increases research efficiency, lowers the likelihood of errors, and allows for more advanced methodological development when compared to starting from scratch. Our workshop leaders, shared an example of protocol sharing across

2 Our instructors differentiated reproducibility from replicability: they explained that reproducible research is identified by the ability of another researcher to apply the same methods to the same materials, or to similar, pre-existing data, to achieve the same results. Replicable research, on the other hand is identified by the ability to implement the same methods for data collection and analysis to arrive at the same results. It is a matter, they told us, of the difference between internal and external validity: “You can reproduce something,” we learned, “that was wrong in the first place.”

disciplines, demonstrating the ways research protocol sharing enabled researchers extracting RNA from primary cortical neuron cultures to adopt a method originally developed by researchers of fish parasites. In disciplines outside the natural sciences, scholars in STS and anthropology have suggested data sharing for the possibility it offers of revealing unforeseen insights by encouraging open-ended collaborations across time and space and multiple interpretations of the same data. The collaborations enabled by open methods and digital archiving of data, they suggest, might traverse disciplinary and methodological boundaries, and carefully-constructed platforms might make it possible to credit a broader range of interlocutors and non-academic collaborators for citable data contributions (Fortun et al., 2016, 3; Okune et al., 2022, 6).

But other dominant framings of the problem that open science seeks to solve invoke, as Moody, Keister, and Ramos put it, Louis Brandeis' assertion that, "sunlight is the best disinfectant" (Moody et al., 2022, 74). Restricted access to scientific artifacts is sometimes taken as a kind of smoking gun, "interpreted," as Alicia Grubb and Steve Easterbrook write, "as an indication that scientists have something to hide, thus decreasing their credibility" (Grubb and Easterbrook, 2011, 1). Maybe scientists are acting badly, but under cover of darkness. Maybe their bad action is unfolding in ways we—their colleagues, the public, the funders and taxpayers who give them money, the policymakers and healthcare providers and regulators who use their findings to inform practice and lawmaking—cannot perceive. Maybe, by that concealed bad action, scientists have been finding not facts, but untruths. And maybe, then, our follow-up research, our sense of how the world works, our policies, our healthcare interventions, and our regulations are built not on the solid foundation of truth, but on the shifting sands of fabrication, misuse of methodological tools, *post-hoc* hypothesizing, and opportunistic decisions to exclude data and selectively analyze or report variables, measures, and research conditions. This possibility preoccupies many calls for open science, and it drives the sense that there is a relationship between facticity, legitimacy, and the degree to which others can see what scientists did to arrive at their results.

Inadequate revelation of what scientists did to arrive at their results is just one way this problem is identified, and our instructors encouraged us to loosely hold the goal of reproducing the same results. But as they suggested particular tools for registration, the sense lingered that part of what these tools could offer was, to be colloquial, receipts: "By preregistering a protocol and having a timestamp and a [record of] versions," one told us, "you have evidence that 'hey, I planned this analysis ahead of time. I did what I said I did.'" This, they told us, could help avoid accidental p-hacking—but it could be useful for qualitative research, too. If researchers register plans and data collection, for instance, they told us, then "we know that this was for real."

This is for real, and I did what I said I did: implicit in specific calls for data-sharing in ethnography and many general calls for open science is a sense that there is a set of practices that, if used correctly, could reliably yield facticity and legitimacy. To believe that truth has been found, calls for open science often suggest, we need to see that something particular has been done. But ethnographers need not only respond to the suspicion of calls for open science or for data-sharing in ethnography by amending our own practices. Instead, we can treat this suspicion—and the suggestion that often accompanies it that there are correct ways to act in order to find facts—as a signal that

open science is a site where we can study the potent relationship between moralities and facticity.

Doing science the right way: epistemic virtue

For the past half-century, sociologists of knowledge and scholars in and near the field of science and technology studies (STS) have challenged the idea that facts are out there to be discovered, if only we do things right, and that researchers are extracultural figures who could avoid corrupting science with social practice. These scholars have suggested that if we want to understand where facts come from, we can attend to what scientists *do*, the cultural settings in which they do it, and the normative precepts by which they discern what *doing things right* might mean. This might involve, in the tradition of laboratory ethnography, observing the ways that individual scientific facts are distinguished from alternatives—not purely deductively or logically, but instead by attaining meaning and significance in "microprocesses of negotiation" between human investigators and nonhuman phenomena from laboratory layouts to schedules (Latour and Woolgar, 1979, 42, 135, 145–6, 158). In letting go of the idea that there is a realm of pure science outside of culture where truth is discovered, ethnographers gain the chance to observe the ways that a scientific discipline is a distinctive cultural setting with its own conventions and patterns, "cultural machineries," ontologies and semiological systems, and understanding of "what it means...to work empirically" or to measure (Knorr-Cetina, 1999, 9–12).

Many powerful studies of scientific action have attended to the unfolding of the everyday process of fact-construction, but moments of disruption afford particular opportunities for analysis. Such moments include both emergence—"paradigm shifts" and moments in which new forms of scientific thought evolve (Fleck, 1979; Kuhn, 2012)—as well as conflicts, disputes or controversies (Latour, 1987, 258; Nelkin, 1992). Scientific disagreements are good places to understand the social and cultural work of settling or stabilizing facts. But in controversies over the formation of new fields like genetics or climate science, we have natural laboratories for observing whole belief systems, methods of inquiry, or perceptions of reality (see Fleck, 1979; Edwards, 2010; Hilgartner, 2017).

This latter kind of dispute is a normative, even an ethical one: it is over the unsettled question of what scientists will recognize as "epistemic virtue" (Daston and Galison, 2007, 16). Controversy gives us the chance to ask a key question: "How and why were certain practices and beliefs accounted proper and true?" (Shapin and Schaffer, 1985, 14). Practices and ideals that we might now take for granted as the right ways to act or the right ways to observe in pursuit of scientific fact—using experiments to produce credible proof, for instance, or aiming for objectivity in representations—are, these scholars have shown, in fact the *ends* of dispute (Shapin and Schaffer, 1985; Daston and Galison, 2007, 17–18). They are forms of conduct for which scientists began to strive in a particular moment, and they are definitions of the way scientists ought to behave that have often coexisted with others. Controversy studies have shown that where there are claims that a particular sort of conduct in science is right and another kind is suspect or wrong, there is analytic opportunity.

Some time after the workshop I attended, I sat down to review a journal article and found myself remembering something. One of the

ways to we'd learned to identify the "baseline" of what needs sharing by way of data or analytic tools was to think about what would be necessary in order to review a paper. "It's often difficult [when] there's not enough data and code to see what actually happened and tell whether they are doing a good job," our instructor elaborated, leaving reviewers to "sort of tease it back out from the tables." Remembering this, I found myself thinking about the different ways we might identify the right way to do peer review. When I review an article—a task I am more likely to do for a qualitative than a quantitative paper, thus leaving me with fewer tables to tease out—do I see it as part of the purview of my task to not just evaluate a description of the methods used, but to follow and evaluate each step of analysis? It struck me that behind this definition of the task of reviewing, too, was a particular moment in which epistemic virtue was defined, a particular definition of what it means to see enough to evaluate the quality of research.

It might be tempting to describe this kind of inquiry as a sort of dodge, or a way to sidestep the hard work of empiricism. Why try to make a "reasonably reliable rendering of the social world" (Duneier, 2011, 2; see Murphy et al., 2021, 43), a skeptic might argue, when we can instead sit back, relax, and declare that there is no such thing? My suggestion that ethnographers study the social and cultural action by which facts are constructed and epistemic virtues are worked out in open science, is, however, not an abandonment of empirical work. Instead, it is a call for more of it. It is not a turn against the idea that something really is happening in the social world, something from which we can learn if we pay attention. Instead, it is a call to understand the sites where knowledge is made and the conditions of its production are debated as part of the social world, deserving of ethnographic attention, and capable of revealing a great deal. Readers might indeed have "a right to a reasonably reliable rendering" (Duneier, 2011, 2) of the social worlds of science and technology—and of the social worlds where prescriptions for the right way to do research are formed.

In the moments when new epistemic virtues arise, and in struggles over the degree to which they will win out, what is hashed out exceeds visions of the right way to act in a laboratory or understandings of the kinds of actions that make for credible knowledge or adequate proof. Instead, as Shapin and Schaffer famously put it, "solutions to the problem of knowledge are always solutions to the problem of social order": moments in which we see shifts to accepted modes of scientific action are also moments of political contest. Working out "the genuineness of knowledge" takes working out the structure of a community of knowers, and in renegotiations of scientists' roles and activities, we also see renegotiations of understandings of moral citizenship, of definitions of the relationship of scientists to states and politics, and of visions of the right ways to organize social authority (Shapin and Schaffer, 1985, 332–341). We think of epistemic virtues as if they are self-evident at our own peril. When we treat epistemic virtues as if they have been there all along, we not only misunderstand them, but we also lose analytic opportunities to ask about their emergence and to see what is at stake in the struggles that surround them.

If ethnographers recognize the emergence of open science as a moment of contest over epistemic virtue, we can track the consequential renegotiations of facticity, of social authority, and of morality that such moments inevitably contain. Open science advocates who see a crisis of legitimacy in scientific research have

suggested that inadequate facticity can be traced to "researcher degrees of freedom," or the myriad decisions researchers make in the process of analyzing and even collecting data (see Simmons et al., 2016). Ethnographers and historians of science, however, have turned this proposition on its head, showing how the social action of science does not impinge on or threaten objectivity. If the process by which facts are produced is instead always saturated with the contingency of culture, convention, dispute, and negotiation, then by "playing stranger," by finding gaps in the taken-for-grantedness and self-evidence of propositions about how to do science, we can gain great analytic opportunity. How might ethnographers take up this opportunity? Ethnographers of open science in practice might observe processes of opening and closing data, analytic tools, and research plans. As they make both programmatic and momentary decisions about what to share and how to share it, how do researchers negotiate within and between teams, definitions of facticity and empiricism, and material qualities of data objects, specimens, and technologies of analysis? How do scientists recognize or identify when they have "done a good job" at openness? How do factors such as researchers' understandings of their own—and one another's—roles, experiences of pleasure or enjoyment, or time matter for the process of opening research materials? Studying open science ethnographically in this way can help us analyze the shifts to practical understandings of facticity, good scientific conduct, and the right relationship between knowledge and governance in a contemporary moment of dispute and controversy.

Standardizing openness

We can learn a great deal about shifting understandings of epistemic virtue by studying the data- and research materials-sharing practices of individual researchers and teams of researchers. But as scholars of open science who study the platforms and policies of open science have argued, we might also fruitfully study the infrastructures, institutions, and classifications that organize, constrain, promote, and limit those practices. As Stephen Hilgartner puts it, opening and closing research—the "work to control which knowledge become available to whom, when, under what terms and conditions, and with what residual encumbrances"—does not "take place on an open field, unconstrained by history, identity and institutions." Instead, it is shaped by "regimes of closure" (Hilgartner, 2012, 267, 272). Regimes like these link knowledge and governance through "legal or quasi-legal forms," are embedded in "institutionalized discourses and practices" from publication and intellectual property law to disciplinary epistemic cultures, and deserve ethnographic attention too.

Critical scholars of open science who analyze infrastructures of scientific work have paid particular attention to platforms and systems for recording and sharing data and workflows. These analysts have emphasized that while some knowledge infrastructures are intentionally built to account for power, others take a one-size-fits-all approach that can retrench extractive and exploitative dynamics. Once infrastructures for open science are in place, they point out, actors do not all benefit equally and scientific work does not automatically unfold equitably (Okune et al., 2018, 2). For scholars in library and information science, studies of organizations, and other fields, documentation work like

that of recording the process and materials of research is central to the sustenance of social structures, documentation work is a “mode of collective sense-making,” and it is often through documents that people “make their understandings and intentions known to others” (Geiger et al., 2018, 772–3). If the design and use of knowledge infrastructures matters, these scholars have suggested that their ownership does too. Some analysts of open science have raised concerns that open science platforms could easily be dominated by profit-driven infrastructures owned by commercial firms that have been critiqued for their surveillant and extractive strategies (Sadowski, 2019; Dembicki, 2022, 3; see Pooley, 2022).

If we study open science with a particular eye to epistemic virtue, however, we might pay attention to research infrastructures in another fashion: we might study the ways that they “embody,” as Susan Leigh Star and Karen Ruhleder famously put it, standards and classifications (Star and Ruhleder, 1996) for research conduct. Empirical academic researchers, myself included, now do our work in the context of increasingly prevalent mandates and incentives to share our data—mandates and incentives that flow not directly from our employers, but instead through other institutions. The workshop I attended, for instance, was not required in any way. While it taught me a great deal, especially about the nuts and bolts of organizing my research in ways that might enable me to collaborate and retrace my own steps, I attended entirely out of my own interest. Neither data-sharing nor open science training are mandated by my university. And though I earned “professional development” credit for participating, I could have earned the same credit by attending, for instance, a workshop on PDF accessibility, or one about how to use a particular tool in the Canvas online learning site to which my university subscribes. If sharing the raw materials of research is to become a key signal of credibility and legitimacy in research or a key step in our common-sense definitions of doing research the right way, it might not happen because researchers are directly compelled by their employers.

For ethnographers interested in the negotiations of epistemic virtue that unfold around the questions of how and why to share research materials, this means that some of the action we can fruitfully observe may be located around the standards and policies of funding and publication infrastructures. “If you are doing funded research, US government funders almost without fail—and increasingly, other funders—will require these sorts of things,” our instructors told us. Openness standards are increasingly woven into disciplinary work at the level of publication, too. In my home discipline of sociology, major journals I frequently use in my research and teaching have adopted a standard set of Transparency and Openness Promotion guidelines (Nosek et al., 2015), including *Sociological Methods and Research* and, by virtue of an Elsevier-wide policy, journals such as *Poetics*, *Social Science and Medicine*, *Social Networks*, and *Social Science Research* (see Breznau, 2021, 8).

In funding and article submission workflows that encode incentives or penalties based on standards like the Transparency and Openness Promotion guidelines, we see infrastructure that embodies “judgements about what constitutes a legitimate intellectual contribution, for whom, and with what implications” (Levin and Leonelli, 2016, 283). Ethnographers of standards and standardization have emphasized that standards do not implement themselves:

instead, they are “tinkered with” in practice and rarely work as their designers intend them to (Timmermans and Epstein, 2010, 81; Timmermans and Berg, 2003). If ethnographers of open science treat the trajectories of these judgments standards as indeterminate, we have the chance to ask important questions: How, we might ask, are standards for adequate transparency and platforms for sharing data and other research materials actually being used in scientific spaces like laboratories and research teams? What forms of sense-making and what social structures in research communities are maintained through the use of publication and funding infrastructures that incorporate standards for data-sharing? How and when are these platforms and standards used in novel or unintended ways, navigated strategically or creatively, hacked or expanded? How is their use embedded in and enrolled in coordinating the day-to-day work of research? As they use research infrastructures that incorporate standards for open science, what do researchers actually do with the judgments they embody about what makes for intellectual legitimacy? And what do researchers do when they are caught between multiple standards for sharing research materials, or between standards for data-sharing and other normative systems?

Regimes of closure, Hilgartner reminds us, are multiple (Hilgartner, 2012, 274). Classifications and standards, meanwhile, emerge from action. “Someone, somewhere, must decide and argue over the minutiae of classifying and standardizing,” write Susan Leigh Star and Geoffrey Bowker (Bowker and Star, 2000, 44–5). The indeterminate histories of competing classifications and standards for open science are scenes of negotiation we might fruitfully observe. In disputes and negotiations over standards of openness, ethnographers have a natural laboratory of controversy and change in which to observe constructions of epistemic virtue. By studying when and how classifications and standards of openness are deployed, undermined, challenged, ignored, tinkered with, and embraced, we can treat mandates to share data and standards for openness not as automatic conduits for new definitions of epistemic virtue, but instead as rich sites from which to observe what happens to those definitions in scholarly and scientific practice.

Data-sharing for governance: open science and the problem of social order

Yet when scientists and other researchers publish findings, their legitimacy is not only appraised by funders, publishers, or other scholars. Classifications of research as credible, trustworthy, or done the right way reverberate beyond the laboratory, seminar room, or journal page: they are also closely related to the question of whether that knowledge will be treated as publicly and politically legitimate, well-enough-made to be used in governance. In studying calls for open science as moments of struggle over epistemic virtue, we can also track these calls—and the standards and infrastructures that surround them—beyond academic settings. In the following example, I offer a number of lines of inquiry that ethnographers might pursue in investigating open science’s significance beyond scholarly settings.

The idea that open science has something to do with politics is a common one. Development of those same Transparency and Openness Promotion guidelines now used by many sociological journals, for instance, was a central and early project of the Center for

Open Science (COS), a major open science organization founded with the goal of transforming the process of research and the ways research findings are assessed and used in policy-making. Although far from the only organization whose activities and initiatives sustain practices of sharing data or research materials, COS, which both hosts the Open Science Framework repository site I learned to use in my university's workshop and created the template for the workshop's curriculum, has become a major force in the creation of both standards and platforms for open science. The organization was launched in 2013 by philanthropists Laura and John Arnold's Foundation. The Arnold Foundation remained, with the Department of Defense agency DARPA and charities established by the late investor and philanthropist John Templeton, one of its three largest funders by 2019.³ The organization's work is guided by a fairly radical vision of data-sharing: "direct access" by "default" to the data used by scholars to support their claims and preservation of "all scholarly content" ([Center for Open Science, n.d.](#)). Founding COS is part of what Laura Arnold described as the Foundation's "aggressive investment" in "evidence-based policy" ([Arnold, 2017](#)). The Arnolds' work aims to promote policy-making guided by research findings—not just any research findings, but research findings whose legitimacy is confirmed by their use of particular methods, by the publicity of their research materials, and by their replicability.

