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Letter from the Editors

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Welcome to our second issue as QMMR editors! Over the past year, we've been constantly looking out for potential contributions and assessing submissions. This gave us the opportunity to read cutting-edge work and incisive reflections about the practice of qualitative and mixed-methods research. We are grateful for this opportunity to learn from and serve our extraordinary community.

In our previous letter, we stated that our goal was to make QMMR a more diverse venue where junior and senior scholars come together to share methodological developments and experiences as practitioners and teachers of qualitative and mixed-methods research. This goal is also at the heart of our Section. We hope readers will agree that the current issue lives up to this goal. We feature original research and reflections on practice and teaching from Ph.D. students, early career researchers, and seasoned scholars who represent different epistemological traditions and social science disciplines.

A theme that runs through the different contributions is the importance of *training* political scientists in qualitative methods and increasing overall levels of "qualitative literacy" across the social sciences. "Literacy" implies both adhering to high standards when conducting qualitative research and being able to assess the quality of qualitative research with the right standards. This issue explores both dimensions.

In the following pages, you will find one original article, one book symposium, one Note from the Field, and one Note from the Classroom.

The article by Kate Cronin-Furman and Stephanie Schwartz draws on an original survey to highlight the need for more standardized and better field research training in political science – a discipline that (at least in some subfields) emphasizes fieldwork as a source of academic credibility and thus incentivizes graduate student to abandon their desks. Identifying the perils of ad-hoc solutions, which reflect and further existing inequalities within our discipline, the authors argue that formal fieldwork training should be incorporated into graduate methodology sequences. While their data comes from US-based political scientists, the value of their reflections and recommendations has no geographical boundaries.

The book symposium shifts attention away from issues related to how to conduct field research to a supremely important question: how should we evaluate the quality of ethnographic and interview-based research? We invited three scholars, two political scientists and a political anthropologist, to comment on a book recently published by two prominent sociologists: Mario Luis Small and Jessica McCrory Calarco's *Qualitative Literacy. A Guide to Evaluating Ethnographic and Interview Research* (University of California Press, 2022). In their essays, Hilde van Meegdenburg, Louisa Lombard, and Santiago Anria praise the book for its timely contribution to the methodological debate and its attempt to set standards for scholars from all walks of life who are faced with the need to evaluate qualitative work (think journal reviews, grant proposal evaluations, etc.). Our contributors also raise a series of critical points that sometimes add to the book's core argument, but others warn about potential negative externalities of moving in the direction the authors propose. The symposium closes with a response by Mario and Jessica.

In our previous issue, we inaugurated two new sections, *Notes for the Field* and *Notes from the Classroom*, intending to draw practitioners and teachers into the QMMR

community and thus serve a broader audience. We didn't know how our readership would receive these sections. We are excited to report that these sections, particularly *Notes from the Field*, have attracted a lot of interest, especially among graduate students. Because the response has been more positive than expected, many excellent contributions are already lined up for future issues.

In this issue's *Note from the Field*, Maureen Fubara, a graduate student from the University of Amsterdam, shares important reflections from her recent multi-site fieldwork in Nigeria, where she is studying issues related to election violence. Her *Note* deals with the challenge of changing positionalities and how to manage multiple identities in the field: being a woman, coming from a foreign university, being Nigerian but from one specific area of the country, and speaking a certain language or dialect.

In our *Note from the Classroom,* Giovanni Capoccia shares with our readers his 15 years of experience teaching a course on case study research at Oxford. The *Note* explains the ins and outs of his hands-on approach to teaching case studies. Over a period of four weeks, Giovanni's students dissect and reconstruct exemplary research published in top-tier journals with the goal of gaining insights into the methodological issues that case study practitioners often face, including the relationship between case and theory; research design and case selection; process-tracing and within-case analysis; and use of primary evidence and secondary sources.

These workshop-like method courses, which seek to understand other researchers' choices and challenges by taking advantage of published work, can complement normal lectures in methodological sequences. This approach is very much in line with the recommendations offered by Kate and Stephanie regarding field research: instead of relying on trial-and-error or drawing on the experience of a supervisor or a peer's advice, students should receive practice-oriented training in their graduate programs. When there is no time to have students *do* qualitative research as part of a methods class, having them dissect exemplary applications of these methods seems to be a productive way forward.

Once again, before we let readers dig into this rich material, we encourage the QMMR community to continue submitting original articles, symposia, and notes from the field and classroom for our consideration. Articles and symposia will be typically peer-reviewed, whereas we will review notes in-house. You can find details about submission guidelines on our website: https://www.qmmrpublication.com

Until the next issue,
Ezequiel Gonzalez-Ocantos, University of Oxford
Juan Masullo J., Leiden Unviersity

QMMR Qualitative & Multi-Method Research

Original Article

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Ill-Prepared: International Fieldwork Methods

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Introduction

The current culture in American political science incentivizes scholars to conduct research abroad, particularly in volatile contexts and increasingly through social experimentation (Mitchell 2013; Humphreys 2015; Desposato 2016; Driscoll and Schuster 2018; Eck and Cohen 2021). These practices reflect the discipline's emphasis on "fieldwork" as a source of academic credibility, particularly for scholars who study violence, international development, and related topics. Ethical and safety issues involved in all types of human subjects research—whether qualitative or quantitative, observational, participatory or experimental—are compounded in fragile contexts. Yet training for political scientists conducting research abroad remains piecemeal and unstandardized.

In practice, this means many graduate students often enter the field feeling anxious and unprepared, with limited advance consideration of the types of ethical issues likely to emerge through their work or how to manage them as they arise. At best, successive cohorts of junior scholars perpetually reinvent the wheel as they learn on their own through trial and error and peer advice how to best manage their safety and the safety of others while conducting research in volatile areas. At worst, they flounder in the face of challenges encountered in the field, risking their own safety, and causing harm to research participants.

Research ethics and the practicalities of conducting field research are increasingly subjects of discussion within the discipline, prompting conference panels, working groups, and publications (see e.g., Wood 2006; Thomson, Ansoms, and Murison 2012; Malejacq and Mukhopadhyay 2016; Kapiszewski, MacLean, and Read 2015; Campbell 2017; Bond 2018; Cronin-Furman and Lake 2018; Thaler 2019; Curtis 2019; Knott 2019; Ansoms, Bisoka, and Thomson 2021; Parkinson 2021; Kapiszewski and Wood 2022). However, for individual graduate students, awareness of and access to these conversations is network-dependent, and advance preparation for fieldwork largely depends on the goodwill (and extent of relevant experience) of mentors within their home departments.

In this paper, we present descriptive results from a targeted survey of international relations (IR) and comparative politics (CP) faculty and PhD students on their attitudes towards, and preparation for, international field research. Our results demonstrate a discipline-wide reliance on ad hoc solutions for field research training that both reflects and furthers inequalities within the discipline. Drawing on these findings, we argue that formal fieldwork training and research design should be incorporated into graduate methodology sequences.

The unequal distribution of informal training and resources perpetuates existing advantages for those at elite institutions. Moreover, because the invisible labor of mentoring in fieldwork methods often falls on women, the reliance on informal training perpetuates gender inequalities along the tenure-track as women scholars dedicate time to service not recognized in tenure files. We argue, therefore, that formal fieldwork training and research design should be incorporated into graduate methodology sequences. Treating fieldwork preparation as methodology will improve individual scholars' experiences and research and have distributional benefits through promoting consistency in access to training and valuing the work that goes into providing it.

The Survey

This article draws on data from a targeted online survey of 292[1] US-based political scientists conducted in July and August 2018 to explore two research questions:

- To what extent do political scientists believe international fieldwork is critical for career success?

1 We received 227 completed surveys and 65 partially completed. We include partially completed answers in the analysis below.

- What training do political scientists receive before conducting international fieldwork?

We chose the sample with the goal of collecting reflections on these questions from scholars at institutions that would reasonably be expected to have the best resources to prepare graduate students for conducting research abroad. If scholars at departments with the largest number of students able to undertake costly fieldwork trips do not feel there is adequate training on conducting research abroad, we can plausibly assert that there is a dearth of formal training. If these scholars also feel that the discipline values and rewards conducting such research despite the lack of training on how to do so safely and ethically, we are comfortable concluding that there is an ethical problem in the discipline.

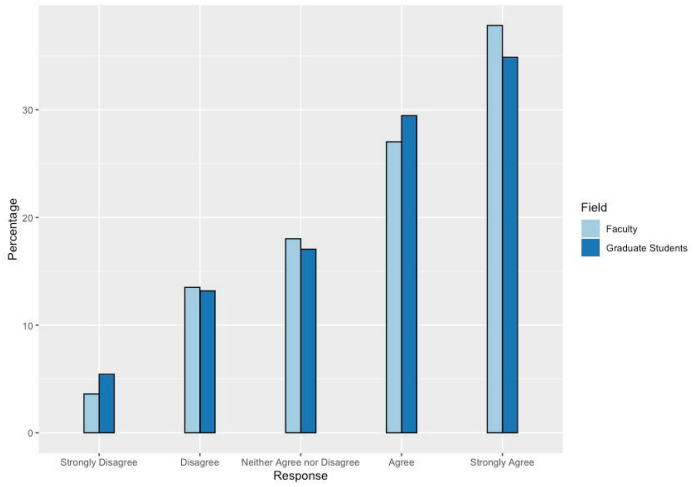
We recruited participants through posts on American Political Science Association section message boards, Facebook, and Twitter, and through email invitations to faculty and graduate students and department listservs at the top-ten-ranked US programs in CP and IR (totaling 15 universities).[2] Our respondents comprise 174 current graduate students, 108 faculty members and postdocs, and 10 researchers working outside of academic hierarchies. Roughly two thirds (68%) were affiliated with the top ranked IR and CP departments, and one third (32%) from other institutions. The majority of respondents identified their primary specialty as either CP (64%) or IR (26%). Three quarters (75%) of the respondents had already conducted international fieldwork. An additional 7% planned to conduct international fieldwork in the future but had not yet done so. Forty nine percent of respondents identified as women, 45% as men, 1% as other and 5% gave no response.

Importance of Fieldwork for a Career in Political Science

From the subset of respondents who identified as faculty, 80% agreed or strongly agreed that conducting fieldwork enhanced their own academic career, while 65% felt that conducting international fieldwork is necessary for scholars in their field. Similar patterns existed among PhD students: 78% believed conducting international fieldwork would enhance their academic career prospects. 65% believed conducting international fieldwork is necessary for scholars who study their topics of interest.

Among scholars studying violence, civil war, and peacebuilding, 87% of faculty and 85% of PhD students felt that conducting international fieldwork was a career asset. And across all specialties, 71% of faculty believed conducting international fieldwork enhances career prospects for their students who study topics like violence, peacebuilding, and human rights.

Figure 1: Importance of Conducting International Fieldwork in Respondent's Field



We expected that scholars who study civil war, violence, peacebuilding, or similar topics would get a reputational boost from conducting fieldwork in volatile environments. And, indeed, 76% of faculty who study these topics agreed or strongly agreed that fieldwork in "dangerous" contexts earns credibility.

When we asked our survey respondents to consider the importance of conducting field research for career success among a list of qualifications relevant to academic job market performance, "substantial international fieldwork" did not rank as highly as publications or quantitative analysis skills. However, a majority of respondents (71%) rated substantial international fieldwork as either moderately or extremely important for advancing the careers of scholars in their specialty, while even more (86%) rated it as either moderately or extremely important for scholars of violence, civil war and similar topics.

2 We used the US News and World Report rankings (2017) of the top ten programs in Comparative Politics and International Relations. The institutions were: Harvard University; Stanford University; Princeton University; University of California, Berkeley; University of Michigan; Yale University; Columbia University; Massachusetts Institute of Technology; University of California, San Diego; University of North Carolina; Duke University; New York University; University of California Los Angeles; University of Chicago; Ohio State University; and the University of Wisconsin Madison (US News & World Report 2017).

Figure 2: Importance of International Fieldwork to Enhance Career in Respondent's Field

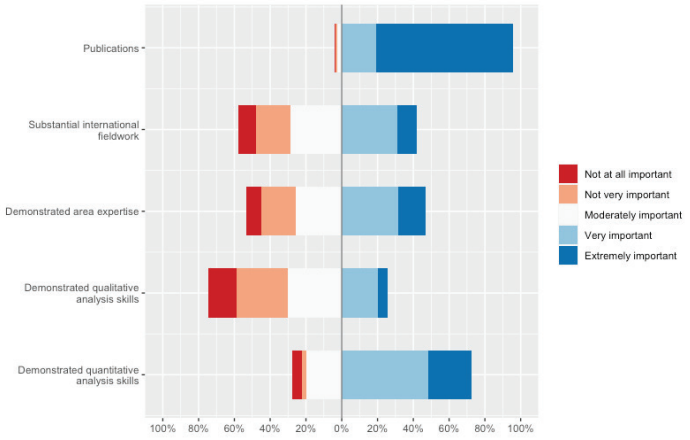
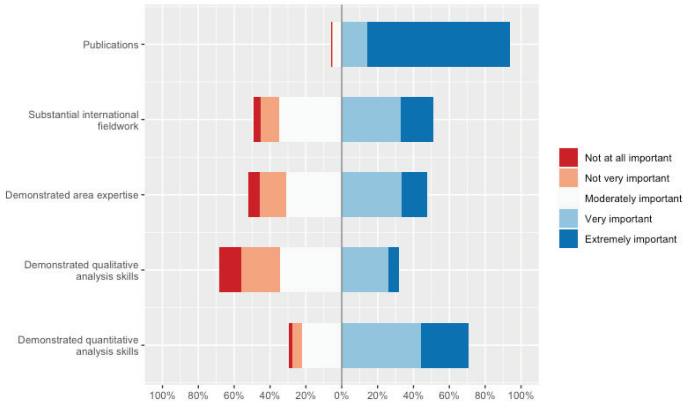


Figure 3: Importance of International Fieldwork to Enhance Career; Conflict Studies



Prevalence of Field Research

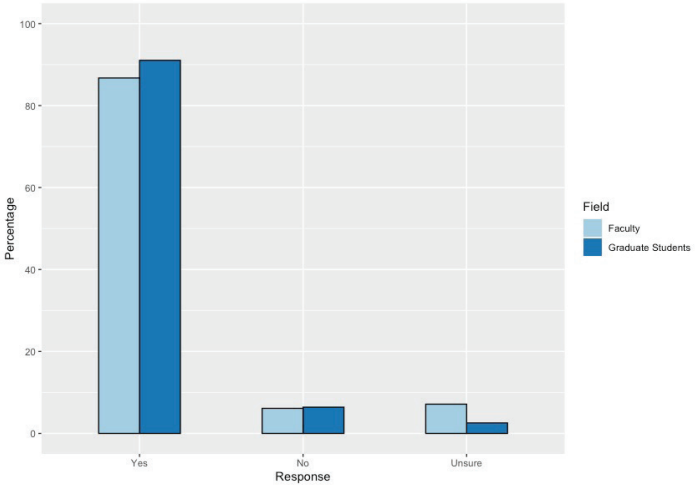
Conducting human subjects research abroad is common in political science generally and in the conflict studies sub-field specifically. For example, between 2014 and 2019, 36% of articles published in the *American Political Science Review,* 39% of articles in *World Politics*, 25% of articles in *International Security*, 23% of articles in *Comparative Political Studies* and 16% of articles in the *Journal of Conflict Resolution* relied on human subjects data gathered abroad.[3] Among scholars in our sample, international fieldwork was most common among scholars of comparative politics, with 80% of faculty and 72% of PhD students reporting they had conducted international fieldwork. In international relations, 74% of faculty and 37% of PhD students had conducted fieldwork (an additional 23% of IR PhD students said they planned to in future).