In a 2017 Tedx talk, Arnold made the case for open science as an essential element of the solution to what she called a key policy problem: "We're routinely making all kinds of decisions based on incomplete, inconsistent, flawed, or even nonexistent data," she said. "These shenanigans happen everywhere." Arnold summarized the Reproducibility Project that spurred psychology's "replication crisis," another project of COS led by its co-founder and director Brian Nosek ([Open Science Collaboration, 2015](#)). "We asked researchers to reproduce 100 psychology experiments that had been published in top psychology journals in 2008," Arnold explained. "You know how often they could find the same results? One third to one half of the time." This publication of unsuccessful attempts to replicate psychological experiments, Arnold said, demonstrated "something that we see throughout academic research and throughout research in general:

virtually everywhere you look, you'll find researchers, many of them prominent and most of them well-intentioned, actively misleading us into believing that bad research is proven fact." This might, Arnold told her audience, not be a problem of intentional fraud. When researchers, for instance, "cherry-pick" positive findings on secondary outcomes and report those instead of reporting null findings on their original hypotheses, they might be doing so because of institutional pressures. "The incentive system in science and research is broken," Arnold went on. "Scientists and researchers are motivated by the desire to publish, which brings tenure, funding, and fame."

In creating standards like the Transparency and Openness guidelines and securing their adoption by scholarly organizations like major journals, COS aims to not just convince researchers that their instruments, platforms, and metrics are good ones. Instead, by using the power of incentivization to create rewards for conducting scholarly work in ways that align with the organization's definition of "openness, integrity, and reproducibility of research" and penalties for conducting research in ways that do not, the organization's strategy seeks to build a scholarly publishing infrastructure that "embodies," to use Star and Ruhleder's term, COS's vision ([Center for Open Science, n.d.](#)). But these advocates describe their work as aiming beyond the academy. "We became philanthropists to change the world," Laura Arnold's TedX talk went on. If philanthropists can and should change the world, Arnold suggests, they can do so in part by changing the relationship of governance to knowledge. The Arnolds are important advocates for using randomized controlled trials (RCTs) to direct public policy, and they advocate against funding programs that are not supported by what Laura Arnold called this "gold standard" of evidence. "We're spending millions and millions of dollars on programs that at worst do not work, and at best, we do not know, or we do not have sufficient data or we do not have reliable data. This needs to change, we need to stop." The foundation's investments in the Center for Open Science and its work to build standards and platforms for data-sharing, Laura Arnold said, were her Foundation's attempt to not only shift what she called the broken incentive structure of science, but also to make "healthy data systems" that could be used in making evidence for policy-makers to "follow." "We try to break this cycle and reform the system," Arnold continued, "by funding organizations that are promoting transparency and good practices and collaboration and data-sharing."

What does open science have to do with the ways that knowledge is classified or recognized as good enough to govern with? By tracing the public trajectories of open science projects like transparency standards that aim to classify some research as reliable or credible enough to be used to be used in policy-making and other research as illegitimate or spurious, ethnographers can observe important contemporary contests over epistemic legitimacy in politics—and ask key questions about how the politics of philanthropy shape public uses of science and research. For instance, ethnographers—alongside other researchers, such as political scientists and comparative-historical sociologists—might analyze the success of efforts like the Arnolds' to define some knowledge as trustworthy enough to be used in policy-making on the basis of particular data-sharing standards. Is a crisis of epistemic legitimacy actually arising from these calls, and if so, what are its terms? How do politicians, regulators, or social movements actually relate to evidence-based policy that cites openness of research as a signal of trustworthiness and reliability?

³ Together, these three sources account for \$4,693,168, or over 85%, of the organization's \$5,468,200 budget—and the Templeton foundations and Arnolds for over 50% ([COS Form, 2020](#)). The Laura and John Arnold Foundation was the largest funder, contributing \$2,289,280; DARPA accounted for \$1,887,072; the John Templeton Foundation donated \$170,561, the Templeton Religious Trust \$163,352, and the Templeton World Charity Foundation \$182,903. The charities Templeton founded prioritize projects concerning religion and science, including some that have been the subject of some controversy in fields like biology and religious studies (see, for instance, [Wiebe, 2009](#); [Waldrop, 2011](#)), as well as projects advancing free markets, such as prominent right-wing think tanks like the Cato Institute and Mercatus Center (see Templeton Foundation and Templeton World Charities; [Grant Database, n.d.](#)). Laura and John Arnold, meanwhile, are Houston billionaires—John, profiled in the book on which the Oscar-winning documentary *The Smartest Guys in the Room* was based, is a former executive at Enron. After Enron's collapse, he went on to found the hedge fund Centaurus Advisors ([Taibbi, 2013](#)). The Arnolds have joined Warren Buffet and Bill Gates in pledging to donate the majority of their wealth ([Laura and John Arnold. The Giving Pledge, n.d.](#)).

When, where, and how is the mantle of evidence-based policy based in open data taken up? When, and why, are these standards taken up or not taken up in the process of making law or funding public programs? In the day-to-day work of legislatures, regulatory bodies, city council and school board meetings, when is data-sharing invoked as a sign of credible research—and when is data unavailability invoked as a sign of doubtful legitimacy? Have philanthropic efforts to formalize open science standards and promote open science actually produced a situation in which science or research that does not share or selectively shares data is treated by policy-makers as an unreliable basis for governance? How, exactly, has the flow of foundation funding to standards for research conduct shaped what is actually treated by legislators or regulators as legitimate or illegitimate knowledge, or as good or bad evidence of effective policy-making?

Echoing studies of the evidence-based policy-making to which calls for data-sharing are related, ethnographers might also ask what *kinds* of policy tend to result from using standards for data-sharing to distinguish reliable from unreliable knowledge. A central criticism of policy-making guided by RCTs, for instance, is that it is a technocratic strategy of focusing on small questions, problems, and solutions, and that it tends to lead to less transformative outcomes. Arnold Ventures, the limited-liability corporation the Arnolds created in 2019 to more efficiently accomplish their political goals by combining their Foundation, their donor-advised fund, and their political advocacy group Action Now (see [Schultz, 2019](#)), has often explicitly framed their interventions in this way: as intended to avoid more transformative reforms by tinkering with existing structures ([Jeffries and Ridgely, 2020](#)). For instance, in the wake of public criticism of the US's system of policing and incarceration, Arnold Ventures similarly described the research it funds as serving to identify inefficiencies and harms that could be reformed while retaining the existing criminal legal system, which the organization described as offering “real public safety benefits” ([Fontaine, 2022](#)). If policy-making guided by RCTs tends to result in “formal, not transformative reforms” ([Gilmore and Gilmore, 2022, 316](#)), are there patterns in what tends to be decided or enacted when data-sharing standards are used to validate knowledge for governance?

Ethnographers of open science who follow standards for adequate public sharing of research materials into policy and politics might also compare the negotiations that surround these standards to those that surround other related instruments. For instance, in addition to spearheading creation of the Transparency and Openness guidelines, John Arnold has been a key supporter of pretrial reform campaigns that rely on algorithmic risk assessment tools (RATs) and Arnold Ventures is a key RAT creator. Their Public Safety Assessment, offered free of charge, is used statewide in four jurisdictions and multiple municipalities. These tools have been criticized by a host of community groups, from the NAACP and ACLU to Mijente and National Bail Out, and also in a 2019 consensus statement by 27 researchers at MIT, Harvard, Princeton, NYU, the University of California-Berkeley, and Columbia, who characterized them as racially biased by nature ([Whitlock and Heitzeg, 2021, 113–118](#)). Technologies and tools like these do not necessarily ensure equity in decision-making, often instead encoding—and then reproducing in the information they produce—both racial biases and naturalized definitions of racial difference (see [Braun, 2014](#); [Noble, 2018](#); [Moran-Thomas, 2020](#); [Liao and Carbonell, 2023](#)). RAT campaigns,

meanwhile, compete with others such as money bail reform mandating pretrial release that are supported by broad-based community groups. There is rich action to observe when advocates create standards, metrics, and instruments, then encourage consequential institutions to make them a part of the infrastructures through which they function. Ethnographers of open science might compare data-sharing standards to other objects like RATs, asking what is naturalized in their use, and following whether, how, and why their paths are marked by similar conflict and contest—and whether and why data-sharing standards are more seamlessly accepted.

Lastly, ethnographers and other scholars of open science have another chance to investigate how, to paraphrase Shapin and Shaffer, arrangements of knowledge in open science are related to arrangements of power: we can respond to and expand upon existing scholarship about the relationship of open science to democracy. Scholars of open science often note that it is frequently characterized as a democratic good ([Okune et al., 2022, 2](#)). Proponents in what Benedikt Fecher and Sascha Friesike call the “democratic school” hope that open science might democratize research by making its products—both data and publications—more widely available through open data and open access publishing ([Fecher and Friesike, 2014, 27–32](#)). This might address problems of equity, as the instructors of my Center for Open Science-developed workshop suggested: even without access to the deep pockets of elite institutions in the Global North, scientists and scholars could learn from the methodological innovations of published papers in their fields that might otherwise be paywalled, and researchers could analyze data—including ethnographic data—without incurring the costs of its collection. Others hope this might happen through “citizen science,” whereby laypeople and noncredentialed members of the public might participate in the research process, and suggest that this participation might yield “radical change to the structures of political power” ([Cavalier and Kennedy, 2016, 117](#); see [Mirowski, 2018](#)). Those in what Fecher and Friesike call the “public” school of open science see the openness of these sorts of citizen science projects—along with the horizontality of science communication projects—as dismantling scientific elitism, serving as a “form of devotion” to a broader audience for research.

In practice, open science projects relate to matters of democracy in research—distributions of decision-making power and resources in the scene of data-collection, data-sharing, and data-use—in ways that are more varied and more contradictory than those imagined by either the “democratic” or “public” schools. We have seen how differently decision-making is assigned in open science initiatives in which regulators mandate data sharing and in open science projects that build shared vocabularies or processes for data negotiations between communities and researchers ([Okune et al., 2018, 8](#)). Meanwhile, critics have noted that many citizen science projects simply reproduce hierarchical and even exploitative arrangements of knowledge production, with credentialed, career scientists making analytic decisions while laypeople are delegated often unpaid, intermittent tasks ([Powell and Colin, 2009](#); [Fecher and Friesike, 2014, 23](#)), and have suggested that if this is a model of democracy, it is a thin one indeed ([Mirowski, 2018, 177](#)). The assertion that open science is a democratic good might, of course, mean a number of different things, depending on whether we understand the word democratic to denote, on the one hand, that something relates or is available to a broad mass of people, or on the other, that it is characterized by decision-making or

governance by a broad mass of ordinary people (Merriam-Webster, 2023; Brittanica, 2023). Critiques that highlight the question of who makes decisions in citizen or open science projects argue that these research arrangements can privilege the first of those definitions over the second: they might make data, research materials, or the activity of data-collection *available* to a broader mass of people, but they do not always turn the *decision-making* about research over to laypeople. These critics suggest that if we want science to become more democratic, we ought to mean something more significant by democracy than mere access to research materials or participation in any stage of the research process.

If we keep in mind the ways that sharing research materials figures in visions of evidence-based policy-making, however, we can recognize further questions about the relationship between open science and democracy. We can ask about how classifying some knowledge as legitimate and other knowledge as illegitimate on the basis of data-sharing standards is related to the question of who does public decision-making, and to the question of whose voices, accounts, and priorities can shape that process. One approach to this question might begin with the relationship between open science funding and broader philanthropic program in which it arises. While both major private funders of the Center for Open Science are deeply invested in a wide variety of other issues—from the Arnolds' grants supporting abortion access (Arnold Ventures; Abortion Care Network, 2023) and emergency Head Start funding during the Republican-led 2013 federal government shutdown (CNN, 2013), to Templeton's foundations' investments in positive psychology and neuroscience (Templeton Foundation, n.d.; Positive Neuroscience Archives, 2023)—Arnold Ventures and Templeton's foundations' investments share another area of overlap. Both have invested deeply in projects steering policy-making away from directions suggested by grassroots organizing, community decision-making, or collective action. Templeton's foundations, which are one of the chief funders of think tanks and advocacy and trade organizations promoting climate change denial (Brulle, 2014), made one of their largest grants to the Atlas Economic Research Foundation. AERF, a self-described “worldwide freedom movement” also funded by the Koch brothers and oil companies such as ExxonMobil, has been credited with convincing Canadian state officials to limit Indigenous communities' ability to prevent oil pipelines and fracking operations by refusing to sign on to a United Nations declaration ensuring Indigenous peoples' right to consent to fossil fuel extraction on their lands (Dembicki, 2022). Arnold Ventures' political work, meanwhile, has also taken direct and targeted aim at the power of public employee and teachers' unions, advancing school privatization through charter advocacy (Support Our Public Schools, n.d.; Ballotpedia) and “dominating” the funding of efforts to slash public employee's pension benefits in favor of private investment in riskier assets like hedge funds (Frying Pan News, 2013). Recalling the second definition of what it might mean for something to be democratic, we can investigate the relationship between visions of good policy-making that use data openness as a marker of legitimacy and the extent to which governance is done by broad mass of ordinary people. If and when the openness of research or information is indeed invoked as a marker of epistemic legitimacy in policy-making in the ways suggested by the projects these funders support, are there patterns in how these appeals bolster or erode the degree to which governance unfolds

democratically, or on the basis of collective action by ordinary people? Do open science standards for good knowledge in policy-making change or limit the voices or perspectives admitted to the process of public decision-making? Is the power of ordinary people to use observational and experiential accounts of their lives and lands to shape what happens in their jobs or their homes increased or decreased by standards for data-sharing in policy and politics?

As sociological analysts of philanthropy have long noted, heterogeneity in funding and incongruity between funded projects and philanthropists' own political identifications and agendas are both common (see Karl and Katz, 1987). Questions of how to define open science and which research practices are proper are, however, closely related to questions of governance. Who should make decisions not just about what happens in the course of research, but about what happens in the world? What kinds of information and knowledge should guide those decisions? And what is the right way to do the work of making that knowledge? Through their close relationship to standards for legitimate policy-making, standards of legitimacy and facticity “embodied” in key open science infrastructures link the problem of knowledge to the problem of order. These links deserve our ethnographic and critical attention.

In the laboratory of dispute: toward ethnographies of facticity

In moments of disagreement about scientific inquiry or change to the practices and ideals by which science is done, we have unique access to the ways that actors ask and answer questions about how science *should* be done. As calls for and practices of open science become more widespread and more deeply institutionalized across disciplines, ethnographers, I have suggested, should study open science with an eye to the way that it is a site of debate, struggle and negotiation over epistemic virtues in contemporary knowledge and governance. I have argued that ethnography benefits by following STS and ethnographers of science and knowledge in treating the open science moment of change and dispute regarding both scientific practice and epistemic virtue as a kind of natural laboratory.

Practices of open science like public data storage are often suggested as tactics that can shore up the facticity of research, including ethnographic work. I have argued that ethnographers can treat open science calls and practices not just as chances to prove that we are not lying, but as provocations to treat facticity as an object of our study, and we can treat the sharing of research materials as not only a strategy to adopt, but a knowledge practice to study. Indeed, the profusion of available data, instruments, research designs, code, and more across differing platforms and audiences that has resulted from open science policies and practices offers itself for ethnographic analysis, including “code ethnography” (Rosa, 2022), policy ethnography, ethnography of documentation, and ethnography of infrastructure (Star, 1999): open science, in other words, may be a great gift to ethnographers of science. By treating calls and requirements for open science, data-sharing, and transparency as not just methodological, but also empirical matters, we gain new questions for analysis, new field sites, and opportunities to revisit and reexamine core questions in the ethnographic study of science.

This article has suggested a wide array of such questions that ethnographers might pursue if we treat the emergence, embrace, and negotiation that surround calls for open science as a fruitful site of

disruption from which to track contemporary contests over epistemic virtue. These questions are uniquely suited to ethnographic methods, focused as they are on not only what important actors in open science debates say—how scientists describe their own understandings of open science, or how funders’ characterize the merits of data-sharing or their own goals in promoting open science—but on what these actors do in the course of their day-to-day work. I have suggested, in other words, that we build on interview- or discourse analysis-based research about open science by observing processes, practices, and creative and intended uses of open science instruments and infrastructures, analyzing action by both those who understand themselves to be part of an open science movement and those who open science advocates hope might act on calls to recognize good knowledge on the basis of data-sharing.

Similarly, studying open science with an eye to its indeterminacy in this way can take ethnographers to a broad range of *field sites*. Certainly the sites these questions have suggested—those where researchers, journal editors, legislators and regulators gather to make and use knowledge—are ones where we might find rich action to observe. But in addition to philanthropists’ meetings, policy-makers’ gatherings, and researchers’ laboratories, conferences, and trainings, ethnographers of open science might visit other sites. If, following STS inquiries into the creative shaping of technologies by users (see, for instance, Oudshoorn and Pinch, 2003), ethnographers of open science might examine the ways that users actually interact with technologies from COS’s Open Science Badges and platforms like Dataverses and Open Science Framework to scoring and verification tools like SciScore or penelope.ai, we might also turn our attention to the software developers and engineers who build these platforms. The advent of open science also offers an opportunity for ethnographers of science to revisit one of its central questions: that of inscription. How have new epistemic virtues, data-sharing practices and open science mandates changed—or not changed—the ways laboratory scientists engage in “coding, marking, altering, correcting, reading, and writing” (Latour and Woolgar, 1979, 49), not to mention image-making (Daston and Galison, 2007; Vertesi, 2015)? Organizational ethnographers can treat institutions that interact with the open science movement—from IRBs to data repositories and libraries, think tanks like COS to state agencies like NIH or NSF—as field sites from which to observe the negotiation of a new set of normative understandings, platforms, tools, and policies, embracing the profusion of social and cultural action that occurs in these sites as worthy of our ethnographic curiosity. Studies like these will be chances for ethnographers to ask a broader range of questions about how the politics and practices of truth and facticity are shifting across a range of communities of knowers and social institutions in the wake of calls for open science.