A significant proportion of our respondents (50% of faculty and 48% of graduate students) had conducted field research in volatile environments. Unsurprisingly, this was most common among those who specialize in topics like violence, civil war, and peacebuilding, with 67% of faculty and 73% of graduate students in these subfields reporting they had conducted fieldwork in an unstable or conflict affected environment.

Training Received

Despite the perceived importance of conducting international fieldwork, the majority of survey respondents (66% of faculty and 62% of grad students) reported that they had been given no formal training on how to do it. Only a third of graduate students and 24% of faculty reported that their department offered a fieldwork methods course as part of their curriculum.[4]

Figure 4: Formal Fieldwork Training



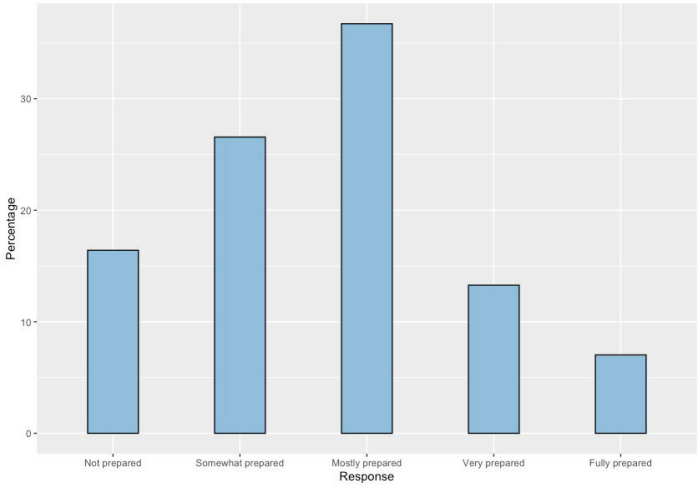
Perceptions of Preparedness

Only 20% of the graduate students in our sample reported feeling "very" or "fully" prepared to conduct international fieldwork—even if they had already done so.

3 These data were compiled by Stephanie Schwartz and Sarah Cueva Egan and includes articles in the above journals published between 2014 and 2019, totaling 1318 articles. Articles are coded as relying on international field research if there was a research intervention in an international environment (outside the author's home institution's country) with human subjects.

4 Of the fifteen top-ranked departments surveyed, four offer a formal course at the graduate level on conducting fieldwork. However, only two of these programs offer this course regularly. The other two programs have only offered the fieldwork training course two or three times in the past ten years.

Figure 5: Graduate Students Perception of Preparedness for International Fieldwork



Few respondents believed that their departments prepare scholars to safely conduct fieldwork.[5] Moreover, 46% percent of faculty respondents reported that they or their advisees had encountered safety issues while conducting fieldwork, with a higher incidence of reported safety issues among female faculty (58%) than male (31%). Roughly half of the graduate student respondents who had conducted some fieldwork reported they had experienced a safety issue in the field, again with slightly higher rates among women (54%) than men (47%).

Figure 6: Faculty or Advisee Encountered Safety Issues

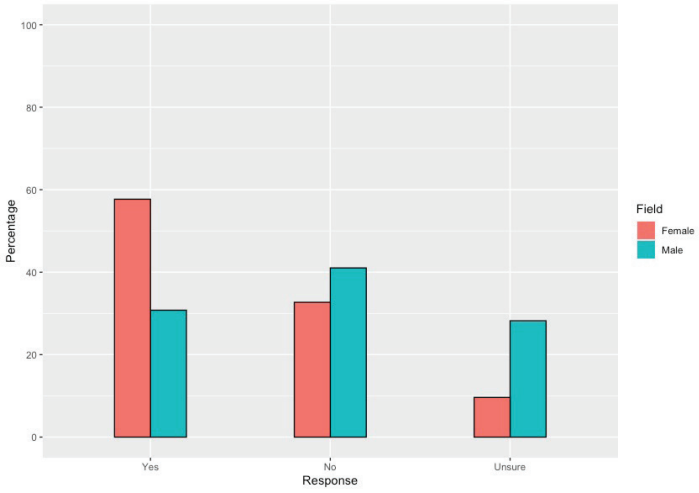


Figure 7: Felt Unsafe Conducting International Fieldwork; Graduate Students

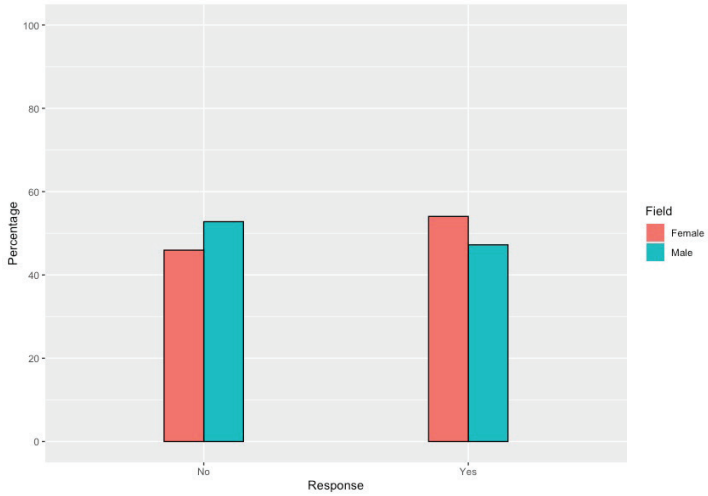
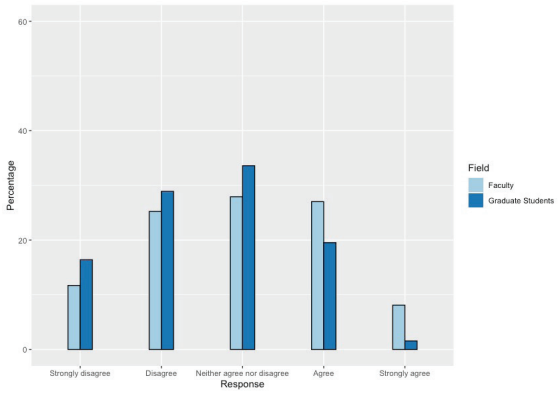


Figure 8: PhD Program Prepares Scholars to Safely Conduct Fieldwork



Compensating for the Absence of Training

In the absence of formal training, nearly all respondents (over 90%) reported that they had used informal mechanisms to learn how to conduct fieldwork. The most common informal mechanism cited was peer-to-peer mentorship. For example, one PhD student respondent stated that she or he "just asked for advice from tons of friends." Both PhD students and faculty mentioned reaching out to colleagues, journalists, or area experts, and drawing on professional experiences prior to graduate school.

Some respondents (more faculty than students) mentioned consulting academic scholarship, but many emphasized that most of their fieldwork know-how had been picked up on the fly. As one faculty member put it, "I have talked to colleagues that have done field work in the same country. Their experiences helped me prepare for my work. That being said, I mostly learned on my own by doing it."

5 Only 21.7% of graduate students who had conducted or planned to conduct international fieldwork, and 35% of faculty respondents reported this.

The Costs of the Absence of Formal Training

This lack of formal training has serious consequences for the discipline. Most obviously, it implicates individual researchers' safety and wellbeing. But a discipline-wide norm of sending underprepared researchers to conflict-affected environments also poses systematic risks to research subjects, undermines the quality of research produced (see, for example, Bell-Martin and Marston Jr. 2021), and contributes to existing inequalities within the discipline. We discuss each of these issues in turn below.

Researcher Wellbeing

The absence of standardized training for fieldwork contributes to a practice of treating the logistics of field research as something scholars should figure out on their own without guidance. Nearly three-quarters (72.6%) of our graduate student respondents who had conducted fieldwork reported that their advisor had not asked them what kind of health and safety measures they had put in place.

Once in the field, 76% of our graduate student respondents who had felt unsafe did not reach out to their advisors to discuss these safety issues. Several cited embarrassment, insecurity in their relationship with their advisor, or the potential for reputation costs as obstacles to seeking advice. In the words of one: "Honestly, I feel that I have an incentive to protect my reputation as a resourceful and independent scholar and reaching out to my advisor for minor safety concerns could damage that reputation." Even more troublingly, another reported that they *had* contacted their advisor about a safety issue but would not do so in the future, "because my concerns were dismissed as over-reactions."

Our survey responses suggest that junior scholars tend to engage their advisors only on what they see as "substantive" issues in which their advisors have specific expertise. Of the respondents who had reached out to their advisors on safety issues, or said they would be willing to, many indicated their willingness was related to their advisor's regional expertise or experience conducting fieldwork. On the other hand, many of the respondents who did not report safety issues to their advisors, or said they would not do so in future, said that their advisors lacked expertise in conducting fieldwork, were not familiar with the local context, or simply could not help from afar.

The survey also suggests that graduate students felt reluctant to engage their advisors on issues related to safety based on a perception that safety issues are separate from substantive issues. For example, one respondent indicated they would consider reaching out to their advisor, "depend[ing] on the nature of the concerns—if not directly related to my academic work, I would likely contact someone else." Another respondent noted that "practical and ethical issues felt like personal problems." In many cases, respondents indicated they felt it was inappropriate to consult their advisors when there was a general sense of insecurity, but not a specific issue in which they felt they were in danger, or which could "directly" affect their research. For example, one respondent who conducts research in countries with high levels of criminal violence reported having been in situations where they were susceptible to hijacking, where small bombs had been detonated near their field residence, and where they had been followed by an individual seeking money. However, this respondent felt that "none of these incidents directly impacted my work." They did not report the incidents to their advisors because they "didn't seem professionally relevant."

The absence of a routinized channel for considering health and safety risks reinforces a culture in which these issues are not anticipated or prepared for in advance, scholars feel unsupported when they do face them, and may understandably draw the conclusion that cavalier attitudes towards their own security will be rewarded (Douglas-Jones et al. 2020). It is worth noting that we only asked our respondents about their experience of physical health and safety issues. Emotional wellbeing problems are common among researchers, humanitarian and aid workers, and journalists who operate volatile contexts (Hummel and El Kurd 2021; Markowitz 2021; Young 2015). The stigma of discussing mental health issues is likely to leave researchers even more isolated dealing with these concerns.

Ethical Consequences of the Lack of Preparedness

Field research conducted in fragile and violent contexts also raises complicated ethical questions around protecting research subjects and partners from risks incurred through participation (see e.g., Goodhand 2000; Brewer 2016; Campbell 2017; Cronin-Furman and Lake 2018; Shesterinina 2019). IRBs and similar institutional ethical review bodies are not designed for social science research and often make incorrect assumptions about the sources of risk to human subjects during field research (Guillemin and Gillam 2004; Blee and Currier 2011; Fujii 2012; Yanow and Schwartz-Shea 2016). IRB review processes may therefore steer students towards fixating on subjects of IRB concern—namely, procedural ethics—rather than considering the true ethical implications of their projects. Fewer than half of the PhD students we surveyed (47%), and even fewer faculty (39%) agreed or strongly agreed that their department's PhD program prepared scholars to consider the ethical issues that may

arise during fieldwork.

In their comments, respondents noted that they felt the discipline explicitly deprioritized ethical considerations when evaluating field research or training scholars in research methods. As one person explained: "[I]t's hard not to consider job market aspects when deciding to pass on data that might be unethical or dangerous to get." Another respondent commented: "Ethics are emphasized in qualitative methods courses, but not in quantitative courses, even though quantitative methods require ethics choices."

Yet practically speaking, security, ethics, and methodological rigor are not separable. Without an understanding of the ethical and security risks at play in the field, researchers cannot guard against or analyze biases in data collected. For example, if researchers do not know why it is unsafe for respondents to be seen speaking with outsiders, they will not be able to interpret non-response or social desirability biases in their data. Discussion of potential risks to the researcher and research subjects is therefore essential to substantive design, analysis, and knowledge produced as well as participant protection.

Inequalities Within the Discipline

Our survey revealed two dynamics resulting from the ad hoc approach to fieldwork training that reinforce structural inequalities within the discipline. First, the ability to access training resources tracks existing privilege. Without access to formal training within regular curricula, most junior scholars rely on their immediate peer circles for guidance on how to conduct international fieldwork. Because of existing biases in the discipline, elite institutions tend to have a larger pool of junior faculty and graduate students who regularly obtain external funding for and conduct international fieldwork. As such, informal mentoring is often insulated within networks at these institutions. Elite institutions also tend to provide more opportunities to access formal training through summer programs, which require tuition or fees, or through research assistantships in the field.

Second, the weight of informally advising students who lack formal fieldwork training appears to be falling disproportionately on female faculty.[6] These results mirror our own observations that women are not only more likely to commit time to preparing for their own safety in the field,[7] but often take on the additional time burden of supporting their students and female colleagues when they go abroad. In fact, 28% of our male faculty respondents were unsure if they or their students had encountered safety issues while conducting international fieldwork compared to only 9.6% of women faculty. This suggests that women at the faculty level are investing more time discussing graduate students' fieldwork experiences. This may be driving the disparity between female faculty and male faculty's perceptions of the insufficiencies in training; only 29% of female faculty believe that their departments adequately prepare students to safely conduct fieldwork, compared to 42% of their male counterparts.

How to Formalize Fieldwork Methods Training

Treating the logistics, ethics, and safety considerations implicit in international field research as methodological concerns would go a long way towards remedying these issues. In quantitative methods training, seminar reading, problem sets, and replication exercises are all designed to lead graduate students through the process of first evaluating and critiquing, then performing, and finally adapting the research methodologies under study. Likewise, scholars should not be expected to develop effective, safe, and ethical field research designs without training.