If ethnography is good for the study of open science, studying open science is also good for ethnography. Studying open science can help ethnographers do three things: first, it can help us to think about the status of our own methods. Second, studying the process of open science can help inform ethnographers’ own decisions about disclosure of research materials. Third, the lines of inquiry opened by studying open science can point ethnographers toward a more general reinvigoration of studies of norms, values, and understandings of facticity in both politics and science.

Ethnographers considering calls to share research materials as demonstrations of transparency have often pointed out that ethnographic research by nature permits less of what Victoria Reyes

calls “one-size-fits-all” approaches to disclosure. At times, as Reyes writes, “the very reason researchers and participants were able to talk about a certain subject was because of the promise of confidentiality” (Schultz, 2019, 188). Reyes suggests that ethnographic transparency might look less like release of primary data by default and might look more like transparency about the process of data analysis, as well as transparency about the decisions involved in whether and how particular people and places are identified and deidentified (Reyes, 2018). Yet as funders, publishers, and disciplinary associations increasingly specifically require data-sharing practices associated with open science as markers of transparency, ethnographers who decline to deploy those practices and interpret transparency otherwise may increasingly hazard disciplinary marginalization (Murphy et al., 2021, 42). Studying open science ethnographically can certainly help us understand these tradeoffs. But recalling how open science advocates’ definitions of legitimacy and epistemic virtue on the basis of particular practices of disclosure speak to both other researchers and to policy-makers, we can see how studying open science might help us to think about the political status of our work. If ethnographers decline, for instance, to share field notes, do we risk not only a loss of legitimacy among other scholars, but also reduced opportunities for our research to influence public decision-making? Better understandings of just how mandates and standards for data-sharing are being used in scientific and policy-making spaces can help us to consider these questions for our own work.

By observing the ways that conversations about open science are also conversations about how to recognize credibility or facticity, we can gain crucial perspective on the relationships between open science visions of knowledge and their visions of order. This perspective can also inform our methodological decision-making in essential ways, allowing us to make well-informed choices about how to set standards for transparency that remain faithful to the commitments to disrupt dynamics of exploitation and domination that emerged from ethnography’s first reckoning. If we are attuned to the relationship between the matter of who decides what makes for credible knowledge and the matter of who decides what makes for a good social policy, we can build ethnographic practices of revelation and concealment that build, rather than disrupt, the power—as both knowers and political actors—of the communities where our research occurs. If we want more democratic knowledge-making to emerge from ethnography’s reckoning with open science, we must consider these matters.

Looking to our own methods can also support ethnographers in generating principles of disclosure that enable accountability, empirical precision and contextualization, fruitful reanalysis, methodological training and innovation, and clarity of inference (Pool, 2017; Reyes, 2018, 208–13, 17)—and that are informed by the rich tradition of ethnographic studies of knowledge and proactively support the power of the communities where our research unfolds as knowers and actors. We can learn from examples of open science and data sharing projects in methodological and disciplinary locations like digital anthropology, environmental justice and environmental health research, STS, Indigenous studies, and decolonial science studies. These might include experiments like those of the Platform for Experimental Collaborative Ethnography and STS Infrastructures, an instance of PECE, that work to treat data as “entangled in systems of relations” and to “acknowledge the ways power is woven into language, common sense, and communicative practice” (Fortun et al., 2016, 18; Okune et al., 2022, 7). Inviting varied ways to contextualize data and building

“interpretive annotation” into a recursive process of sharing data—and, through researcher reflection, making more—data, these platforms aim to encode ethnographic sensibilities in data-sharing infrastructures (Fortun et al., 2016, 16; Okune et al., 2022, 3–5).

They might also include practices of “strongly participatory” science: research processes that aim to enhance what Barbara Allen calls knowledge justice by bolstering laypeople’s power to make claims and advance their visions for their own communities through their participation in scientific work (see Visvanathan, 2005; McCormick, 2009; Allen, 2018). We might also learn from the research methods and data governance models developed by the “civic science” Public Lab for Open Technology and Science, which formed in the wake of the BP oil spill using a model of local chapters. PLOTS has developed what co-founder Shannon Dosemagen and her coauthors call “situated” data-production and data-sharing practices, taking moments in which powerful actors such as police indicate interest in their work as cues that “recursive” refinement of these practices is necessary to reemphasize “the need of vulnerable communities to maintain control over representations of their territory” (Dosemagen et al., 2011). We can learn from these models, such as data trusts in which data are stewarded by a council whose role is defined by those data’s beneficiaries (Dosemagen and Tyson, 2020). Max Liboiron, founder of the Civic Laboratory for Environmental Action Research, writes of this as a question of “tak[ing] up science that enacts good Land relations” (Liboiron, 2021, 22). In CLEAR’s marine science and pollution research, this means that Indigenous groups not only make the invitations that begin any given research project and set the priorities and research questions to be investigated, but also own project data (Liboiron, 2016). Meanwhile, at the Environmental Justice Lab directed by STS scholar Michelle Murphy, the organization of data storage and translation in a pollution reporting application is determined according to frontline community useability (see Pollution Reporter, n.d.).

Ethnography’s reckoning with data collection and data-sharing, as Murphy, Jerolmack and Smith have written, both comes on the heels of and overlaps with another, “first reckoning”: the conversations about power, ethics, reflexivity, and representation that began in the 1980s and 1990s and continue today (Murphy, Jerolmack and Smith, 42–3). Through calls among ethnographers to democratize or horizontalize knowledge production by reversing power dynamics between researchers and non-academics, this earlier reckoning resulted in disciplinary standards requiring that ethnographers robustly consider the ways their own subjectivities and structural positions might influence the knowledge they produce. It also pushed ethnographers to question the notion that highly-trained outsiders should be treated as more authoritative knowers than members of communities themselves. In deciding how research practices are chosen, how data are arranged and stored, and who will “own” data or decide about its use, all of these models begin from epistemic virtues that are informed by ethnographic study of scientific practice and recall the insights of ethnography’s “first reckoning.” These models presume that the “the best research done in the best way,” to quote my open science workshop’s instructors, might not be characterized by the “default” assumption of “direct access” to all research materials for which the Center for Open Science’s mission statement calls. Instead, they situate the question of when and how to share data as one decision among many, all of which ought to be guided by awareness of the ways that claims to objectivity in science have been weaponized in service of maintaining violent arrangements, and by awareness of the impossibility of scrubbing the social from the scientific.

We also gain a different fundamental approach to facticity when we do not just try to achieve it, but treat it as an object of analysis. Contests over truth and credibility are, of course, a highly contentious and intensely politicized matter in our current moment, and there is a great deal of action for ethnographers to observe beyond the laboratory, the academy, or the scientific realm. Our methods are well-suited to investigating the cultural purchase, legal codification, and community negotiation of highly-politicized misinformation and disinformation about matters like abortion, electoral fraud, COVID-19, trans healthcare, or the post-2020 crime panic. Importantly, they are also well-suited to investigating the ways that policy-making and political debate about these matters raise questions for a wide array of actors about what makes for knowledge that is good enough to use in governance. Ethnographic studies can play an important role in giving us better analytic purchase on our current epistemic moment. If we treat controversies over the right way to know as moments of analytic opportunity and ask what is shifting about epistemic virtue and the ways that facticity is recognized, demonstrated, and contested, we gain opportunities to make ethnographic contributions of urgent importance.

Author contributions

The author confirms being the sole contributor of this work and has approved it for publication.

Funding

Open access publication of this article was supported by Virginia Tech’s Open Access Subvention Fund.

Acknowledgments

I am grateful for the excellent library team at Virginia Tech, who sparked the questions that fueled this article, for the reviewers’ and editors’ generative suggestions, for conversations with Lena Eckert-Erdheim, for Amanda Cheong’s extremely helpful comments, and for feedback from Disha Jani, Gözde Guran, Sam Jaroszewski, Kalyani Monteiro Jayasankar, and Leah Reisman.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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RECEIVED 05 October 2023

ACCEPTED 20 February 2024

PUBLISHED 01 March 2024

CITATION

Khan S, Hirsch JS and Zeltzer-Zubida O (2024)
A dataset without a code book: ethnography
and open science.
Front. Sociol. 9:1308029.
doi: 10.3389/fsoc.2024.1308029

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A dataset without a code book: ethnography and open science

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This paper reflects upon calls for “open data” in ethnography, drawing on our experiences doing research on sexual violence. The core claim of this paper is not that open data is undesirable; it is that there is a lot we must know before we presume its benefits apply to ethnographic research. The epistemic and ontological foundation of open data is grounded in a logic that is not always consistent with that of ethnographic practice. We begin by identifying three logics of open data—epistemic, political-economic, and regulatory—which each address a perceived problem with knowledge production and point to open science as the solution. We then evaluate these logics in the context of the practice of ethnographic research. Claims that open data would improve data quality are, in our assessment, potentially reversed: in our own ethnographic work, open data practices would likely have compromised our data quality. And protecting subject identities would have meant creating accessible data that would not allow for replication. For ethnographic work, open data would be like having the data set without the codebook. Before we adopt open data to improve the quality of science, we need to answer a series of questions about what open data does to data quality. Rather than blindly make a normative commitment to a principle, we need empirical work on the impact of such practices – work which must be done with respect to the different epistemic cultures’ modes of inquiry. Ethnographers, as well as the institutions that fund and regulate ethnographic research, should only embrace open data after the subject has been researched and evaluated within our own epistemic community.

KEYWORDS

ethnography, open science, epistemology, qualitative method, methodology

Introduction

Open science has reached ethnography. Yet the problems open science identifies and the solutions open science proposes are grounded in the epistemic logic of particular (i.e., non-ethnographic) scholarly communities. Importing these problems and solutions to other epistemic communities requires consideration of what might be lost in translation. Before ethnographers embrace open science, we must ask, “do the asserted benefits of open science—improved data quality, data reliability, and increased trust in the scholarly enterprise—hold true for ethnographic research?” These normative-laden claims are, as of yet, entirely empirically unsubstantiated within ethnographic practice. In fact, the evidential base for the open science movement in general is relatively weak. Moreover, certain epistemic foundations of ethnographic research—positionality and normativity—are consequential for but not reckoned with in the open science movement.

In this paper we take on one small part of the open science movement: the question of data availability, or what we will call “open data.” Drawing on the work of Karin Knorr Centina, we argue that as we consider importing open data practices to ethnography,

we need recognize ethnography's "epistemic culture." Knorr Centina defines epistemic cultures as "those amalgams of arrangements and mechanisms—bonded through affinity, necessity, and historical coincidence—which, in a given field, make up *how we know what we know*. Epistemic cultures are cultures that create and warrant knowledge" (1999:1). Whereas some view science as "universal," (which Knorr Centina calls the "monist" position), with a singular approach to methods, logic of inquiry, a shared understanding of reasoning, and a specific approach to the relationship between theory and data, the idea of "epistemic cultures" suggests that different fields of scientific inquiry can vary quite considerably in how they understand the logic of science. Knorr Centina developed her argument with respect to two natural science fields: high energy physics and molecular biology. In this paper we extend her insights to the social sciences, considering how the epistemic culture of ethnography—which is to say how it thinks about method, logic of inquiry, reason, and the relationship between theory and data—is distinct from other epistemic cultures. This different does not necessarily make ethnography "less scientific" but instead, "differently scientific."

One could think of the open data movement as a colonizing force, subsuming the cultural logics of different scientific communities under its normative demands, demands which are grounded in the presumption of a singular, or monist scientific approach. In bringing "backward communities" into the light, it promises that the quality and respectability of knowledge will increase, and so too will its power. Yet the disunity of scientific communities—as Knorr-Centina describes—means that such a universalizing impulse is likely both to be met with resistance from those whose primary method is ethnography and to yield distinct outcomes within different epistemic cultures. Not only may some open science practices (and their intended outcomes) get lost in translation, but some epistemic communities may also find themselves deemed "illegitimate" should they fail to comply with a set of external demands. These concerns are recognized by advocates of the broader open science movement. In their review of replication in social science, Jeremy Freese and David Paterson note that open science practices are likely to be adopted in different ways within different epistemic cultures. "The role of replication within a field ought to be understood as the outcome of a process of cultural development which is influenced by both internal dynamics and external pressures rather than a universal feature of an idealized scientific method" (2017: 151). The external pressures they speak of, however, also suggest that for a form of inquiry to be considered a "science" it is increasingly conditional on its embrace of open science principles. Those powerful epistemic cultures that have embraced and constituted the logic of open science are likely to suggest that reluctance of other communities to conform to their favored logic implies they are not truly "scientific." Such work to determine legitimacy of knowledge has long been recognized as a kind of power politics. The colonizing advance is one that places certain epistemic approaches above others in their legitimacy.

Describing the epistemic culture of ethnography is not without its own perils. Ethnographers reside in a methodologically contentious corner of sociology; indeed, in recent years perhaps the most vigorous and impassioned recent debates within the field of sociology have been among ethnographers (see Duneier, 2002, 2006; Wacquant, 2002; Klinenberg, 2006; Jerolmack and Khan, 2014a,b).

Even defining the ethnographic community itself is a challenge. We conceptualize ethnography—which includes both participant observation and in-depth interviews—as a relational-interactive method. A necessary condition for data gathering is that a researcher personally interacts with and enters a relationship with the research subjects (this can include digital ethnographies, where interactions happen within online spaces). Because of these interactive and relational components, the ethnographic community tends to rest upon two epistemic foundations: reflexivity and positionality. Positionality "reflects the position that the researcher has chosen to adopt within a given research study" (Savin-Baden and Major, 2023: 71). It also encompasses the position that is relationally constructed by research subjects themselves, and grounded in the attributes of the researcher (e.g., race, gender, sexuality, class, ability, etc.). The concept of positionality is, in part, grounded in the Black feminist perspective which argues that who we are and how we interact with the world shapes what we find (Collins, 1986, 1999, 2000; see also the perspective in Smith, 1989). Synthesizing this and other insights, Homes argues (Holmes, 2020),

The term positionality both describes an individual's world view and the position they adopt about a research task and its social and political context. The individual's world view or 'where the researcher is coming from' concerns ontological assumptions (an individual's beliefs about the nature of social reality and what is knowable about the world), epistemological assumptions (an individual's beliefs about the nature of knowledge) and assumptions about human nature and agency (individual's assumptions about the way we interact with our environment and relate to it) (2020: 1).

While such positionality is not unique to ethnographic work, it is more acute because of the relational-interactive necessity of gathering data in interview and participant observation contexts. This reflects an epistemic culture wherein Haraway's (1991) classic arguments about the inherently situated nature of knowledge construction (and the inevitability of a partial perspective) are more fully embraced than they are in other scientific communities. From this perspective the question is not about how to minimize the impacts of positionality in order to guide us closer to objective observation, but instead is about how to establish "an agenda for the assessment of subjectivity" (Malterud, 2001: 484). Reflexivity is a central part of the agenda. As Malterud continues,

Reflexivity starts by identifying preconceptions brought into the project by the researcher, representing previous personal and professional experiences, pre-study beliefs about how things are and what is to be investigated, motivation and qualifications for exploration of the field, and perspectives and theoretical foundations related to education and interests. (2001: 484)

Such a reflexive epistemic approach starkly contrasts that of the more positivist open data movement. And it is of particular importance because successful replication (a core demand of the open science movement) requires the capacity to fully convey the observer's position in the field and subjectivity as a person. That prospect is necessarily incomplete, thereby undermining the capacity of certain forms of replication.

We do not oppose the application of some open data principles to ethnographic research,¹ but we do provide some words of caution based on our own research experiences. We suggest that open science principles should not be directly *adopted* by any epistemic community; instead, we suggest they may be *adapted* to recognize and reflect diverse epistemic cultures. Such adaptation requires empirical study in addition to good arguments, and empirical study has been curiously absent from those who advocate for open science principles. We argue, for example, that making our own research data broadly accessible would have interfered with our data gathering, lowered the quality of our data, and decreased the reliability of our findings. Yet other forms of data verification (e.g., hiring someone to independently check the claims in our book (Hirsch and Khan, 2020) against our fieldnotes and interview transcripts) likely increased readers' trust in the reliability of our findings and arguments.

Our argument proceeds in three parts. First, we review the literature on open data, outlining the problems with scientific knowledge production and dissemination that open science purports to address. We also summarize the subsequent proposed solutions to these problems. We construct three logics that scholars have converged upon: epistemological, political-economic, and regulatory. We recognize the value of these arguments but note that they have not been grounded in the ethnographic enterprise nor do they recognize the epistemic culture of the ethnographic community, nor have they been rigorously studied. As might be anticipated, we highlight the first diagnosis—epistemic—as the most acute. But we also note how the distinct culture of ethnographic scientific production creates challenges for the political-economic and regulatory justifications for open data.

Second, we use examples from our own ethnographic research on sexual violence (Hirsch and Khan, 2020) to provide grounding and texture as we evaluate the implications of open data for ethnographers. We conclude by reflecting on the next steps we think the scholarly community should take to address the concerns we outline. Specifically, we argue for the importance of empirically evaluating the impact of open data and considering its implications for the specific epistemic culture of ethnographic work.

Three justifications for open data in sciences

Open science is a broad movement that considers all aspects of the process of scientific production. This includes, but is not limited to, access to how data are generated (i.e., availability of research instruments), access to raw data, transparency of coding decisions/analysis and material/replication packages, and public access to scholarly outputs. In this paper we primarily focus on the second in that list of elements of the open science movement: what we refer to as “open data.” Open data focuses on the availability of the various empirical materials produced during research: the “raw data” that will subsequently be analyzed. We are highly supportive of other parts of

the open science movement, such as the availability of research instruments, papers, and coding/analysis schemas.