Formalizing methods training in international fieldwork would provide both an opportunity to communicate current best practices to students as well as a venue for the open discussion of mistakes. Moreover, the recognition of field research as a method would grant legitimacy to these subjects, helping to dispel the impression that topics like safety and logistics in the field are tangential to substantive research. Acknowledging that conducting fieldwork is a learned, never really perfected skill that requires updating and refining (just like any other methodological approach) should help junior scholars to feel more comfortable asking the questions necessary to keep themselves and their research participants safe.

Over-reliance on informal training can be dangerous when unethical or dangerous practices are reproduced and best-practices are learned by trial and error in high stakes environments, rather than in the classroom (Wood 2009). Providing the opportunity to discuss common mistakes through formalized methods classes could help scholars avoid some of the most common issues, rather than repeating them. It would also enable conversations about when and why it may be prudent to choose *not* to conduct fieldwork in a certain area, or to pause fieldwork efforts, alleviating some of the pressure to conduct

6 We anticipate that this dynamic is not only gendered but racialized, tracking general trends regarding who is doing the work of informal advising in the academy, but we did not collect demographic data aside from respondents' gender identity.

7 This is clear in the survey results: When asked if they use a formal or informal mechanism to check in with someone about their safety while in the field 74% of female faculty answered yes, compared to 43% of male faculty.

fieldwork, particularly for scholars of violence. Similarly, formalizing classroom discussions of best fieldwork practices would offer an opportunity to acknowledge how fieldwork can affect researchers' mental health and reduce the stigma of seeking appropriate support upon return.

Moreover, formalizing field research methods training could combat the perpetuation of inequalities. The survey provides preliminary evidence that female scholars are providing the majority of support for scholars preparing to conduct fieldwork, and that support is largely happening in informal environments. This work remains largely invisible and unrewarded and can take time away from the research necessary for advancing in the field. Formalizing methods training would render this work visible and provide an opportunity for these scholars to incorporate it into their regular teaching and mentoring duties.

The need for specialized information on the considerations for conducting research in specific contexts does not contravene the recommendation to formalize international fieldwork methods training. In fact, the practice of engaging external networks to develop context-specific knowledge is itself a skill that can be taught, practiced, and critiqued.

A more formalized approach to training could be implemented via multiple avenues. Many PhD programs already sponsor junior scholars to attend additional summer training on specialized methodologies or interest areas.[8] In the same vein, they can set aside dedicated funding to sponsor students interested in conducting fieldwork in conflict-affected contexts to attend summer training courses focused on safety and ethics, like the annual Advancing Research on Conflict (ARC) summer program, or Hostile Environment and First Aid Training (HEFAT) courses like the ones which many NGOs and journalistic outlets require their staff to attend before traveling to volatile environments.

However, summer programs may have limited availability or attendance may be impractical for many students. We therefore think it is important that PhD programs move towards offering courses on fieldwork design and practice as a part of their standard methods sequences. Dedicated field research courses can offer students the space to think about ethical issues beyond the IRB and to practice skills like getting informed consent, training enumerators, interviewing, and observation in environments that are designed to be low stakes. Where such additions to the curriculum take time to implement or are otherwise infeasible, research design courses that already exist can be updated to emphasize logistical and safety considerations more strongly and to integrate scholarship on field research ethics into their syllabi. This will better prepare junior scholars to understand research ethics as an integral component of methodological rigor.

These changes to the discipline, however, are unlikely to happen unless there is an incentive for scholars to invest in these skills. In a system that evaluates research quality separately from the ethics of the approach, there is little external motivation for researchers to develop their skillset regarding ethical and safe design. Instituting an expectation that peer review of journal articles and academic press manuscript submissions consider the ethical design of the research in addition to the theoretical and empirical contributions would go a long way towards shifting these incentive structures (Cronin-Furman and Lake 2018; Jacobs et al. 2019; MacLean et al. 2019). The logistics and ethics of research interventions are directly linked to the quality of the knowledge produced (Malejacq and Mukhopadhyay 2016; Parkinson 2021) and should inform our understanding of rigor in our evaluations of others' research as well as in our training of graduate students.

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8 Examples include the Inter-university Consortium for Political and Social Research (ICSPR) Summer Program in Quantitative Methods, the Institute for Qualitative and Multi-methods Research (IQMR), and the Summer Workshop on the Analysis of Military Operations and Strategy (SWAMOS).

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QMMR Qualitative & Multi-Method Research

Book Symposium:

Small, Mario L. and Jessica M. Calarco. 2022. Qualitative Literacy: *A Guide to Evaluating Ethnographic and Interview Research*. Oakland: University of California Press.

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Qualitative Literacy

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"This is anthropology," a senior political scientist told me years ago. I had just presented a work-in-progress using interview data in the context of a graduate seminar—a version of what people now call the "job market paper." "Is it at least *good* anthropology?" I asked. "I would not know how to tell—as you know, I'm more drawn to quantitative analyses. But it's well written."

Though at the time those words were painful to hear (and hence I memorized the exchange), they also revealed a real problem. Few guidelines exist for discerning good versus bad qualitative work—between empirically sound designs (and executions of empirical strategies) and nicely-written works.[1] *Qualitative Literacy*, an excellent and much-needed book by Mario Luis Small and Jessica McCrory Calarco, aims to narrow this literacy gap and reduce uncertainty in how to evaluate the quality of interview-based and ethnographic studies. The book is narrow in scope, but ambitious in its larger aim. Small and Calarco want to generate a set of metrics for assessing the quality of data collection and the reporting of data collected—or co-created—through in-depth interviews and participatory observation. They want to persuade practitioners and, perhaps more centrally, scholars drawn to quantitative methods like the senior faculty above, about the importance of building common standards for assessing quality. Their starting point is that efforts at data transparency do not really fix the issue. Small and Calarco are refreshingly unapologetic; however desirable data transparency might be, they claim, it doesn't help to discern good from bad work. What then?

Their answer is simple. When it comes to evaluating qualitative work, it might be helpful to follow a handful of non-exclusive standards and best practices:

- *Exposure*. Have the researchers been sufficiently exposed to (or immersed within) the empirical phenomena they are trying to explain? How much *actual time* have they spent collecting data?
- *Cognitive empathy*. Does the research capture the point of view of participants? Does it reveal sensitivity to subtleties and empathetic connections with the research subjects?
- *Heterogeneity*. Does the research include a diversity of voices? Is it attentive to alternative, and at times, contradictory perceptions, experiences, and motivations revealed by participants?
- *Palpability*. Is the evidence "thick" and sufficiently detailed? Is it presented in concrete rather than abstract terms?
- *Follow-up*. Is the research open to serendipity—flexible to pleasant or unpleasant surprises that arise in the field? Do the researchers adjust their focus and angle of vision considering those surprises?
- *Self-awareness*. Are the researchers generally aware of their position as "outsiders"? Are they open and reflective about how their background and presence in the field might affect the data generated?

1 But see Cyr and Goodman (forthcoming).

While exposure is explained in the introductory chapter, each of the remaining criteria is described in abstract terms in individual chapters. Each is illustrated with fictional—and revealing—examples. These are followed, at the end of each chapter, by notable examples of qualitative works that score high on the criteria at stake.