We provide an abbreviated account of the literature on open data. We do not limit our focus to qualitative research or even social scientific research. Instead, we look at how a specific set of “problems of knowledge creation” are diagnosed across a range of disciplines. Scholars are not so naïve to propose open data as the *sole* solution to the problems they outline. We do not review the broader range of suggestions, beyond open data, in this paper. We also do not interrogate what scholars describe as the problem of knowledge creation and dissemination in-and-of themselves. We would note, however, that in our assessment, all the problems that scholars have identified are “real” and it is reasonable to be concerned about them. We would even agree that open data is, for many of them, a reasonable step toward a solution. Our argument is that the diversity of epistemic cultures and scientific communities warrants problem-framings and solutions that are sensitive to these contexts; open data faces challenges when uniformly applied. We also believe that the open science movement must encompass empirical work to evaluating its own claims—something that is curiously underdeveloped from such a “scientific” movement. Arguments for open data give three principal justifications: *epistemological*, *political-economic*, and *regulatory*. Table 1 provides a summary of the problems the open science movement has identified, these three justifications for the open science movement, and the proposed logic of the solution.

The first problem scholars identify is about *reliability*. Within certain epistemic cultures the ideal form of knowledge production is one where observations are independently and uniquely re-observed to establish that a measure is reliably constructed, the techniques of observation are accurately described and executed, the analysis is reproducible, and findings can be replicated. In practice, this is often impractical – and sometimes impossible. Rare episodes can be enormously difficult to re-observe; when dealing with human subjects, temporal constraints can make re-observation impossible. This suggests two kinds of replication: that of the study, and that of the analysis. In a review of the state of replication norms in sociology, Freese and Peterson offer a two-by-two matrix of replication practices in quantitative social science. The axes they identify are *similar/different*, and *old data/new data* (2017). They designate the two practices that work with existing data *Verifiability* (similar), direct reproduction of results with the same data and code, and *Robustness* (different), reexamination of the same data under different specifications and analyses. While the *new data* axis is the gold standard (i.e., testing for *Repeatability* (similar) or *Generalizability* (different)), openly accessible existing data provides the next best solution to empirical reliability.

It is also the form most likely to be used. In an editorial for *PLOS Computational Biology* laying out best practices for data-sharing and management, Goodman et al. (2014) note that “the amount of real data and data description in modern publications is almost never sufficient to repeat or even statistically verify a study being presented.” When data are presented to their audience, the full picture is rarely sufficient for critical examination of the findings. Operationalization decisions are blacked boxed. Potential negative cases are not all systematically presented. Making data accessible allows for the replication, reproduction, and validation of published results. Scholars can evaluate how raw data are operationalized into values. Cases that “do not fit” can more clearly be seen and evaluated by a community of

¹ We have strongly advocated for instrument sharing, which can allow for a kind of replication (Hirsch et al., 2010; Hirsch and Khan, 2020); in this piece we are principally concerned, however, with data accessibility.

TABLE 1 The problems of knowledge production and dissemination and the logic of the open data solution.

Problem	Justification for open science	Logic of solution
<p><i>Reliability</i></p> <p>Scholars make a range of decisions about how they gather and analyze data: how concepts are operationalized, what observations are categorized, what counterevidence is mobilized or ignored, etc. These decisions are often “black boxed,” thereby undermining reliability of data analysis.</p>	<p><i>Epistemological</i></p>	<p>Empirical observations are independently and uniquely re-observed, operationalization is systemically evaluated, and negative cases are re-explored to establish that measures and variables/relationships are reliably constructed. Claims are thereby re-established, and findings can be replicated. Alternatively, and typically, the same data is re-analyzed.</p>
<p><i>Equity & Efficiency</i></p> <p>Data is enormously expensive to collect, and it would be highly inequitable and undesirable to allow data to remain solely in the possession of those who could afford to collect it (and gain advantage from privileged access). The public often pays for research through government programs, but often does not have access to the information it funds.</p>	<p><i>Political-economic</i></p>	<p>Making data widely available addresses inequity by giving more people to access information. Various scholars working off the same data allows for a diversity of perspectives, wherein scholars can make inquiries that others may not have considered or may not have had the time to undertake. In addition, there is a normative claim that publicly funded data should be publicly available.</p>
<p><i>Fraud</i></p> <p>The stakes for scholars are extremely high. There are strong incentives to fabricate work to advance one’s career an argument the scholar has an interest in. Discovering fraud is extremely difficult.</p>	<p><i>Regulatory</i></p>	<p>Data accessibility increases the likelihood of discovery, thus discouraging fraud.</p>

scholars. Nosek et al. (2015) make the case in *Science* for journals to adopt author guidelines commensurate with open science ideals, arguing that “to progress, science needs both innovation and self-correction; replication offers opportunities for self-correction to more efficiently identify promising research directions.” This provides an epistemic justification for open-science principles.

The second problem scholars identify within the scientific community is about *equity and efficiency*. Across the natural and human sciences data can be enormously expensive to collect. This concern may have increased as “big data,” or omnibus data, have garnered more and more symbolic value. In response to such expense, scholars have expressed concern about equity. If data remains solely in the possession of those who can afford to collect it, then scholarly inequalities may be aggravated as those few scholars who can secure funding for large-scale data projects would be able to extract considerable rents from their privileged access. In addition to threats to equity, there is concern that knowledge itself might suffer as limiting who can access large data sets will narrow insights, either because it would constrain which problems are considered to be of interest or because having just one person work on a dataset would severely hinder the amount of work that could be done on those data. Finally, and perhaps most powerfully, given that research funding often comes at taxpayer expense, the public, broadly conceived, should have access to the data they paid for.

This second justification for open data, *political-economic*, is rooted in a normative commitment to the democratization of knowledge, and in a parallel commitment to efficiency in academic research. Efficiency arguments have two basic elements: the effective utilization of public funding and the increased potential for innovation due to cooperation and knowledge-sharing. A salient example of the latter arose in the wake of breakthroughs in human genome sequencing when there were consequent efforts to commodify such knowledge (Borgman, 2017, p. 208). A report published in *Science* as

this debate was unfolding does well to demonstrate commitment to knowledge-sharing. Genomic experts who attended a workshop on the subject had responses ranging from cautious apprehension to outright alarm: “Being able to copyright the genome would make me very uncomfortable,” said Frank Ruddle of Yale. And Caskey asked if there is a precedent for saying, “This information is so important that it cannot be proprietary. This is the first time we’ll ever get this information on man—can we make a special case?” (Roberts, 1987).

An argument for the effective use of public funding can be found the earliest framing of open data and its advantages (Borgman, 2017, p. 44). In their paper, Murray-Rust and Rzepa (2004) contend that “Most publicly funded scientific information is never fully published and decays rapidly.” Wilkinson et al. (2016) point to structural conditions, arguing that “Unfortunately, the existing digital ecosystem surrounding scholarly data publication prevents us from extracting maximum benefit from our research investments.” Similarly, there are those who highlight slower innovation in the absence of data sharing. Work studying researchers’ attitudes towards data sharing argues that “Recent studies have estimated the annual financial cost of not sharing FAIR [Findable, Accessible, Interoperable, and Reusable] data to be at least €10.2bn for the European economy, while the impact of FAIR on potential economic annual growth is estimated to be €16bn annually” (Pujol Priego et al., 2022).

The third justification for open data, *regulation*, asserts that transparent data practices can help deter academic fraud by increasing the likelihood of discovery. As the stakes of academic careers have increased beneath the challenges of contingent employment, more soft money positions, and the exacerbation of rewards to success/risks of failure, so have the temptations to artificially achieve success through forms of deception. Open data is seen as a preventative and proactive mechanism to limit academic fraud.

The authors of an early report about data-sharing, commissioned by the National Research Council, maintain that “The expectation that

further analyses and comparisons will be conducted should discourage dishonest manipulations” (Fienberg et al., 1985). The characterization of these manipulations range from widespread bad practice—“researchers have incentives to analyze and present data to make them more ‘publishable,’ even at the expense of accuracy” (Miguel et al., 2014)—to malicious falsification—“One of the worst frustrations of scientists and decision makers is caused by a revelation or strong suspicion that information that was presumed correct and on which results, recommendations, or decisions were based is faulty. Reactions are particularly bitter when willful fabrication, falsification, or distortion of data is involved” (Fienberg et al., 1985).

Freese and Peterson argue that under a publication regime that expects authors to comply with open data standards, even cases that argue for an exemption will benefit from increased credibility (2017). In other words, even when a strong argument against the public availability of certain data can be made, an explicit disclaimer regarding the possibility that the data “can be verified independently in principle” could bolster the work’s reliability. Open data, by this logic, can be part of a regulatory apparatus that disincentivizes fraud through the increased likelihood that it could be discovered. In the context where the structure of academic careers has incentivized the artificial production of scholarly success, access to the raw material of scholarly inquiry could moderate harmful fraudulent behavior.

Ethnography: what (if anything) gets lost in translation

The call for open science emerged within the natural sciences and has slowly made its way to the more quantitative social sciences. Those in disciplines with diverse methodological approaches (sociology, in particular) and epistemic cultures are thereby some of the first spaces to undertake the translational work of migrating open science into ethnographic practice. We say “translate” because different scientific approaches use different lexicons—drawing upon different logics of inference, different approaches to data gathering, documentation, and analysis, and different understandings of positionality and epistemology.

This translation raises several questions: (1) are reliability, equity & efficiency, and fraud problems in ethnography? (2) if so, are they similar in nature to the problems identified in the open data framework? (3) does the open data framework offer reasonable solutions to these problems? In this section we attempt to answer these questions for ethnographic work in the abstract by drawing upon a concrete reflection on our (Hirsch and Khan, 2020) own work on sexual violence. We find degrees of both congruence and divergence, suggesting significant insights are lost in translation if migrated to the field in too direct of a manner. Overall, we suggest that the problems identified are real, but the solution of open data may not solve these problems, and in some instances, may in fact damage data quality as well as the overall trust in ethnographic practice.

Reliability and replication

At its core, reliability is about whether findings can be replicated. This seems a simple enough concept. Either a finding can be reproduced, or it cannot. Quantitative replication is grounded in generating the same outcomes, broadly presented in two pathways: re-analyzing the same data to generate the same answer or drawing an

equivalent sample to generate the same results. For ethnographers, given the unique situatedness of ethnographic knowledge construction, replication through these pathways presents significant challenges.

In the first pathway, if Scholar A generates an association between two variables, then Scholar B should be able to as well, by following Scholar A’s techniques with Scholar A’s data. Yet ethnographic fieldwork is unlikely to be used in this way. First, because fieldnotes do not contain all the information a scholar uses to write from. The very process of gathering data creates impressions, recollections, sensibilities, and a general feel for a person, place, or context all of which are not fully describable, or that may not be captured in fieldnotes. These are referred to as the “head notes” (Ottenberg, 1990) that accompany our fieldnotes. Efforts to saturate fieldnotes with all these details would likely make such notes less likely to be interpretable, because the mass of information would be overwhelming. Our own data, for example, includes tens of thousands of pages of transcripts, notes, and other research materials. Saturating these data with the headnotes of the team of ethnographers who collected them would make them largely inaccessible for interpretive analysis. This also does not recognize a fundamental epistemic approach of ethnographers, one that relies upon a recognition of positionality and reflexivity. Fieldnotes are not “objectively” constructed; they are positionally constituted, conditional on the relationships the subject-observer has with the research-subject.

In the second pathway, making parallel observations, Scholar B should be able to generate the same association between two variables as Scholar A by drawing an equivalent sample to Scholar A and then using their analytic strategy. This pathway is also unlikely to apply to ethnography because producing the same observations is unrealistic for two reasons. The first is, simply, time. New and unique observations will be generated in a different place/time from the original observations. Those two sets of observations are unlikely to align (though this is not necessarily evidence of a lack of reliability). Second, a different observer is likely to have different interactions with subjects. This post-positivist epistemic stance is an important distinction within the practice of ethnography. Both positionality and reflexivity suggest that parallel observations are unlikely to generate equivalent findings. As Murphy et al. (2021) note, “each ethnography is a snapshot of a vanished moment in time captured from the distinct perspective (or bias) of the researcher.”

Given these challenges, one might suggest that ethnography cannot be replicated and, therefore, it may not be a “science.” But the game has been fixed, grounded in assumptions not from ethnographic knowledge production, and instead from particular kinds of quantitative knowledge production, with its distinct epistemic culture. The very concept of replication is likely to be quite distinct within ethnographic inquiry, in part, because ethnographic work operates under a different logic of generalizability than that of most quantitative work. Ours is not a radical position; indeed, Freese and Peterson (2017) have noted that “replication is simply the wrong language to apply to qualitative studies” (159).

Let us be clear: quantitative epistemic culture is valuable and legitimate. It differs, however, from the equally valuable and legitimate ethnographic epistemic culture. The focus of ethnographic inquiry is not on empirical generalizability and replication. Some ethnographic work is highly descriptive, seeking to describe the world from the subject’s point of view, or provide an account of a group of people, a

place, a kind of interaction, without any attempt to generalize. And when ethnographers do generalize, rarely do they seek to do so empirically. Instead, the ethnographic focus is on conceptual generalizability (Hirsch et al., 2010), which Collins et al. (2024) define as “Generalizations ... not to a population of interest but instead to the abstracted concepts elucidated by observing an example or multiple examples of a case.”

Whereas quantitative sampling often rests on the assumption that (probability) sampled cases are generalizable to cases not sampled, ethnographic work operates under no such presupposition. In fact, we assume specific observations are unlikely to be reproduced. In part, this is because observations are temporally contingent and relationally bound to a particular observer. Patterns should be reproduceable, but specific empirical observations may not be. For example, in our own work on sexual violence (Hirsch and Khan, 2020), it is unlikely that we would have made the same specific observations had the institution we observed been a public university in a rural setting instead of an elite private one in the largest city in the United States. Or, if an observer were to “redo” our study at the same location just 3 years later, changes in student culture after COVID and shifts in the national conversation around sexuality would likely have had a considerable impact.

In this case, the critical feature for replication should not be whether specific observations were reproduced; it should be whether the conceptual framework applied or not. Does the conceptual apparatus continue to help make sense of the specific findings? This is a very different kind of logic of replication. And under such a logic, empirical reproducibility is not the standard. We wonder whether ethnography should be held to the broad conception of reanalysis as a “gold standard” in debates about open data as posed by Murphy et al. (2021). They define reanalysis as any attempt to “independently evaluate an ethnographer’s interpretations and consider alternative options” (2021, p. 44) and go on to present a set of different modes of evaluation, which include conducting secondary analyses of fieldnotes, re-visiting research sites, and comparing ethnographic interpretations to quantitative evidence. We argue that each of these strategies are hardly an appropriate means to the stated end. Their idealness is more reflective of the epistemic cultures driving open science than they are those of ethnographic practice. If embraced, they would likely construct a strong distinction between “scientific ethnography” (which would be viewed favorably) and ethnographic practice which does not take the epistemic culture of quantitative research as its ideal start point.

Secondary analysis does not question the facts but instead aims at evaluating the extent to which a body of data reasonably converges on a particular interpretation and subsequent theoretical implications. Given the sheer volume and intricacy of ethnographic data, not to mention the extent to which they are grounded in the researcher’s positionality and are marked by their partial perspective, we question the expedience of reanalysis for verification or replication on both a pragmatic and theoretical level. As for re-visiting research sites to see how a different researcher at a different time might reach different findings, comparing ethnographic findings to different forms of evidence, and testing for generalizability, all read more like mainstream scholarly practice which builds on former research to construct new research, rather than narrow replication/verification practices.

We are not arguing against the importance of reliability in ethnographic research. But the justification for open data rests on an

epistemic foundation that differs from the epistemic foundations of ethnographic knowledge. Can the conceptual value be reproduced with other methods? Can a new set of observations generate not the same observations, but the same general patterns? In what ways do differently situated scholars generate different results because of their standpoint and because of how interactions change when the relational context between observer and observed also changes? Whereas the epistemological concerns within quantitative work can be solved by an open data approach (i.e., by specific observations reproduced), the epistemic foundations of qualitative work gives reproducibility a different meaning.

Put succinctly, reliability is important, but it is not “solved” by making data available, or by reproducing specific observations. This suggests that the response to the open data movement may look very different within ethnographic practice. Rather than start from the assumption that when it comes to addressing reliability, what is good for quantitative data (i.e., open data) should also be good for qualitative data, we should instead ask, “what is the qualitative problem around reliability, and how could it be better addressed?” The solutions to this question should be offered not on the basis of normative claims about desirability, but instead empirical study within our own community.

Political-economic/equity and efficiency

Most ethnography is simultaneously extremely cheap, and enormously costly. The reason for its trivial and exorbitant cost are the same: for the most part, ethnography involves one person (or in the case of our work, a small team of people) gathering information outside the time they must spend to sustain their life and career. The high cost is due to the absence of an economy of scale. On a community level, a lot of work is done that is underutilized. The low cost is attributed to graduate students and faculty for whom ethnography typically means conducting fieldwork between bouts of teaching. A lucky few have fellowships or sabbaticals, which exacerbate inequality within the field. This is in addition to other factors that result in significant inequalities within ethnographic production: familial/community commitments that interfere with the one’s ability to be in the field, lack of material resources required for moving to a specific location to gather data, and other resource constraints (e.g., time, money, etc.) required for other practicalities of research like transcription. These costs disproportionately affect more marginal scholars and they are different from the inequalities of, say, who receives privileged access to tax return data. The political-economic problems in ethnography are more about institutional challenges than they are about whether or not data are made accessible; qualitative scholars face problems with the support they receive (e.g., the logic of graduate programs, the organization of teaching, etc. are not structured with ethnographic research in mind). Scholars, especially marginal ones, require more support to do their work.

This is particularly important because focusing on the challenge of privileged access to data as the main political-economic problem may not result in a solution for more marginal scholars at all. While increased access to existing data may well reduce the cost of gathering information in general, more advantaged scholars will have determined what kinds of information are gathered. The data may provide a lot of information, but not the information a marginal researcher is interested in. For example, the recent American Voices

Project (AVP) is an enormously valuable—and expensive—“qualitative census.” It asks a probability sample of Americans to tell their stories. The data provide a rich archive of life in the United States today. We have even used it ourselves (Caputo et al., forthcoming). But the data do not, for example, ask subjects about sexual identity. Researchers interested in queer life will find that AVP does not apply to their research interests. Using it is not an option; they’d need to gather their own data.