As someone who has conducted interview-based research and engaged in participatory observation—and as someone who has not received any significant formal training in either—I found these criteria quite helpful. I read them as general principles for good practice that practitioners would do well to follow, and as tentative rules that non-specialist evaluators might want to keep in mind when assessing qualitative work. Though several did seem recognizable (even intuitive) to me as at the data collection stage, they helped me rethink how qualitative data can be presented more effectively—which is never easy. I especially appreciated, for example, their emphasis on "thick description" over aggregation—they offer concrete advice on how to train one's reflexes to resist the temptation to quantify. I also liked that although each criterion stands alone, their common element is that they help us bolster quieter voices and present nuanced empirical findings.

I'm strongly supportive of Small and Calarco's agenda and have no fundamental disagreements, but I do have some observations. Small and Calarco do not rank or weigh *all* their evaluative criteria, which is fine, but the highest value seems to go to the degree of exposure—a precondition, in their view, for strong qualitative research. The greater the amount of time a researcher spends in the field, they suggest, the better the chances are to collect good qualitative data.

While this may appear intuitive at first glance, some healthy skepticism might be warranted. What's missing in the discussion is a link between theory and empirical strategy. One can be sufficiently exposed to a particular phenomenon and able to see and document empirical regularities in a compelling manner. When it comes to *evaluating* specific works, however, the most pressing questions to ask are how well a theoretical explanation holds up empirically and whether its explanatory power is greater than that of rival explanations. What if, for example, a researcher has spent too much time immersed in the wrong field site? Can exposure ever work in the opposite direction and even have detrimental effects? I'm reminded of Funes the Memorious, a fantasy tale by Argentine writer Jorge Luis Borges often used in conversations about ethnography (Auyero 2012). The story tells the challenges of Irineo Funes, a boy with a prodigious memory who could think of nothing but details. He was too trapped in the weeds at the expense of abstract thinking. Exposure was his virtue; it produced heterogenous and palpable evidence. It was also Funes' curse; he was incapable of linking detail to general patterns.

Evaluating quality, in short, may not be easily decoupled from theory. One can find plenty of examples of qualitative political science research that score high terms of exposure (and on the other evaluative criteria), but that, at the end of the day, do little to force a major rethinking of established theory or generate novel insights about poorly understood political phenomena. How effective is qualitative literacy *by itself*? While the authors don't claim that it's necessary and *sufficient* for high-quality research, if the aim is to persuade outsiders, a stronger link between theory and qualitative rigor must be made.

Readers of this journal might also wonder about how useful the best practices proposed by Small and Calarco are for process tracing methodologies, when what's at stake is either scrutinizing alternative arguments or illustrating a theorized causal mechanism. While I think the suggestions are compatible with process tracing overall, they are hardly sufficient to generate good work. They are particularly helpful, I think, for generating compelling mechanistic evidence and to trace out a theorized causal process (e.g., Beach and Pedersen 2013). They are helpful, in other words, to illustrate *how* and *what* questions, which are surely critical for theory generation (Fu and Simmons 2021).

Small and Calarco's suggestions may be less helpful in the application of Bayesian process tracing approaches, or when one is estimating how strongly the empirical evidence fits with the working theory when set against rival explanations (e.g., Fairfield and Charman 2022). What matters within this framework is the *validation* of evidence—using other interviews, secondary analyses, counterfactuals, and alternative sources of data. The book tells us little about how this can be done compellingly. This is an area for further literacy development, as works using process tracing that rely on weak qualitative evidence do abound. I just read one in a prominent outlet, for example, that relied on secondary interviews (conducted by a scholar other than the writer of the study) for validation but offered no discussion about how the interviews were conducted, raising significant issues about data quality, reliability, and trustworthiness of the findings.

A promising area where I think Small and Calarco could push further is to help us think of guidelines on how to do this well. While the "heterogeneity" criteria points in that general direction, being attentive to variation in participants' responses and to changes over time doesn't fully hit the mark. When specific works

of political science are evaluated, it is reasonable to ask whether they include all the relevant evidence or whether "inconvenient" evidence is ignored—whether the accounts presented are not only diverse and palpable, but whether they hold up against other forms of evidence. I would welcome Small and Calarco's suggestions on how to navigate these complicated waters.

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How to Judge? Qualitative Literacy and Process Tracing Studies

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We all had papers and research proposals rejected invoking standards that, at least to the qualitative researcher, were clearly not applicable: no external validity; difficult to replicate; mere description; selection on the dependent variable(!). We know the list. As Small and Calarco point out, qualitative research's purview is growing but "qualitative literacy" is not keeping pace. That is, the competency of "others" to assess the quality of qualitative research is often insufficient.

Against this background, the authors set out to provide the reader-reviewer with a set of indicators to help recognize craft and assess qualitative evidence competently. In short, how to distinguish good from not-so-good interview-based and ethnographic research? Now, here is a certain irony in being asked to review academic work that is, essentially, about how to review academic work—and unfortunately for me, Small and Calarco's indicators cannot offer guidance. Therefore, in what follows, I do two things. First, I consider the extent to which the authors achieve their primary objective: to provide reviewers, scholars, journalists, and others with a "nonexclusive set of indicators" to evaluate research drawing on in-depth interviews or participant observation. I argue that although their objective is more than meritorious, they, at times, stray in the direction of advising the practicing scholar more than the reader-reviewer. Second, as a practical exercise, I consider how their indicators fare in relation to another approach to qualitative research: process tracing. Here, I find the indicators carry wider than the data collection methods addressed in the book. In fact, I believe *all* qualitative research would benefit from taking them—exposure, cognitive empathy, heterogeneity, palpability, follow-up, and self-awareness—seriously, and, by extension, that the quality of qualitative research can indeed (despite my initial critique) be assessed along these lines.

First, the objective to equip reader-reviewers with the tools to assess the quality of qualitative work is more than meritorious. We can only hope future reader-reviewers will have such a (tentative) list and the book provides a wonderful basis for it. For one, although the authors say the list is "nonexclusive," I had difficulties coming up with indicators to add. Honesty came to mind; the idea that a researcher should be forthcoming about doubts, considerations, contradictions, changes, limitations, and so forth. But honesty is probably covered by heterogeneity, follow-up, and self-awareness. Second, they introduce their indicators in clear terms supported by easy to follow, well-constructed examples slowly building up in complexity. The non-scholarly or scholarly non-qualitative or non-interpretivist reader should really be able to grasp the examples. Where, then, does the critical point I raise in the introduction come from?

My point is this: the assumption seems to be that when we know what good data looks like, we should be able to recognize it. Although the indicators are extensively discussed, also providing generous advice to

the practicing scholar, the reader-reviewer is at times less well served. Especially for the "untrained eye," a clear summary or short list of key terms for each indicator would have been helpful. What are the signs of cognitive empathy or self-awareness in a manuscript? How can we see the researcher elicited heterogenous and palpable data, or that they practiced follow-up? I am aware these (sub)indicators are in the book, it is just that they remain implicit, scattered throughout the pages. I have three reasons to raise this point. First, offering a set of indicators is so explicitly the aim of the book and I am afraid the five primary indicators may be too abstract. Second, I would like reader-reviewers, including myself, to have that summary to pin to their (our) walls. Third, as I hope will be clear from what follows, I believe the indicators do, indeed, offer a good basis to begin to assess the quality of qualitative research and it is high time we, as qualitative scholars, start quibbling its details.