Providing access to data for more marginal researchers only addresses inequities if that data includes inquiries into the topics more marginal researchers are interested in. Large scale shared datasets are, by their nature, aimed at the core interest area of the discipline. Therefore, they are unlikely to generate much value to those who undertake research in more marginal areas of the discipline that would reflect their perspectives. Equity is an important scientific value, and while it is more likely than not to be improved when data becomes more accessible to quantitative scholars, we continue to see how qualitative data gathering is different. Equity means supporting work of marginalized scholars, not giving them access to work that others have produced.

Fraudulent findings and ethnography

The third primary justification for open data is regulatory: preventing fraud. Fraud happens. Within biomedical and life sciences papers, studies suggest that most retractions (67.4%) were because of fraud, duplicate publication, or plagiarism (Fang et al., 2012). Retractions in science journals are rare, and there is evidence that a small number of scientists are responsible for a disproportionate number of them (Brainard, 2018). We should be cautious of this claim, however, as one retraction may precipitate discovery of additional cases by the same person which suggests those who are “undiscovered” have not yet triggered their own retraction cascades. Within the natural sciences themselves, researchers who have investigated the link between data sharing requirements and article retractions or corrections have found none (Berberi and Roche, 2022). This suggests that open data may not increase corrections nor have significant impact on fraud.

Within qualitative research there are some well-known and rehearsed accusations of fraud, but there is little concrete evidence. Some scholars have been highly skeptical and critical of ethnographic knowledge and suggest a high probability of fraud (Lubet, 2017, 2019). Regardless of the degree of accuracy in these critiques, there is certainly fraud within qualitative research and the lack of concrete evidence suggests it has largely gone undiscovered. This is troubling, particularly given findings that textual evidence from mixed-methods research could not be verified in about one out of five studies, even upon contacting authors of studies in question (Moravcsik, 2014). The question is not whether there is fraud, but instead whether open data would make it more discoverable.

Given the overall absence of discoveries of fraud alongside the near certainty of its existence, it is difficult to argue that open data would have a negative effect upon reducing fraud. But so far the limited empirical evidence suggests that open data does not significantly reduce fraud so it cannot be expected to be an effective treatment for this problem. As we have learned in other contexts, punishment of those who violate our shared normative commitments can help reduce such violations. But scholars of punishment also note that when preventing norm violations, punishment is a relatively weak

tool (Kleiman, 2009) that comes with a host of negative sequelae for communities (Fagan and Meares, 2008).

Negative impact on data quality?

So far, most of this reflection has been in response to the logic of open data. Before we conclude, it is important to shift perspectives and consider some of the unintended consequences open data may have for qualitative scholars. We use our own work (Khan et al., 2018; Hirsch et al., 2019; Hirsch and Khan, 2020) as a guide. The motivating question is, “what would happen to the quality of ethnographic data if it were required to be broadly accessible?” We suggest that there may be perverse implications to open data—a movement meant to increase data quality—wherein data quality may decline.

Our own research would not have been possible with open data practices. We can say this with some degree of certainty because most victims of sexual violence elect not to engage in processes wherein their experiences could be known or identifiable. Such a claim runs counter to what Mozersky et al. (2020) found in their work, where they interviewed 30 people who participated in sensitive qualitative studies to better understand their concerns about data sharing. These interview subjects had participated in a health or sensitive health behavior study, involving topics like substance abuse and/or sexual behavior. While their study provides important *retrospective* accounts of how people felt, it does not provide a *prospective* understanding of what interview subjects would have said, had they known their data would be shared.

One of our works, “I did not want to be ‘that’ Girl” (Khan et al., 2018) reflects the sentiment of many survivors of sexual violence. The research subjects did not want to be known as the ‘girl’ who was raped. Time and again, people were willing to tell us their stories in part because we promised to hold their stories close, do them justice, and protect their identities. We heard stories not only from people who experienced harm, but from those who committed it. We cannot fully predict what would have happened if we had told research subjects that their narratives would become available to others, but we have reasonable grounds to speculate: fewer than 5% of assaults are reported, and a primary reason is the potential publicity of that reporting (Mellins et al., 2017).

If these accounts were sufficiently anonymized to protect the research subjects, so many details would have to be removed or disassociated from the narratives that the data would become unusable. The questions of reliability would be senseless because other scholars would not have sufficient information to reproduce or even evaluate our claims. What if a reader of our open data did not know that someone who told us about assaulting one of their classmates also told us about their own previous, extensive experiences with sexual harm? The story would be profoundly transformed; the explanations would likely be grounded in a lack of information and understanding. What about instances wherein we, as researchers, could compare accounts because we knew two of our interview subjects were dating? But linking them would threaten their privacy. De-linking this information would be necessary but would not give an external party to our research the capacity to fully understand or even evaluate our claims. While for quantitative work there are extremely reasonable and credible justifications for open data—including improved data quality—the general practice is to remove data that is highly

identifiable (so, where the “cells” contain fewer than 5 observations). For ethnographic work, the “richness” of the data is conditional, in part, on producing “cells” of one. De-identifying people to the point where they shared enough in common with others to be undiscoverable would render data useless for the purposes of not only replication, but for any analysis at all. For most ethnographic work data quality and usability would likely decline should information become more accessible. Stories would go untold. Key facts would remain missing. We would likely know less, not more.

Of course, ours is a special case. But it is not a rare one. Nearly 50% of women will experience an assault in their lifetime. And there are other kinds of experiences (e.g., family abuse, suicidality, transphobia, systemic racism, etc.) that subjects are unlikely to potentially make public because of their existing marginality and the risks their stories being revealed to others carry. This is to say nothing of those in particularly dangerous positions (e.g., LGBTQ identified people in nations where they risk death from revelation, political dissident groups in oppressive regimes, etc.). Open data can undermine the legitimacy of sensitive ethnographic studies; such studies often focus on the hardest problems that people face in the course of their lives.² There are also the researchers themselves. In contexts where ethnographers study violence and crime, authors have argued that “unmasking” “can get an ethnographer harmed” (Contreras, 2019: 293). While the “obvious” solution may seem to be to allow special cases exemption from open data, we need to acknowledge that this would further undermine the legitimacy of research that is already hyper-scrutinized. Sexual assault findings are systemically questioned, deemed unbelievable, or viewed as ‘produced’ by motivated researchers. Return to the third sentence of this paragraph and ask, “did I want a citation here? Did I find it credible that this many women are sexually assaulted?” Decades of research shows this number to be quite stable and has been reproduced across contexts. What are we doing if we put an asterisk next to findings from fields that generate insights about harm to marginalized populations? An asterisk that indicates: “this work on more precarious people or on this sensitive subject is not as scientific as others, because it does not meet our standards of open data.”

We should also consider how making fieldnotes and other ethnographic documentation available impacts the documentation itself. Demands that we, as scholars, redact our fieldnotes to the point where they would be readily accessible to other scholars (thus preserving the anonymization promised to our subject), would profoundly harm the quality of the raw data. So much information would have to be lost by rule. This would be done for the unlikely chance that others might want to access it. We speculate that the harm to knowledge would be far more profound than the problem such a solution attempts to fix.

Discussion/conclusion

In his classic and often quoted paper, “Whose Side Are We On,” Becker (1967) acknowledges the challenges of ethnographic science and suggests a path forward.

We can, I think, satisfy the demands of our science by always making clear the limits of what we have studied, marking the boundaries beyond which our findings cannot be safely applied... for instance, that we have studied the prison through the eyes of the inmates and not through the eyes of the guards and other involved parties. We warn people, thus, that our study tells us only how things look from a vantage point... (247)

We see the enduring wisdom in Becker. This is not to say that in embracing Becker’s perspective, ethnographic practice should ignore demands for data transparency in the information age. In fact, there have been important contributions to the exploration of how ethnographic practice should respond to these demands (Murphy et al., 2021). And as Freese et al. (2022) note, data can be transparent or reproducible without necessarily becoming “open.” Secure data repositories with clear policies and procedures for data security are one such example (see also, Pool, 2017).

Ricœur’s (1970) distinction between a “hermeneutics of suspicion” and a “hermeneutics of faith” is helpful in thinking through the interpretation of qualitative data. Advocates of a hermeneutics intent on restoring meaning offer the concept of faith as a corrective to the hegemony of interpretative practices that are founded in suspicion. Ricœur famously named Freud, Marx, and Nietzsche fathers of suspicion, in that they offer models of symptomatic interpretation, viewing phenomena as they present themselves as surface-level artifacts of a deeper pattern prescribed by their strong theories.

We wish to broaden the application of this distinction beyond the realm of data to the interpretation of scholarship. Given the powerful tools at the disposal of quantitative social scientists who readily acknowledge their sensitivity to specifications, the relative transparency of quantitative data, and the often performatively “scientific” nature of their claims, we accept that a somewhat high level of suspicion might be necessary when evaluating causal work. This, in turn, serves as solid ground for a legitimate application of an open data framework to quantitative social science.

On the other hand, given the intrinsically interpretative and narrative nature of much ethnographic work, we cautiously offer a more restorative approach to its evaluation. To best read ethnography, we must, to some extent, suspend our suspicion. To be sure, we are in no way advocating for a non-critical engagement with ethnographic work, but a strongly suspicious approach to the facts as they are presented seems impractical to us. This is in part because ethnographic replication is not about the facts being reproducible; it’s about the interpretive capacity of the conceptual framework produced from those facts. Importantly, as an epistemic community we address suspicions through more work on a topic—as a community of scholars that consistently re-engages arguments to evaluate their quality and capacity for explanation.

Put simply, if someone does not want to believe the ethnographer or if the tools at the ethnographer’s disposal seem inherently unscientific to any given reader, nothing the ethnographer does will be enough to appease that suspicion – open data practices included. We are therefore skeptical of applying open data practices to ethnography, not least out of a concern that the process of introducing broader open science logics might play into the delegitimization of interpretative research.

² We would like to thank the third reviewer of this paper for this sentence.

Our skeptical assessment of the open data movement and its impact on ethnography may be read as too definitive. There are three implications to our claims. First, we have suggested that the three primary justifications for open data—epistemic, political-economic, and regulatory—may not fully apply to the ethnographic context. Second, we have asked whether the presumed improvements to data quality may instead have negative impacts. And finally, even if we are wrong regarding both claims, we have asked whether the subsequent data would even be useful. What would it mean to have a dataset without a codebook?

We advise caution before requiring ethnographers to comply with open data requirements. Just as qualitative researchers do not import epistemic and ontological understandings from more quantitative work, so too should they not unquestioningly import the reasoning, justification, and solutions provided by the broader open science framework. Instead, we end with two suggestions. First, the inquiry into open science be scientific and not ideological, with support for research on the impacts of different solutions rather a simple commitment to a particular movement. It is not unique for a scientific movement to rest primarily on normative claims rather than evidence, but there is some irony to the relative lack of empirical work supporting open science, and open data in particular. Ethnographers have an opportunity to begin from a different place: that of study. Second, instead of importing the problem, justifications, and solutions from our natural science colleagues, we should instead look to our own challenges so we can fashion solutions that improve our understanding of the social world.

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Author contributions

SK: Writing – original draft, Writing – review & editing. JH: Writing – review & editing. OZ-Z: Writing – original draft, Writing – review & editing.

Funding

The author (s) declare that no financial support was received for the research, authorship, and/or publication of this article.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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RECEIVED 02 February 2023

ACCEPTED 19 February 2024

PUBLISHED 05 June 2024

CITATION

Enriquez D (2024) Publishing publicly
available interview data: an empirical example
of the experience of publishing interview
data.

Front. Sociol. 9:1157514.

doi: 10.3389/fsoc.2024.1157514

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Publishing publicly available interview data: an empirical example of the experience of publishing interview data

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In September 2021 I made a collection of interview transcripts available for public use under a Creative Commons license through the Princeton DataSpace. The interviews include 39 conversations I had with gig workers at AmazonFlex, Uber, and Lyft in 2019 as part of a study on automation efforts within these organizations. I made this decision because (1) I was required to contribute to a publicly available data set as a requirement of my funding and (2) I saw it as an opportunity to engage in the collaborative qualitative science experiments emerging in Science and Technology studies. This article documents my thought process and step-by-step design decisions for designing a study, gathering data, masking it, and publishing it in a public archive. Importantly, once I decided to publish these data, I determined that each choice about how the study would be designed and implemented had to be assessed for risk to the interviewee in a very deliberate way. It is not meant to be comprehensive and cover every possible condition a researcher may face while producing qualitative data. I aimed to be transparent both in my interview data and the process it took to gather and publish these data. I use this article to illustrate my thought process as I made each design decision for this study in hopes that it could be useful to a future researcher considering their own data publishing process.

KEYWORDS

open source, qualitative methods, interview data, secondary data, archival data

1 Introduction

A few weeks before I published my interview data online, I announced through Twitter that I had a set of interview transcripts with gig workers from Amazon, Uber, and Lyft that I intended to publish for public use. I did this as a first step in my distribution effort. I know distribution is a numbers game: the more people who know the data exists, the more likely it is to reach the handful of people who need it. What I had not anticipated, however, was how excited Academic Twitter became about this news: within 24h, my tweet passed 200,000 views and the thread was filled with requests from other academics and UX researchers who asked me to share the link to the data set once I posted it. I learned that within academic and industry settings, there is a lot of interest in public qualitative data sets and many questions about how one would make recent qualitative data public. While many people were excited and grateful to see my announcement, some researchers have concerns about the privacy of the subjects and the value other researchers can derive from interview transcripts read outside the original researcher's fieldwork (Daries et al., 2014).

These data were the product of a larger study on pre-automation and experiences with self-driving vehicles. I began this project with a team of five researchers. I handled the design and gathered the interview data for this study with input from the PI and my other colleagues. My interviews were a piece of a larger project that relied in part on publicly accessible survey data, patent data, and my qualitative fieldwork. My co-authors on the larger project included three quantitatively focused researchers in addition to my qualitative work, which was overseen by another qualitatively focused PI. This larger project produced three articles using different parts of the project's complete data gathering effort.

One condition of our funding for the initial project was that we contribute some of our original data to a public archive. Increasing numbers of funders are encouraging this kind of output from studies with the hope that the data can aid other researchers in their work. As a group we determined that the interview data was the clearest data asset for us to contribute to an archive. Within our overall data collection efforts, the interviews were a new data contribution in a rapidly changing field, whereas the quantitative data was already publicly available information. Once we began our study design, I needed to consider what it meant to create a meaningful asset to the publicly available qualitative data. Every design decision I made about the fieldwork – from the first draft of the interview guides through publication – was made with an eye toward risk assessment for the interviewee and the desire to produce a meaningful public asset.

There are many debates about open science, including debates focused on qualitative research. Some debates focus on the practice of open science and the value of collaboration production of new ideas (Hughes, 1993; Cowan, 1999; Becker, 2008; Aad et al., 2015). Some debates focus on improving transparency and reproducibility in sciences overall (Travis, 1981; King, 2011; Elman and Kapiszewski, 2014; Moravcsik, 2014; Aguinis and Solarino, 2019; Shaw et al., 2019; Pratt et al., 2020; Jacobs et al., 2021; Nosek et al., 2021). Others focus on the ethics of publishing qualitative data (Wax, 1977; Parry and Mauthner, 2004; Mauthner and Parry, 2009; Mauthner and Parry, 2013). Others theorize about the potential value secondary data has for open science (Heiskala, 1998; Wallerstein, 1998; Corti, 2000; Moore, 2007; Abu-Lughod, 2008; Heaton, 2008; Knorr-Cetina, 2013; Tsai et al., 2016; Freese and Peterson, 2017; DuBois et al., 2018; Feldman and Shaw, 2019; Ruggiano and Perry, 2019; Class et al., 2021). Though these are important debates, they are not the focus of my article. I reference some of these debates as I elaborate on my thought-processes and my design decisions, but I am more focused on what decisions I made as I prepared to publish my interview data. When I decided to publish my data, I had trouble finding a step-by-step account of how to publish it and what design decisions I needed to make along the way. The purpose of this article is to provide that step-by-step example I needed in case it is useful to someone else considering making their own qualitative data contributions.

My position is that the qualitative researchers who study technology and other rapidly changing fields will need to work more collaboratively to work effectively. One way to improve collaboration may be to share qualitative and quantitative data more frequently with both other academics, as well as the journalists and policy makers who are expected to record these historical moments and design appropriate policies. The data we gather is also useful to the researchers inside companies like the ones I consider in my study: while internal UX researchers may not be able to demand funding or working time

to study the issues workers face in automated workplaces, my data provides a historical record of their experiences. The record itself may be useful for designers and engineers inside firms who are thinking about the best ways to improve worker experiences alongside the more finically focused goals of their product managers.

Through careful design and on-going conversations with respondents, I believe there are responsible ways to gather and publish some kinds of qualitative data. I agree that not all qualitative data can or should be shared publicly for a variety of reasons that I will discuss. I want to be clear: I designed my study with the intent of publishing the interview data but not the more informally structured qualitative data I gathered to support and corroborate my interview findings. I argue a crucial piece of secondary data analysis is completing some form of additional data collection to compare and corroborate qualitative data sets as a method of analysis in new studies. I present my case as an example of how someone could prepare their data for a public archive, as well as design choices I would make differently next time.

In this article, I explain:

- 1 my thought process at each stage of the design and implementation of my study,
- 2 how I prepared these data for publication,
- 3 how I published and distributed these data,
- 4 and the use cases that emerged once these data were public.

I conclude the article with the lessons I learned as I implemented my project and what I might do differently next time. I hope this step-by-step discussion is useful to others who are considering their own data publishing processes.

2 Why I was motivated to share my data

I decided to publish my interview data for two reasons: (1) I was required to make a public data asset as a requirement from our funder and (2) I believe in the efforts emerging in Science and Technology studies to produce more collaborative, real time research.

Even before the funder's requirement, I was motivated to publish my interview data because I am committed to the work made possible through collaboration and comparison of qualitative data through emerging "open science" experiments. Collaboration between researchers already happens within Science and Technology studies as well as Anthropology (Ducheneaut, 2005; Shamir, 2010; Heller et al., 2011; Collins et al., 2016; Collins, 2017). These modern cases of collaborative qualitative research, especially around rapidly changing fields like Science and Technology, demonstrate that there is value in interpretive collaboration within qualitative social science. It may, in fact, be necessary. As an important example: in Vertesi's (2015), a team of scientists collectively interpret images of Mars to develop a system of building knowledge for an environment they will likely never experience outside the data collected by their rovers and the images processed by their computers. As Vertesi describes: these images, as a type of qualitative data, require this kind of collective narrative building because the data itself is so far from our own contexts that it requires a team effort to interpret. At its core, this is a scientific

example where scientists must work with limited data to inform their future experiments and data gathering processes. Another example of a collaborative method in ethnography was the “swarm ethnography” method developed by Goodman and Vertesi (2012). Their method was designed to capture the full picture of a technology in a social context through simultaneous, fast-paced qualitative work with the intent to contextualize several different researchers’ data and analyses alongside one another. While each researcher managed their own interviews and gathered fieldnotes, they collectively had to make decisions about how to format and present their fieldnotes to the whole team alongside their analysis. Their effort to package their fieldnotes and analysis in consistent formats would improve their ability to compare data and analysis across each interviewer’s contributions. These are two examples of collaborative qualitative work that benefit from collaboration between researchers and their data sets.

My data and research are also grounded in the methods and literature of Science and Technology studies as much as they are grounded in Sociology. I chose to study how workers reacted to technology in their workplace, knowing the specific kind of worker I wanted to study was often very pressed for time and difficult to access. Given how quickly the technology evolves and how difficult these studies can be to conduct, I decided early on to share my data as a potentially useful snapshot of a technology and human reactions to the technology in time. To me, publishing my interview data as my data snapshot alongside the articles where I introduced my analysis of the field was a useful contribution to a rapidly changing field. I want to invite other researchers to collaborate with me and compare our interpretations over time.

3 Part one: designing a study with the intent to publish interview data

There were many design decisions I needed to make upfront once I decided I was going to publish my interview data. These decisions felt important because I know that not every data set should be made publicly available. I was determined to figure out what a public data set that was respectful of my interviewees’ privacy and still useful to a broader audience could look like. The questions I considered in my design choices are not meant to be exhaustive – they should be seen as a case that moved from design through implementation and into the aftermath of publishing public data.

4 Design decisions I made during my interview design

I strongly believed that I needed to make careful design decisions about my fieldwork and the final interview dataset from the beginning. This felt like a crucial step toward defining and managing risk for the interviewee. The safest way to gather the data I needed and assess risk for interviewees seemed to be through semi-structured interviews. The risk assessments would be translated into my consent forms and discussed with interviewees before we began. I explain here how I approached the design of my interviews and assessed my hypothetical data to determine which pieces were publishable and which seemed too risky.

I chose to focus on publishing my interview data, rather than the fieldnotes I assumed I would collect, because I believed a structured interview set would be easier to (1) manage for potential risks to interviewees and (2) work with as an artifact in a data archive. When I define my interviews as “structured data,” I mean I wrote a list of specific questions I wanted to ask. While there could be some variation in how the final question was worded, which would allow me to use the same kind of technical terms my interviewee preferred to use when describing their jobs and the technology they used at work, the intent and meaning of the question were clearly defined. The interviews would be short and efficient (15–20 min with three sections: one about their work experiences, one about their technology at their gig work job, and the final section focused on their perceptions on the future of technology). In contrast, I describe a “semi-structured interview” as an interview with structured questions and additional, flexible time in the interview for more thematically defined questions that would adapt to the specific person. I did not leave time for this kind of undefined interview portion – my intent was to ask each interviewee the same questions.

The clearer structure of our conversation thus gave me a clearer sense of what risks could arise with each question. Through structured interviews, the cases are a little easier to compare and the structure helps me corroborate company/organization-related events or technology features someone describes to see if what is described to me is a common occurrence in that moment of time or an unusual case. Given the triangulation I already do with my interviews, I hope someone else later could work with the structured data in their own triangulation efforts and/or trace specific themes over time (DuBois et al., 2018). Thinking about these future potential use cases among scholars impacted the way I thought about the structure of my data. I did not design my fieldnotes with the intention of publishing them as a public asset, which I will explain later in this article in the section on preparing data to be published.

5 Establishing a consent protocol

The depth of qualitative data and the relationships we build in our field sites give us access to intimate details of people’s lives. Within these relationships, some data are more sensitive than others. As researchers, we can anticipate some aspects of an interview that could be sensitive topics for an interviewee. Other sensitive topics may be difficult to predict. I tried to think carefully about how to communicate and manage risk for my interviewees.

5.1 Assessing risk for interviewees

One risk I know researchers face as we conduct research with living respondents is that legal constraints can limit participant privacy in unexpected ways. A case where an interviewee described planned violence against another individual, for example, would mean I was legally obligated to report information to law enforcement (Weiss, 1994). Another potential risk was that my publicly available qualitative data could include information that causes trouble for the individual if their testimony became part of a criminal case. While we as researchers can sometimes anticipate these issues and file for exemptions, we cannot always predict what someone will share with us nor whether it might

become interesting to law enforcement (Khan, 2019). In my assessments of risks, I determined that I did not anticipate these issues as serious risks in my brief and structured interviews because I was focusing on (1) the interviewee's legal work experiences, (2) their interactions with existing and legal technologies in their workplaces, and (3) their perceptions of hypothetical technologies like self-driving cars.

A more difficult step in my risk assessments was about how to describe privacy risks that come with published interview transcripts that could be accessible to anyone on the internet. Privacy on the internet is an increasingly challenging obstacle for researchers and private citizens. Murphy et al. note the pressure ethnographers face to switch from traditional handwritten notes to more shareable audio files and other digital formats of fieldnotes (2021). Moving toward sharable digital files presents new concerns about managing participants' privacy (Murphy et al., 2021). No matter how many data security measures researchers take, we know from the frequent data leaks at large companies that there are limits to privacy when participant information is stored digitally (Balebako et al., 2013; Ragab et al., 2021; Leonardi and Neeley, 2022). Even survey data could be used to trace information in the shared data sets back to respondents (Narayanan and Shmatikov, 2009). Digital ethnographers face even more extreme privacy challenges: because much of their fieldwork occurs in a searchable public domain, it is particularly difficult to maintain participants' anonymity (Geiger and Ribes, 2011; Shklovski and Vertesi, 2013; Reich, 2015; Enriquez and Vertesi, 2021).

I believe concerns about privacy and other risks associated with sharing recent data exist on a spectrum. There are different kinds of issues and degrees of severity of risk that exist with fieldnotes, interviews, and survey data. There is also greater risk to individuals sharing personal data (i.e., financial, or medical data) vs. more observational data (i.e., how does this kind of technology work, how do you do your job). Knowing I would be sharing our interview data when this project was completed, I developed a discussion guide that was focused on the interviewee's experiences at work and with the technology they were using. With the design of the guide in mind, I could explain the kinds of questions I wanted to ask them as I established a consent protocol. It is easier to consent to something more concrete (including specific kinds of questions I wanted to ask) than it is to ask for consent to observe someone and then publish my notes about their behaviors, for example.

5.2 Communicating risk to interviewees

One consideration I had as I designed the study is that most adults are now used to hearing or reading interviews in public media – whether this is on the radio, in a news story, or through a family member's school assignments. Collections of interviews come in many different formats for public consumption. For example, collections of interviews, like Nobel Prize winner Svetlana Alexievich (2017), present a curated series of interviews that together paint a picture for the reader of what it was like to experience the collapse of the USSR. Public projects like the Story Corps allow individuals to upload their own oral histories, interviews, and conversations into a public forum for anyone to use.¹ Oral histories are frequently sampled from

library collections and public forums like Story Corps to produce radio shows like the BBC's "Listening Project"² and NPR's Story Corps channel.³ I introduce all these very public cases to demonstrate that the process of gathering and listening to oral histories and interviews is a very common experience, and our interview subjects are very likely to be familiar with the format. I could mention these examples of publicly available interviews to interviewees while explaining where the interview transcripts could appear in an archive to a student or someone else hoping to read the interviews. When one is familiar with the format and the final product, it is easier to consent to than when the final product is as distant to them as an academic article, for example.

There are debates in qualitative research about whether an interviewee could fully assess the risks associated with publishing their full interview transcripts. Many ethical debates focus on the harms that could occur and that respondents may not want their data to be public, but they often omit the idea that maybe the respondent would like their full transcript available because there are also harms in being misrepresented or presented out of context (Tamminen et al., 2021). I am uncomfortable with the assumption that our adult respondents cannot comprehend the risk of sharing their experiences or opinions enough to consent to have their interview transcript published in full in a public archive. The full transcript does not necessarily feel riskier to me than using some of the deeply specific segments of interviews as quotes in academic articles, which carry their own risks for being similarly traceable. In practice, even in sensitive contexts where the subjects are asked about their health, many respondents are willing to contribute their data to other research projects beyond the current project because it is perceived as a public good (Yardley et al., 2014). Some researchers argue that publishing data, as a product co-produced by the researcher and the respondent, should not be the choice of the researcher alone (Parry and Mauthner, 2004). I agree – consent to publish data is necessary. However, given their role as co-producers, adult respondents should have agency to decide whether their structured interview data becomes public. We may even consider whether interviewees should have a right to demand that their full interview is made publicly available alongside the selected quotes an author presents with their article.

While I could explain some privacy risks upfront, it is necessary to remind interviewees that we cannot predict every possible use case of a public interview transcript. Several scholars argue that it is impossible to state or predict every possible outcome of research or archival data – after all, if we knew every outcome, there would be no need to conduct the research (Bishop, 2009). Discussing the outcomes as some cases we can predict and others we cannot is honest. It may also be necessary to remind the respondent that once something is on the internet, it is nearly impossible to control how someone interacts with it. I see these as features of modern conversations about informed consent in any protocol because they apply to the brief quotes and descriptions selected for papers as much as they apply to a full interview transcript.

¹ More about Story Corps: <https://storycorps.org/>.

² More about the BBC Listening Project: <https://www.bbc.co.uk/programmes/b01cqx3b>.

³ More about NPR: Story Corps <https://www.npr.org/series/4516989/storycorps>.

5.3 Designing the consent protocol

I decided that consent was never going to be a single conversation with the subject. Consent and privacy are a multi-party process, rather than a single moment (Cutcliffe and Ramcharan, 2002). There are also active moments of gathering consent, engaging with the interviewee's comfort level throughout a conversation, and providing the interviewee time to reflect after a conversation and before interview transcripts are published. The direct and indirect moments where someone could choose to revoke consent are important – consent in the abstract may be challenging to give, but consent after an interview is completed and an interviewee has time to reflect can be useful too.

My conversations about consent often happen in several stages during an interview. The first occurs during recruitment, when an interviewer presents information about a study and the potential interviewee decides whether to pursue more information. The second occurs when more information about the study is presented to the potential interviewee and an interview is scheduled. Third comes with a formal conversation about consent – ideally as both a consent form that is reviewed and signed by the interviewee AND a verbal consent review before the interview begins. Fourth is the opportunity for an on-going consent reminder/check in if there are uncomfortable moments during an interview. And finally, there is a consent remind at the end where the interviewee is offered time to reflect on their responses and the interview experience. Interviewees are presented with contact information to request redactions in the interview text or file complaints before analysis and data are published.

The first set of design decisions about the interview structure and, subsequently, what I was asking a subject to consent to happened in a loop. I considered the kinds of questions I wanted to ask, assessed how likely the question would be answered with deeply individualized and traceable responses, and then considered how I would ask someone for their consent to share that information with me and the broader public. This interview guide development-consent conversation editing was iteratively produced as I prepared my application for the IRB. My final questions were structured to pose minimal risk to interviewees. This project allowed itself to be more anonymous because I was looking for common work experiences and interactions with technology.

As I anticipated risks to the subjects, I knew that there could be peripheral details in each case that might be sensitive to the individual to discuss. I focused my questions on their experiences at work and interacting with a piece of technology assuming these questions would not be traumatizing or especially sensitive to the subject, given how often these two topics are normalized in discussions one might have with a relative stranger as a form of small talk in social settings. Asking about work could be sensitive in cases where some of the work is illegal or stigmatized, but I was specifically interested in legal forms of gig work completed by publicly traded companies – this means the company and the work completed have some degree of oversight by federal regulators. As I recruited subjects for the interviews, I asked them screening questions about their jobs and told them what I intended to ask about (1) their gig work experiences and (2) their experiences with technology. This screening process helps interviewees manage their expectations at this first consent conversation, where a potential interviewee decides whether to request more information about the interview opportunity.

During my recruitment process, I described the study as a project about gig work experiences and experiences with technologies at work. I was clear that I was not trying to gather personal information about the individual, nor would they be compared to other respondents based on sensitive characteristics like race or religion. While there are many cases where extreme care toward privacy and confidentiality is important, I would argue there are other cases where the descriptions of an experience are so commonplace that the text itself would be extremely difficult to trace back to an individual. In my case, I needed to find a lot of respondents who had common experiences with the technologies they were using so I could describe the technology. I could explain upfront to the workers that I was trying to learn about a specific kind of work and a specific kind of technology through their experiences and perceptions. The biggest concern I anticipated was a worry that a less worker-friendly firm like Amazon might discover an AmazonFlex driver had spoken to me about their experiences. When I brought it up later with some workers during our conversation about consent, several of them noted there are so many workers in so many warehouses with so much turnover that it would be hard to track it to any one person.⁴

The formal consent process consisted of a document I sent to interviewees before the interview for review and their signatures. This consent form reached them through the email address they used to contact me as we scheduled their interviews. I verbally reviewed this consent form at the beginning of our interview and again at the end of the conversation as a less formal check-in to see how the interviewee felt about the conversation. During the consent description at the top of the interview, I also gave them examples of how I would scrub their interviews to remove identifiers and trails back to them. I mentioned I would check in with them informally throughout the conversation if they seemed uncomfortable, and I reminded them that we could skip questions or remove answers if the interviewee expressed any hesitation to a question. In this way, I tried to remind interviewees that consent was both an active conversation and one where I, as the interviewer guiding the conversation, needed to practice reading the room. Consent is not always easy to give, which meant I also needed to read the non-verbal cues of discomfort.

At the end of our conversation, I reminded them that they had resources like my PI's contact information and the IRB office information for Princeton if they needed to register a complaint. I also reminded them that they had my email address if they left the interview and decided they wanted to strike anything from the conversation from the final record. I decided that interviewees should be reminded that there is a lag between interviewing and publication – which gives them time to reflect on an interview and assess any negative experiences or risks they associate with the now completed experience. Sometimes, the period of reflection helps someone weigh the risks a little more easily than they could in the abstract. This final form of consent is more passive and open ended – I presented it as an opportunity to contact me, my PI, or the IRB if they changed their mind or had any questions. If they did not write to us, we are led to believe their earlier moments of direct consent remain valid. Maybe

4 Jason Del Rey, "Leaked Amazon Memo Warns the Company is Running Out of People to Hire," Vox, June 17, 2022, <https://www.vox.com/recode/23170900/leaked-amazon-memo-warehouses-hiring-shortage>.

there should be another point of checking in with them when the interview is published, but I did not offer this final step for this particular project. By the end of our conversation, the interviewees and I had several points of contact about consent.

5.4 Reviewing the consent protocol with the IRB

As part of my consent design process, I had several conversations with Princeton's IRB. I knew that before I gathered any data, I had to explain to Princeton's IRB what kind of data I wanted to collect and why I wanted to make the data public. This required me to explain how the risks to the respondents compared to the public good of making our data publicly accessible. My direct and more hypothetical conversations with the IRB officers helped me navigate the interview guide-consent guide iteration process I mentioned as I designed my consent protocol.

Rather than seeing this as a challenge to be “negotiated” with a hostile regulatory group, I treated this as an opportunity to discuss the possibilities of what I could do in the field and with our data after the project. As an example of what I explained to the IRB: in evaluating the risk of gathering and later sharing this data, my goals were to be (1) minimally invasive in their work days, (2) specific in the kinds of work procedure and technology questions I was asking them, (3) clear that the workers did not need to answer any questions they did not want to answer, and (4) clear that the workers were free to add whatever additional context they felt was important to share with me as they answered my questions.

I argued there could be risks in describing the future of automation in their workplaces, given the existing precarity of their work and the stressful ideas of futures where steady work was even less accessible, but we hoped the interviews we were gathering could help us demonstrate more clearly the role that these workers play in making technology work correctly. While Uber, Amazon, and Lyft may advertise themselves as technology companies, at least the delivery side of their work relies very heavily on the intervention and improvisation of the human workers completing tasks (Shestakofsky, 2017; Enriquez and Vertesi, 2021; Vertesi et al., 2021). The public benefit to making these interviews accessible was (1) academic and (2) allowed the workers themselves to provide their own narrative of their work experiences to a broader audience.

6 Part three: gathering data

6.1 My interview data

The qualitative interview data I published included 39 transcripts of interviews with gig workers from AmazonFlex, Uber, UberEats, and Lyft. I published these data with a ReadMe that provided context on the study that led to this round of fieldwork.⁵ As a basic summary overview, the interviews consisted of a near-even split of 21 female drivers and 19 male drivers. One transcript was omitted from the final

interview pool because I discovered he was lying about his gig work experience. Because I ran recruitment ads through FreshEBT and Facebook, I reached gig workers across the United States, including 25 workers in urban settings and 15 rural workers. While I produced ads and discussion guides in English, Spanish, and French for this study, all the interviews I completed were in English at the preference of the interviewee. I include these details to demonstrate what kind of meta data we decided to include with each interview because these items were visible to me but often not discussed directly in the interview transcript.