Second, inspired by the book but myself neither an ethnographer nor (solely) relying on in-depth interviews, I consider how the indicators fair in my own field of methodological expertise: process tracing (PT). PT is a method relying on in-depth case studies to expose the *causal mechanism(s)* that brought about a given outcome (Beach and Pedersen 2019; Falleti and Lynch 2009; Goertz 2017; Mahoney 2015; Mayntz 2004; Runhardt 2016). Data, and therewith data collection, can be diverse: interviews are frequently employed, but so is archival research, desk research, and so forth. As Beach and Pedersen argued: "evidence can be any type of material that might be left by the workings of our theorized causal mechanism" (2019, 171). Personally, I use PT to study state foreign policy making and combine PT with a focus on narratives (van Meegdenburg 2019; 2023).

Exposure—Although not treated separately, exposure is put forward by the authors as a precondition for high quality data: without adequate time in the field the other criteria would hardly be achievable. For PT, this is no different. Exposure would be understood more broadly as hours spend searching for data—including on-site explorations and interviewing, but also time spent searching (digital) archives for documents, minutes, reports, newspaper articles, you name it—but as with interview-based or ethnographic research, case saturation is important. And the only way to achieve saturation is through adequate exposure. Like ethnographic and interview-based work, PT can be laborious and time consuming.

Cognitive empathy—Reflecting on my own work, cognitive empathy is relevant, but, here too, in a slightly different way. For PT, interviews are possible but not necessary. Yet, when I am working with archival data or, especially, narratives, my aim is often to try and tease out how the main actors "view[ed] the world and themselves—from their perspective" (Small and Calarco, 23). That is, in laying bare a (social) mechanism, actor motivations, believes, perspectives, and emotions, as well as their causal relevance, are often, if not foregrounded, at least playing an important role in the background. Therefore, even when not producing (all) data in a reactive interaction, cognitive empathy should apply to much PT work. To the extent actor perceptions, meanings, and motivations are part of a mechanism, cognitive empathy should be part of our practice.

Heterogeneity—Directly applicable, heterogeneity is important in PT. If someone were to find every step of a hypothesized mechanism perfectly confirmed, "the reader would have reasons to be suspicious" (Small and Calarco, 62). For me, these reflections were timely. I am currently working on a manuscript aiming to expose particular foreign policy narratives and have to find ways to present the heterogeneity that does, most certainly, exist. I found actors and actions can be ambiguous; narratives are always contested; and archival data and documents rarely speak with a single, perfectly aligned voice. As the authors remined me: heterogeneity in the data does not weaken our conclusions; presenting heterogeneity actually heightens the credibility of the research. And this applies to PT, and the evidence we present for our mechanisms, as well.

Palpability—Palpability, or "the extent to which the reported findings are presented concretely rather than abstractly," is also directly applicable. In the case of PT, however, it may have a very specific implication. Palpable data would be data that is explicitly linked to a *particular step or part of the mechanism*. The more abstract argument generally regards the mechanism as a whole, whilst mechanistic evidence should focus on the concrete points, the steps or parts, in between. In my case, the abstract argument regards discursive justification or rationalization as a mechanism, whilst concrete evidence regards specific discursive interventions at specific points in time. In fact, I think PT, and its analytical focus on mechanisms, begs for—and may therefore help elicit—palpable data. Thinking in terms of processes and mechanisms,also when a more formal PT is not the aim, could help qualitative scholars consider what palpable may mean in their case.

Follow-up—Follow-up in PT is what Beach and Pedersen call "iterative" research (2019, 286). PT is often a back and forth between data and theory, between adjusting expectations and gathering more, specific, new, and different data. In my own research, interview statements would alert me to the existence of certain documents, whilst new documents would send me back to talk to people. In fact, over the course of the project

I changed my research focus; I started focusing on narratives only after the data made clear that my initial suspicions were too simplistic, too blunt to describe the much more nuanced processes I was witnessing. Follow-up, thus, is relevant too.

Self-awareness—Lastly, self-awareness may well be the indicator least explicitly discussed in PT. And yet, of course self-awareness also matters when we are not eliciting data through reactive interactions. Who we are, how we think, read, and look, shapes what we see and hear and what we think is significant and worth following up on. In fact, I think self-awareness may well be a key lesson for non-interview heavy or ethnographic process tracers. For me, this includes an awareness of our biases and the behaviors they elicit. The authors discuss *outgroup bias* in relation to heterogeneity, for PT, especially when starting from theory, a *confirmation bias* may play a similar role. I can only say that this point, at least by me, is taken to heart.

Overall, for I really must round this discussion up, I must conclude that PT, and probably *all* qualitative research, would benefit from taking the indicators seriously, and that the quality of qualitative research can indeed be assessed along these lines. To that extent, the indicators provide ample input for reflection, and the book takes a big step towards advancing qualitative literary. This does raise one final thought; the book really begs for a companion: *Qualitative Writing: A Guide to Writing-Up Ethnographic and Interview Research*. Because—as also occasionally shines through in the discussions—the way that research is written affects whether reader-reviewers can recognize the standards that informed the practice.

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Ordinary Extraordinary Ethnography

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This is a very useful book, for which ethnographers and other qualitative social scientists should be grateful. It is helpful in the way of style guides: a rejuvenating reminder of first principles, helpful no matter the stage of one's mastery of the subject. Qualitative researchers can easily fall victim to the "curse of knowledge" (Pinker 2014) and take shortcuts in both the analysis and presentation of data. Small and Calarco name five traits that sound qualitative research has: cognitive empathy, heterogeneity, follow-up, and self-awareness. Like a style guide that includes first a convoluted sentence and then a clear one, they include interview and participant observation extracts that do not benefit from the robustness that comes from those traits and then ones that do. The side-by-side comparisons are compelling and give practical ways for people to improve their literacy both in consuming and producing research.

At times, the authors' examples seem a bit too perfect. I have never interviewed anyone who spoke in the tidy, direct quotes the authors present as examples. I'm not sure anyone has. But reading them improved my research practice. Based on the authors' reminders about getting into the "hows" and the "whys" that are the added value of qualitative research, I found myself asking follow-up questions more attentively.

Small and Calarco divide qualitative research into two main categories: interviews and participant observation. This division makes sense from the perspective of their exposition. However, it is not how research tends to occur. Doing ethnography is not just a matter of observing the details of a scene. Usually, the ethnographer is also speaking with people— sharing opinions and asking questions. Similarly interviewing is not simply asking questions. It is also about observing and noting reactions and context. In my comment, therefore, I'll add to Small and Calarco's list of indicators a few that arise out of the fundamental intertwining of conversing and observing, a pairing that literate qualitative research maximizes, but which is not much discussed in the book.

Ethnography has become trendy even beyond is disciplinary roots in anthropology. Yet many ethnographers use only one blade on the ethnographic Swiss Army knife and ignore the more interesting tools. For instance: observing the details of a scene, or noting daily practices, are the easiest-to-open tools, but not those that achieve the most interesting results. Ethnography should compare what people *say* and what they *do*, or in some cases the *ideal* and the *actual* practice. Do people do what they say? Or do they do something different?

Being consistent and coherent is a difficult project, unlikely to succeed absent deep reflection and effort. For instance, Muslim women participating in the piety movement in Cairo put immense care and thought into aligning their daily practices and habits with those they saw as delineated in the Koran, as Saba Mahmoud's brilliant *The Politics of Piety* (2004) documents. In fact, most people do not do what they say they do—or at least, they do it differently from how they talk about it. A good ethnographer should ask questions and make observations to understand the relationship between the ideal and the actual.