I made the decision to drop an interview from the published data set because the interviewee was lying about being a gig worker. I am not the first qualitative researcher to navigate an interview where the interviewee is clearly making something up, either with the intention of telling me what they thought I wanted to hear so they could collect the promised compensation or because it was entertaining to them to do so. Owens (2022), for example, describes encountering a “professional research subject” during fieldwork and how the respondent's responses were different enough from the others in the study that it was clear the respondent joined the study not as a relevant source, but as someone hoping to give the interviewer whatever they wanted to hear. In my interview data set, this subject was clearly lying because the claims he made about his work life varied wildly from sentence to sentence and did not correspond to the descriptions of any of my other research participants. By the end of the 20 min, it was clear this person had not interacted with the gig worker version of the apps I was studying. Owens' case of a “professional research subject” was recruited from a community with a similar economic status as my own catfishing research participant.⁶ Since this case was not an accurate representation of the data I intended to collect, I dropped his case from the final published data set.

Within this process of evaluating the validity of the interview, I realized it could be difficult for someone less familiar with the field site and unable to hear the hesitations and mid-word contradictions of the interviewee over the phone. The reader would be able to see my attempts to untangle some of his conflicting statements and provide clarity, as I experimented with my hypothesis that he was lying about his work experiences but left room for him to clarify his points and show me he had specific examples of how he worked with the technology despite my growing doubts. In the specific clarifying questions I asked, it is clear this interview is different from the others. I still decided to drop the interview from the final data set because I did not think it provided useful insights on the topic I intended to cover with the data when I published. Upon reflection, given the use case that emerged where professors were using the transcripts in classrooms to teach students to code data, it could also have been a useful tool in how to evaluate the validity of qualitative data. I will note that in the critiques of interviews, there are many concerns about

⁵ I included my interview guide and consent form draft in the [Appendix](#).

⁶ Owens describes recruiting subjects for her study through public housing projects, which implies a threshold for how much this individual could earn annually before they were no longer eligible for this kind of housing. My subject was recruited through an ad placed on app for recipients of SNAP and/or WIC benefits, also implying this individual earned under a specific threshold set for those who qualify for state sponsored food subsidies.

people lying or exaggerating in their responses. From this case I present and Owen's example, there are ways to validate interview data that are very difficult to do with standardized, lower social friction data like surveys.

6.2 On-going engagement with consent

As I anticipated, I found in my consent-focused conversations with interviewees, it makes it easier for subjects to consent to their data moving into the public domain when they are familiar with the format and some of its use cases.

Each interviewee and prospective interviewee were emailed a copy of the consent form before our interview began. The consent form is included in the [Appendix](#). I also read and talked through the specifics of the consent form at the beginning of the call (especially around the privacy protocols and that we would publish the transcript when the project was completed). I talked to them about how I think about and clean data. This was to ensure the interviewees understood my intended uses for the data in my study and that they understood the tools I offered them if they were uncomfortable and wanted to withdraw from the study.

One consideration moving forward: I described the audience for the interviews as “researchers” in the written consent form. To academics, the assumption may be that “researchers” indicates other academics. When I verbally reviewed the consent form with each interviewee, I explained to them that these data were going in an academic archive, but anyone could see them. I listed some of the people who would use them but also said there may be other people outside of universities who read them, like journalists. I found that my interviewees interpreted “researchers” to mean anyone conducting “research,” which could include students and journalists, without it becoming a stressful distinction. For future versions of the consent form, it may be better to write out that possible audience members include “generally curious individuals on the internet” to establish that the audience would not just be academics. Some were excited about this possibility; the rest gave neutral responses (for example: “ok,” “sounds good,” “go ahead.”) encouraging me to continue to the interview portion of our conversation. Many of the interviewees returned signed copies of the consent form before we met, though some sent the letters after we met, and I reminded them to submit the letters. All interview subjects consented or reconsented at the beginning of the call verbally before I began the interview. I checked in throughout the conversation, especially when someone provided some personal backstory, to make sure they were still comfortable.

Our consent forms explicitly stated our intention to make the interviews available for public use and offered the interviewees the chance to request that specific parts of their answers be omitted from the final public copy. We often frame data sharing as solely a risk for participants, but I learned in my consent conversations that many of our participants were glad we planned to share their interview transcripts with students and researchers. They were excited that their stories were going to be used in broader research efforts. One interviewee told me about several times throughout our conversation that he was proud to participate and share his story. It clearly meant something to him that his specific story, in his words, was going beyond my research files and into a space for students. This desire to help students and provide evidence from personal experiences is

something I frequently encounter with my interviewees across my studies. The interviewees explained that participating and contributing to data that helps researchers and students learn about the world felt like a benefit, as we hoped it would be perceived to be when we filed our IRB application stating our intentions to make my data public.

In some ways, I expected this conversation about publishing the transcript of our full conversation alongside my academic article to be a positive one. I argue that in the age of social media, where more people are more conscious of their public presence, there are cases where our data is not especially sensitive and we are better equipped to consider what it means to have information about ourselves online ([Marwick, 2015](#)). I find subjects sometimes get nervous about being “quoted out of context.” Thus, offering to make the full conversation available along with my article and my selected quotes from their responses might reassure some respondents who feel better when their full story in their own words is available for corroboration. As a subject later told me, they felt comfortable knowing their stories would be there along with the “rest of [their] own words.”

Further, when I checked in with my subjects at the end of the conversation to ensure they were still comfortable sharing their responses, many of them stated they were eager to be part of a public conversation about their working conditions. Several of them mentioned they were more interested in participating because their data “could help students.” My project was not especially interesting to them, but they liked that the interview could help a broader community of researchers. The results of my conversations about adding the co-produced data to archives are not unusual: even cases of completed studies where an archive contacted respondents directly to ask for their permission to add their interview transcripts to an academic archive were met positively by most respondents ([Cutcliffe and Ramcharan, 2002](#); [Bishop, 2009](#)).

This especially enthusiastic participant, along with several others, chose to disclose other personal information about themselves as additional context outside the scope of the initial discussion guide, but the interview guide and general project goals were not designed with the intention to gather sensitive personal data. When some of our conversations grew more sensitive, I listened and gently tried to guide them back toward the specific questions I mentioned we were trying to discuss. I used the time constraints I promised them (that the interview would be about 20 min) as one way to remind them that I wanted to respect their time and the scope of the interview, but they were welcome to take the time they wanted to add context. I hoped this added reinforcement was helpful for reminding them about the nature of the study and would give them a minute to reflect on whether they wanted me to know the information they were sharing. In the end, two of the interviews, in particular, concerned drug use/recovery. One of the subjects chose to move away from the topic after he made a specific contextual point he felt was relevant to my questions and the other took the time to reflect on why he was completing the kind of work he was doing now.

6.3 Other omitted data: the choice to omit my fieldnotes

I gathered several different kinds of fieldnotes at different points in the study and omitted all of them from the final published data package. While I took great care to consider the structure and goals of

my interviews with an eye toward publishing the interview transcripts, it seemed impossible to structure and assess risk for my fieldnotes in quite the same way. I use fieldnotes as a flexible method to (1) help me review my fieldsite and interviewee recruitment strategies, (2) help me corroborate information presented to me about technologies and/or the structure of a job, and (3) to keep track of news related to these topics shared through social media channels and discussion boards. I typically prepare my fieldnotes into more coherent summaries of information gathered over time for presentation in an academic article. The information I gathered is never completely listed in an academic article, but the analysis is. Further, my fieldnotes for this project were usually not related to specific interviewees themselves, and thus, it did not make sense to attach them to a specific transcript as an addendum with the text. Further researchers may consider what form fieldnotes should take to be shared more publicly and what risk it carries to present them alongside a set of interview transcripts.

In addition to the interviews, I also completed a digital ethnography component to support my interview data for my articles. I spent 1 year gathering fieldnotes observing discussion threads in three Facebook groups that serve as crowdsourcing communities designed and operated by Amazon Flex workers, many of whom also drive for Uber or Lyft. As a general method strategy: I often try to include an ethnographic component in my projects to either help me understand the language and processes I want to ask about directly as I structure my interviews OR I complete the ethnographic work after my initial, focused interviews to corroborate my initial findings. The hypothesis behind the latter as a method choice, given that I study strategies and uses of technology, is that the issues/strategies my interviewees tell me they use are likely visible in the digital community spaces they participate in through the digitally recorded entries of other community members' behaviors (referred to as "digital traces" by Geiger and Ribes, 2011). In the way that ethnographers might do a first round of fieldwork, begin writing, and then return to the field for a more targeted round of research, I used my digital ethnographic work as a very focused look at the types of technical complaints, their frequency, and the responses that Flex drivers introduced to help each other address their problems (Small and Calarco, 2022). This complimented my interview findings and allowed me to extend my data. I include this description here as a case of how one may decide to supplement my interview data with their own fieldwork extension.

The fieldnotes I gathered during the interviews served as the first step in my analysis – I focused on connecting points between different interviews, noting differences in interpretation between interviews, and notes on language (how specifically someone described a technological feature). I did not share these notes because they were a draft of the analysis, which was later refined into an article and shared publicly in that prepared format. I had other fieldnotes from the beginning of the study that I gathered as I was figuring out how to enter the field site. As noted in the description of my data and fieldsite, it is very challenging to get this kind of worker on the phone. Gig work means that the connection between time and money is especially pronounced. We also live in an era with increased phishing and scams, so I had to be careful about how and where I contacted people offering a small amount of compensation for a few minutes of their time for an interview. As I made multiple attempts to contact people through different settings, I made notes on what was or was not working and why I suspected it worked or did not work. These specific methods

notes again made it into my article as a summary of methodological choices, so I did not share the fieldnotes.

The final type of fieldnotes I gathered were around the patterns of conversations that occurred within public Facebook groups run by AmazonFlex drivers. These notes were the least structured kind of fieldnotes I gathered and often it was focused on revisiting questions/points made by my respondents in interviews. As one way to check how frequently an error or issue may occur, I sometimes reviewed the discussions around specific technology or working conditions in these groups. My hypothesis was that if an error/experience a respondent described in an interview was common, it should not be difficult for me to find similar discussions within these central community spaces. My fieldnotes from these secondary review processes were frequently lists of tallies of how often something came up in conversations, variations between different perspectives in the group and my interviews, and cases where the point a respondent made seemed to be a very isolated case. It helped me determine which points in my interviews I would share as the core evidence in my article. As a discipline, our position on how/when we consent and use public digital discussions is still under debate. While I had alerted everyone in these groups to my presence and reason for being there more than once, it is hard to consent everyone on every conversation within these massive groups. My primary purpose for gathering this kind of data was also to structure my analysis and corroborate my findings, so the interviews were still the clearest commentary I could provide to respondents.

I could control and anticipate the design of my structured interviews much more closely than I could anticipate my fieldnotes and ethnographic findings. This made it much easier to structure a conversation about risk and consent when I asked my interviewees for permission to publish our interview transcripts. I was already nervous about the potential response from researchers when I published my structured interview data, so taking an additional leap to try to structure and publish unstructured, adaptive fieldnotes in their rawest form felt too risky to attempt this time and would be an interesting experiment in a future study.

7 Part four: preparing the data

7.1 Reflecting on the audience

Given how quickly technology changes – and so do perceptions about what a technology does or does not do – I knew my interview data would become a historical, rather than present, record on some aspects of the information I was gathering. Capturing data quickly and frequently is especially important in Science and Technology studies, where the regulatory environments and technologies of interest can become practically unrecognizable in a matter of years or even months (Leonardi and Barley, 2008; Wajcman, 2015). Thus, it became useful to think about the final output as a submission to an archive first and a current sociological data set second. I saw this because I think the next project where my interviews are useful is first in a comparison of changes over time using snapshots like mine and second, as a tool for designing future sociological studies or developing hypotheses for future projects. I suspected the data would be useful to journalists studying gig work in in-depth reporting contexts or for the many academics I knew who transitioned from socio-technical work into

industry settings and were trying to improve worker-facing interfaces for these emerging technologies. While the exact data may be outdated for these latter two cases, it could help them develop hypotheses for future research questions in similar ways to sociologists and historians. Thus, I was guided by some of the principles developed by historians and other qualitative researchers who focus on archival work and thought about the kind of information I could preserve in the present that would make my snapshot more useful to them later (Fielding, 2004).

With all this in mind, it was useful to me to think about the historians and other archival researchers who might be interested in my data to answer different kinds of questions. The Oral History Association has provided historians with guidelines and protocols on how to preserve and share data since 1968. Historians who record interviews frequently do so with the intent to add their data into historical archives after they publish their work (Ritchie, 2014a). Sharing interview data in library archives, in their view, contributes to the permanent record of a historical moment and thus offers (?) opportunities for others to interact with the data to produce new theories or alternative interpretations (Bishop, 2005, 2007, 2009, 2014; Ritchie, 2014b). These principles were useful to me in the design stage because I could see how researchers worked with an object once it was further away from the initial moment where the data were produced. I could see how historians and other archival researchers triangulate between different artifacts to make sense of a historical moment, and I could try to anticipate some of these needs in the structure of my interviews and final meta data produced.

7.2 Cleaning data and masking

Designing the data-anonymizing protocol is part of the IRB review and the consent review process. Other challenges emerged once I needed to complete the data masking process. Some edits (like proper nouns) are easier than others. One especially important issue I considered: this data is co-created between me and the interviewee. When I choose to omit something, am I giving the interviewee space to represent themselves as they wanted to represent their perspective?

With qualitative data, each element removed comes at a cost. The more context or details about the subject that are removed, the more difficult the data would be for another researcher outside the original field site to analyze the final data set (Jerolmack and Murphy, 2019). Thus, I weighed each element of an interview transcript by (1) risk to the individual, (2) value to the context of the document, and (3) the structure of the data set overall, specifically whether there are opportunities to provide useful context about the data set as a whole that is less compromising to the individuals in the data set than direct quotes, while still providing useful field information to a new researcher.

In the larger project, we had two coders work with the data for our articles, so it seemed appropriate to have more than one person read through and mask the data as an added security precaution. Our masking efforts focused on removing direct and indirect identifiers (Kapiszewski and Karcher, 2021). When we first produced the transcripts, we had the transcribers sign NDAs as they listened to the audio files. We asked two undergraduate research assistants under NDAs to review and remove personal identifiers from the transcripts before I returned to the transcripts and removed any lingering

identifiers. Each undergrad research assistant on our team did the first read and removed data they considered sensitive or identifying.

This first pass generally removed the direct identifiers – proper names and sometimes names of towns/cities. I went back through each transcript for a second read and removed other indirect identifiers like landmarks or indicators of specific locations within a state (though geographic regions, like rural Connecticut, were allowed to remain) (Kirilova and Karcher, 2017). I also removed regionally specific businesses that could be used to identify location when the businesses were especially concentrated in a geographic point on a map, and any other unique case-related identifiers. For example: in cases where the individual held a series of jobs that included nationwide companies AND a local small business – I would abstract the small business by describing it by its function rather than its name. As another example, some respondents described going to a regional grocery store chain and I removed the name. I also removed information like how frequently the regional grocery store chain was available in their specific location, replacing it with an abstracted description of the business such as (common midwestern grocery store chain) to provide useful information without indicating an exact location. Overall, my strategy for masking my data for archival use reflects many of the recommendations made by Corti et al. (2000) given their experience with the UK's Qualidata archive.

After this standardized approach to cleaning and anonymizing the data, I returned to these two transcripts that presented issues because they contained discussions of more sensitive topics than I expected to gather given the scope of this project. I mentioned my concerns to my co-author: that these transcripts included references to drug use/recovery that we could argue were relevant to the study, but we could also argue were outside the scope of the study. In one case, the interviewee references drug activity to establish himself as an expert in identifying some patterns of behavior among passengers in his car. In the final interview transcript, we discovered that the audio quality was poor at that moment in time in the interview, so the words are hard to understand in the audio file unless you were part of the initial conversation. We did not need to omit them in the final script because the most specific details are caught as [unintelligible]. In the second transcript, the interviewee describes longer term addiction and his process toward recovery. The work he was doing at the time was a step in his recovery, which is why he wanted to share his background with me. We decided to keep the stories about his drug use, homelessness, and the limited resources he was able to access as an army veteran in the transcript because he was very clear throughout the interview that he was proud to participate in the study and he hoped his story could help others who found themselves struggling. My digital ethnographic work also revealed that these two cases of Uber drivers were unlikely to be easily identifiable based on their veteran status or past drug use because there are many drivers who were veterans and/or individuals recovering from substance dependencies. When we considered the sensitive data to be part of the context around when/why someone accepted this kind of gig work role, we could provide a better description of the drivers and their circumstances, which was a goal of this qualitative research. For these reasons, we kept the long descriptions about mental health, substance use and recovery, PTSD, and other conditions that the driver describes.

From these cases, my co-author and I had an interesting conversation about what it meant to participate in the study and how much of the consent process could be defined by us in advance vs.

what the interviewee wanted to contribute to answering our questions. Was there a point where I needed to reconsent the interviewee if I wanted to include all the data he wanted to share with the study, even if it felt out of the scope of our interview goals? I had already decided to drop the interview case of the interviewee who was lying about his experience as a gig worker. Should I omit sections of this interview because they were out of scope? Was this a step in what it meant to “clean data” for public consumption, in the ways a survey data researcher might omit noise from a data set? We decided to keep this section of the interview in the original transcript because a clear argument could be made that these detailed stories about his past life were part of his decision to drive with Uber at the time. They were also details that, while specific and possibly identifiable traits, are unfortunately common stories about the workers who come to rely on gig work platforms for employment when many of their financial and other living conditions are in flux (Auguste et al., 2022). We determined that the interviewee had repeatedly said he wanted students to know his story, and when I reminded him I could remove information from the final transcript, he said again that this story was important to him to share. While I had a goal for the kind of data I wanted to collect, I felt a responsibility to honor his request as the co-producer of his interview data.

8 Part five: publishing the data

We used existing infrastructure through our university to host our data, rather than develop our own infrastructure as a team or a department. Princeton created “an online repository designed for archiving and publicly disseminating digital objects which are the result of research, academic outputs, or administrative work performed by members of the Princeton University community.”⁷ There is a lot of debate about the kind of screening tools that should exist between a potential user and the data set as a form of security: many researchers recommend that potential users should register with the archive and declare how they intend to use the data (Bishop, 2005; DuBois et al., 2018). This may be controversial, but I will explain my choice: our platform does not require users to register themselves to access my interview data.

While there are many emerging data archive platforms, I chose this platform because I was familiar with the support my interview data would receive through my institution's library. Princeton's library system promotes open access and would continue to maintain the platform after I left the institution. This library, like many other academic libraries, has dedicated librarians who manage the data and help students find the resources they need for projects. Librarians are important partners in maintaining and distributing the data in our growing digital archives (Mannheimer et al., 2019). This institution, compared to a government-managed platform, was small enough for me to know who the data stewards were and to give them my input on how I wanted the data to be hosted and distributed. In this sense, the platform is a community effort within my university setting. The DataSpace team is also committed to making the data easy for students

and other researchers to discover, which was important to me. There were mutual benefits for me as a researcher publishing in an environment that provided me with a DOI link and for the university library to have a new data set that drew in a lot of public traffic. As some researchers mention, the broader public responds favorably to researchers providing data for public use in an accessible way (DuBois et al., 2018).

Given the small scale and local setting of our archive platform, I had frequent conversations with the DataSpace curators to ask for advice as I prepared my data. New items are reviewed and the packaging around these items is edited by a set of DataSpace curators as a team within the Princeton Research Data Service. These curators pay particular attention to the information included in the ReadMe files that accompany each item as one way the digital objects are introduced to new audiences. Beyond the packaging, these curators also reserve a DOI and provide guidelines on how to cite the materials in other contexts.

The ReadMe file became the introduction to the data set and our attempt at “Meta data” for the interview collection. We worked with the repository's curators to determine what kind of data was necessary to include in the ReadMe. This was largely informed by the other data sets on the website and the standards determined by the organization within Princeton that set up the repository. The repository is formatted such that each transcript is its own PDF that someone could download as an individual file. The ReadMe file offers general information about the “author” (me), a summary of the data including how many files there are and when it was gathered, how to cite the data, information on articles written using this data, and a codebook for how each file is labeled. I included details about the field site methods, including how the subjects were recruited and what criteria were used to select them in our screening process. Our interview guide was simple, given how brief each interview needed to be, so I included the list of structured questions at the bottom of this document rather than attaching a new file as “interview guide.” Finally, in this file, I explained the consent process and the anonymization process for each file before we published the data. Our intended audience was not a very specific category as we prepared the Meta data text. I assumed primarily students and/or departments teaching interview coding strategies might be the first to use the data. As this was my and my co-author's first experience with open qualitative data, we were curious to see where the data would go and had few specific expectations.

I realize my decision to have it in a public, non-restricted forum may seem controversial. Many researchers argue that the respondent's privacy is best protected by data enclaves and other platforms that require registry for use (Bishop, 2005; Field et al., 2021). First, it is the policy of the platform I chose to leave the door fully open and not require users to register themselves before they access data. I accepted this policy because there are already so many barriers between academia and others outside academia that I wanted to choose an environment that welcomes more casual exploration of my data without the users needing organizational credentials to access the transcripts. Sometimes the organizational credentials themselves feel too intimidating for someone to register, even if this line of identification is not required. I wanted to make sure people like my respondents could find and read their transcripts without feeling self-conscious about the space, even a digital one, they entered. I embrace the idea that analysis and discussion of the topics I study should happen outside purely academic research environments. Beyond my

⁷ Princeton Research Data Service. “About DataSpace.” Online repository, 2022. <https://researchdata.princeton.edu/research-lifecycle-guide/about-dataspace>.

own considerations, other researchers note that qualitative research is often meant to generate, rather than test, theories (Haven and Van Grootel, 2019; Field et al., 2021). Not every researcher who approaches my data will have a clear study that they intend to conduct using my data. My data was published during the COVID-19 shutdown, thus, some of the heightened interest in my data was probably due to the heightened demand for data that could be accessed remotely (Corti, 2000). Overall, I would like to normalize more exploratory research, especially when recent qualitative data sets like mine are accessible through archives.

It was important to me to find a platform where the data would be free for broader public use, thus, I was interested in pursuing a platform that used a Creative Commons license for free usage of my data so long as the user cited the original source of my data (Mannheimer et al., 2019). I selected the Creative Commons license because it was the format I was most familiar with – it is a system designed to encourage sharing and reuse. The fact that my data would be accessible to anyone, regardless of their education-affiliation or background, was appealing to my respondents, given their own varied backgrounds of education and socio-economic class. I worried more restrictive licenses that prevented redistribution, for example, would violate my promise to my respondents that their data would be broadly accessible. Beyond the requirement that the user cite the original data so others can find the data, I did not pick something more restrictive that would divert resources away from the platform to enforce it. The Creative Commons license allows them to see and use the data for free while making it easier for others to find their way back to my data archive.

9 Part six: emerging use cases of the published data

Before I published the data, I assumed the interviews would be useful to instructors in classrooms teaching their students about how to code data. Though the students were not in the field with me as I produced these data, they can practice “decontextualizing” the data and finding other ways to interpret my data against the other sources of information they have (Moore, 2006). My assumption was further supported by Bishop and Kuula-Luumi’s (2017) study of how qualitative data sets from the UK Qualidata repository are typically used; teaching and research were more common uses among graduate students and professors, while undergrads primarily downloaded the data set as an opportunity to apply what they were learning in class to a final project.

Another use case I expected was from other researchers who practice triangulation in their fieldwork: Qualitative researchers approach their work with different goals in mind based on their understanding of what their research offers. Those who are excited about my data may be supportive of the idea of contributing to general knowledge through collaborative and simultaneous projects. Others who conduct qualitative exploratory research before they attempt larger scale quantitative studies also expressed interest in my data and the process of publishing these data, even when my topic was outside the scope of their research. The final group interested in my decision and process to publish these interviews seemed to be those who follow their quantitative work with qualitative research (DuBois et al., 2018).

Others who are more skeptical of what my data could offer may be more focused on understanding a particular phenomenon in its context. Researchers who are more skeptical of the value of interview data may reject the value of these published interviews, arguing that they lack the deeper context that participant observation or ethnography provide (DuBois et al., 2018). Some researchers express concern that methods of analysis like grounded theory do not allow for the same structured hypothesis testing and review at the core of natural sciences (Glaser and Strauss, 1967; Corti, 2000). Again, I stress that the design of the overall data from the beginning matters greatly and that treating my data set as a case study in a moment of time/design of a specific technology makes it useful in some cases.

Now that the data have been publicly available for a year, the use cases I am aware of include: (1) use in universities’ classrooms to teach students about interview coding and analysis, (2) undergrads completing independent research projects using these data for their analysis, (3) public media stories about the conditions gig workers experience at work, and (4) community organizations using these data to corroborate issues they have identified within their own work experiences. In the first two cases, the data provided a more hands on experience for undergrads to engage with qualitative data. The second two cases were more specific to the year when I released my data and may be less relevant over time (especially in the case of embargoed data). Another case is likely to emerge in the future when historians return to historical data about gig work in the time before the drastic changes that occurred during Covid-19 and the battles over gig work classification.

The most active use case was within methods classrooms. As instructors added the data to their methods courses, they tweeted sections of the interview transcripts and credited me to describe their classroom discussions from earlier that day. Several of them referred to my data sharing and maintenance as “service to the field,” which may be one way to anticipate the additional stewardship that became necessary with the public data set. One example of this unexpected stewardship occurred when the hosting website was down for the day and several instructors sent me direct messages to my Twitter account to ask if I had intentionally hidden the data or if there was a technical issue with the website.

At this point, one of my articles using these data was published and the other was under review with a journal (Enriquez and Vertesi, 2021; Vertesi et al., 2021). I acknowledged the risk it could pose to me if I published my data before my second article was published. I decided, however, to respect the wishes of our funder rather than continue to embargo my data until an unclear final date. The instructor asked if I would share both of my articles using the data so she could walk her class through interview coding and theory building using my articles as the end case. At this point, I had to decide how to manage a public data set against the timeline of my other articles. I concluded it might be easier to publish public data AFTER I had finished using it for my own articles. In this case, we released it before my second article was published to maintain our agreement with our funder and because we did not feel that our article would be threatened by the release of our data.

I had the bonus experience of watching how my data was interpreted by UX researchers in technology companies – from my own time within these organizations, I know there was a lot of hesitation around reusing data. I would argue training in how to reuse and triangulate data is currently very limited, thus adding to the

overall suspicion around using existing data for current research questions. Some researchers are tackling the problem head on and expanding on the methodological literature around secondary data analysis. Kern and Mustasilta (2023) define secondary research analysis as using primary data generated for a different project to answer new substantive questions. Several researchers offer clear processes through real data on how one can work with secondary qualitative data effectively for a study: Bishop (2007) recommends the researcher begins by “understanding context, defining a subject area, finding data and sampling, later sampling and topic refinement, and relating to transcripts.” Bishop notes that an important part of contextualizing archival data is understanding who the researcher was and what that meant for the interaction between respondent and interviewer – and describes how this tricky relationship can be discussed in the analysis section of an article. This analytical statement is not a challenge reserved for only researchers conducting secondary analysis: Fielding (2004) argues that the interviewer and the researcher using the archival data are both tasked with describing the role and influence of the interviewer in the co-production of data. Chatfield (2020) recommends approaching the data flexibility and knowing some degree of mixed data or method may be necessary to answer the substantive question guiding the researcher through the data. Kern and Mustasilta (2023) describe how to reconcile different interview data sets as “cases” for comparison. Together, these articles provide enough for a methods lesson on using secondary analysis (?) that is valuable in a wide variety of contexts.

Throughout the year, I did my best to provide the additional context and resources as use cases for my public data set emerged – including many disclosures about myself as an interviewer. I saw this as part of my role as a steward of the data. The students and professors who wrote to me with questions about the data helped us think through the other kinds of meta data we might provide with our transcripts. These series of interactions painted a picture for me of what to expect in the maintenance of my data set but also in the ways my presence as something embedded in the data would continue to require my interaction with the researchers who chose to engage with my data set. A handful of college seniors emailed me directly to thank me for the data and sent along abstracts or outlines on how they might want to use the data for a project. Their follow up questions reflected researchers trying to add in the fieldwork context they knew was important as they read the interview transcripts I shared with them.

For example, one college senior wrote to me asking about the gender and general location of some specific speakers. She was studying gender and ideas about entrepreneurship, so she wanted to understand the social context of the speaker. Some of the undergraduates remained in touch for the entire year while they worked on their projects and provided regular updates on their analysis and progress. Very politely, one of them asked me if I would be willing to talk to her so she could have a little more context on my fieldwork and the tone of some of the interviews she read already. Another student asked me if I would share additional demographic information about the different speakers because, as the transcripts were written, it was hard to tell the race, gender, and general location of these individuals. After discussions with my co-author, I decided to share a table with each interview listed by gender and state. I omitted race, because while I had this data available on some of the interview subjects, there were others I could only guess from other context in our interview or from the profile pictures they used on whatever

electronic payment tool we used to transfer the compensation I promised them for participating in the study. The student who requested the gender and location data began a project focused on some of the political leanings expressed in the interviews, a topic very distant from the original intentions of my data collection and the articles I produced from these data. She believed these traits could be important to help her form her conclusions. Her questions to me in the following year introduced one of the clearest cases of someone using the data to answer a very different kind of question than the data was originally gathered to answer. With the support of her academic advisers, she found a way to work with my data to answer a very different kind of research question.

10 Part seven: conclusion: lessons and future considerations

I still believe that publishing my interview transcripts required both (1) upfront planning and (2) post-publishing adjustments as use cases for the data emerge. Ideally, by sharing my planning process and the cases I encountered after I published my data, other social scientists will have more information available to them as they plan their own public data releases. In summary, before I gathered data, I developed a clear sense of what kind of data I wanted and weighed the risks to the subjects of gathering this data with the intention of making it public. While I did not consult the DataSpace curators in the overall design of my project, it may be practical for other researchers to discuss the study design with an archive's managers early on. Particularly, the librarians and others involved with maintaining an archive and distributing data may have useful insights on what works/does not work with an open data collection (Mannheimer et al., 2019).

During the fieldwork, I focused on clear consent protocols and did my best to keep the scope of the interviews within the guidelines of the discussion guide, knowing the discussion guide was where I had developed my risk assessments. These ethical debates around informed consent are important, and I agree that not all data should be added to a public archive – especially without a conversation about consent with the respondent. I disagree, however, with claims that informed consent is impossible and that subjects cannot comprehend what it means to have their interview published. While I was initially worried it may turn respondents away, it turned out to be a useful tool for recruiting respondents.

My experiences in gathering consent and publishing the interview transcripts lead me to believe it may be necessary to ask more respondents what they would like to have happen with their data beyond my academic outputs. I think there are interesting questions to consider about whether researchers should need to make their data public if the respondent requests it, given the respondent helped create the data. Does the respondent have a right to request the publication of their full interview? Afterall, the benefit of an academic article to the respondent is often minimal, but it may be seen as a public service or good if their interview can then be used in classrooms or to inform government policy. For some respondents, it may also be useful to have the full interview context available if they feel they were misrepresented by a quote taken out of context in an academic article.

Once the data is added to an archive, we have other important design decisions to consider. I want to voice my agreement with

several qualitative scholars who argue that while we should have ethical standards and design best practices in preserving and publishing some aspects of archival data, flexibility for the researcher is very necessary (Bishop, 2005; Jacobs et al., 2021). Recent qualitative data archives present an opportunity for exploratory and comparative research – something desperately needed for early career scholars, remote working scholars, and historical scholars. I support the efforts to control sensitive data, but there should also be space for more general access to less sensitive data. The risk around publishing data is on a spectrum and I am interested in continuing to explore what is possible through more public exchange of appropriate kinds of qualitative data.

In retrospect, I could have designed policies before I published the data on how involved with the distribution and explanation of the data I would be after I published it. I decided to participate very actively and answer all the questions that came my way because I saw this was an interesting opportunity to observe what happens to the data in its second life. I also saw my presence in and around the data as a necessary piece of context. I am always excited to collaborate with other social scientists, so I embraced this ongoing experiment as an opportunity to learn about unplanned collaboration.

Finally, if the co-production of the primary data occurs between the interviewer and the respondent, I would argue a much larger team is involved in producing the secondary archival data. Each team member (in our case, the interviewer, respondent, transcriber, markers, archive curator, archive editor, and librarians who serve as on-going stewards for the data) offers an important skill and knowledge that is a valuable part of the data preservation, and their needs should be considered and negotiated into the final secondary product with the privacy/wishes of the respondent serving as the priority consideration.

As we move forward with more collaborative social science programs, the issue of diversity within our conversations becomes increasingly important. Though we might be able to rely on multiple coders to capture varied interpretations of fact with a team, when our team offers similar perspectives on the field site and the data, we only capture some of the diversity necessary to understand the scope of the field. Some qualitative researchers already develop collaborative relationships with individuals deeply enmeshed in their field sites and ask for their opinions on the researcher's analysis from the field (Duneier, 1999). However, we often draw our collaborators from our social networks, which may mean our overlapping similarities still cause us to miss important context within unfamiliar environments. For example, the debates and expectations around patterns of clear responses and direct communication emphasized in the US may not be an option or social norm in other environments. Reaching beyond our existing networks may help us develop more interesting conclusions than we can see from our current dominant perspectives. For example: styles of partially direct, partially indirect dialogue are very common in Mexico today in discussions of local gang/cartel violence, but they are also visible in the ways Eastern Europeans in the USSR described communication patterns, how members of organized crime units signal membership, and how many artists engage in public discussions of difficult politics without directly drawing the attention of those they critique (Gambetta, 2009). Information that may seem minor to an outsider may be deeply meaningful and relevant to someone with a deeper knowledge of the context. Maybe the best way

to include them in our research process is to invite them to engage with more of our data and not just our analyses. There are many other places where the social norms of other cultures may help us improve our interpretations of data and sensemaking.

In summary: I am eager to see how other scholars design their studies with the intent to create public qualitative data sets. I am eager to see more literature reviews that blend the analysis of scholars with the analysis of respondents in existing interviews. I encourage and embrace decontextualized and triangulated interpretations of my interview data – especially because each student I met through my public data this past year taught me new ways to understand where my research could go next.

Data availability statement

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found at: <https://doi.org/10.34770/4324-yn77>.

Ethics statement

The studies involving humans were approved by Princeton University Institutional Review Board. The studies were conducted in accordance with the local legislation and institutional requirements. The participants provided their written informed consent to participate in this study.

Author contributions

DE conducted the fieldwork and analyzed the data for the original publication, prepared the transcripts for publication, and published them through the Princeton Data Space. She is the main point of contact for individuals working with the data who have questions. JV was the PI on the project and coded the interview transcripts for the original publications drafted using this data. JV also hired different undergraduate research assistants to help anonymize the data before DE took a final read through the transcripts and published them. JV was consulted on a final copy of this article before DE submitted it for review.

Funding

This project and the other articles associated with this data set were funded by the Sloan Foundation.

Acknowledgments

I would like to thank the original project's co-PIs, Janet Vertesi and Adam Goldstein, for their guidance and support on the fieldwork I completed and the articles that emerged from this project. Thank you also to Shay O'Brien and Alex Kindel for reading drafts of this work in its early stages and helping me refine it. Thank you to

historians Mikey McGovern, Kathryn Carpenter, and Fabian Krautwald for talking through their methods of working with oral histories with me. Finally, thank you to Colin Jerolmack for following my experiment since I first published the data and encouraging me to submit an article for this special issue and to his co-editors Alexandra Murphy and Victoria Reyes for your guidance and support through the revisions of this article.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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Supplementary material

The Supplementary material for this article can be found online at: <https://www.frontiersin.org/articles/10.3389/fsoc.2024.1157514/full#supplementary-material>

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