In my research with Rwandan soldiers working as UN peacekeepers, I have met many soldiers who speak passionately about protection of civilians. They describe how their own loved ones were not protected during their country's genocide (1994) and civil war (1990-1994), and their desire to do things differently. I have no reason to suspect that their sentiments are insincere. And yet when they are deployed, they tend to default to "following the rules," like peacekeepers who do not have that kind of personal conviction. Part of my task is to investigate the discrepancies between stated reason and actual practice—not to catch them out, but to isolate and articulate how "the social" (that elusive force that operates with and without the awareness of human agents) intervenes in their conduct.

Let me add an item to the authors' list of indicators of qualitative literacy: its insights should be *non-obvious*. I borrow this term from the sociologist Randall Collins. Qualitative research is a process. You do something (ethnography, interviews), and by virtue of doing that thing you learn something that you did not know. Ideally, you learn something that the people you are researching did not know. When I share findings with my interlocutors, I hope they will respond as a person might respond to a therapist who has noticed something self-concealed for reasons of humility, shame, or anything else: "I hadn't thought of it that way, but you've given me a lot to think about."

A contrasting model of qualitative research is to be a spokesperson for subjects. In that model, the subjects know everything, more than any researcher ever could, and the researcher's job is to "gain access" (the cliché most often used) so that people grant her their knowledge. The researcher-as-spokesperson model places inherent value on being an insider (a "native ethnographer," to use a mostly outdated term). That's fine; I will never have the depth of detailed, personal knowledge about peacekeeping that my interlocutors have. But I speak to many people—reflecting, observing, putting things together, juxtaposing other things—such that at the end of the process I can arrive at conclusions they recognize themselves in and yet have not articulated themselves. This is the realm of the non-obvious.

Linguistic research offers a helpful comparison. By definition, native speakers have perfect linguistic judgment. They cannot produce an incorrect sentence. However, if you ask a native English speaker the precise adjectival order for attributes of origin, color, size, and age, he will likely reply with a blank stare. But if you give him the adjectives and a noun he will immediately produce "big old red American car" and not "red American old big car" or some other variation. The social knowledge produced through ethnography is not as technical as what comes out of linguistics, but in a similar fashion there are modes of knowledge and insight to which the research interlocutor has privileged access, and there are capacities that the researcher brings that help draw out an analysis that sounds right to the people being described but that is likely different from what they would have written themselves.

But how can people assess whether something is non-obvious and correct, or non-obvious and false? You start by avoiding clichés in all forms. They work by evoking something we already know, to provide a fast-track to an answer, in the process circumventing our usual faculties of critical reflection and consideration of multiple hypotheses and factors. A version of seduction by cliché is what happens all the time on social media. It's part of why people "share" or "like" what they do. Yet we know that if a story elicits outrage or dudgeon,

and if the findings coincidentally match self-flattering moral narratives, then probably something has been left out and questions went unasked, or lines of reasoning were never pursued. If a qualitative study is suffused with liberal/progressive orthodoxy while ignoring or dismissing alternatives (for instance, anthropologists often reject perspectives they attribute to economics), the findings might circulate widely within the field but without the vigorous criticism that they might receive if they challenged that orthodoxy. That criticism might actually spur greater insight that can be more broadly understood and taken into account.

Another trait associated with non-obviousness is poetically expressed by the German romantic Novalis, who wrote:

> To romanticize the world is to make us aware of the magic, mystery, and wonder of the world; it is to educate the senses to see the ordinary as extraordinary, the familiar as strange, the mundane as sacred, the finite as infinite. (quoted in Beiser 1998, 294)

This richly descriptive idea has turned into a cliché, oft repeated in introductory anthropology courses, that ethnographers should "make the strange familiar and the familiar strange." But I like Novalis's version better, for the way it inspires us all to shake off our common sense and approach everything and everyone as a curious newcomer who takes nothing for granted. After all, common sense is a hyperlocal phenomenon with radically distinct variants in different societies.

Making the familiar into something strange (or vice versa) requires integrating a variety of levels of analysis: everyday practices, key symbols, worldview, material factors (what some call "structure"). However, some ethnographers focus on one level, usually everyday habits and practices, and exclude others. In so doing, the familiar can get taken for granted, and the level of insight decreases. A recent and prolific sub-genre of peacekeeping research uses this "everyday practices" approach to assess peacekeepers' presence and effects. They catalog things that peacekeepers do. That approach generally doesn't compare what people say with what they do. It also doesn't take into account the organizational or material power-imbalance factors that contribute to producing the strange state of affairs being analyzed. And consequently, the conflicting allegiances, priorities, and paymasters that people actually are dealing with get centrifuged into a uniform and non-conflictual culture of practice.

The anthropologist Max Gluckman offered a way to sidestep the problem of presenting culture as static and uniform, when he was doing research in the 1930s, in what he called "modern Zululand" (now South Africa). At the time, it was common for anthropologists to study "the Zulu" as if they lived on an island and never came into contact with other people. Gluckman (1940) took a radically different approach: he looked at the dynamics of actual social situations in which Zulu interacted with other people, notably government officials. By doing that, he could better grasp the power and politics—material conflict, status, cooperation—that simultaneously tied people together and divided them. In interactions among people who saw themselves as different the unwritten rules became much more apparent, as did the conflicting loyalties and values that people were trying, often in vain, to bring into alignment.

In contrast, the most prominent political scientist of everyday peacekeeping practices, Séverine Autesserre, presents good-intentioned but ineffective peacekeepers who are walled off from "the local," which is presented as "the source of wisdom and truth," in Joshua Craze's (2021) critical appraisal. Presenting locals as wise founts of solutions and peacebuilders as walled-off and ignorant makes it possible to offer a fix: get the peacebuilders to listen to the locals. But it takes too much for granted about this set-up: "Peacebuilders might have all the good will in the world, but that counts far less than the structural limitations of the organizations they are part of" (Craze 2021). Indeed, I have met peacebuilders who speak local languages and have read every book about the places where they are working, but in tense moments they behave the same as their counterparts who have just jetted in and would have trouble pointing out which direction is north. Looking at situations—what people say and do in relation to each other—draws out material power imbalances, conflicts of values and moments of awkwardness, and often provides a chance to compare what people say with what they do. In my experience, studying interaction rituals across identities is a "royal road" to making the familiar strange, and in so doing, to producing new insight into why the world is the way it is.

While reading *Qualitative Literacy* I often recalled the historian, Carlo Ginzburg. Ginzburg (1989) points out the fundamental, irreconcilable differences between the "conjectural" model of the "humane sciences" and the "Galilean scientific" model of quantitative fields. The former is the successor to the skills humans developed in hunting: tracking, learning to identify and assess clues, and putting all those impressions together into a coherent theory. In the conjectural way of thinking, individuals and individual cases matter, "precisely because they are individual, and for this reason get results that have an unsuppressible speculative margin" (106). Ginzburg (1989, 124) wrote:

> The quantitative and antianthropocentric orientation of natural sciences from Galileo on forced an unpleasant dilemma on the humane sciences: either assume a lax scientific system in order to attain noteworthy results, or assume a meticulous, scientific one to achieve results of scant significance … The question arises, however, whether exactness of this [scientific] type is attainable or even desirable for forms of knowledge most linked to daily experience—or, more precisely, to all those situations in which the unique and indispensable nature of the data is decisive to the persons involved … In such situations the flexible rigor (pardon the oxymoron) of the conjectural paradigm seems impossible to suppress.

There is no contradiction between *Qualitative Literacy's* contention that there are standards and traits that we can assess in qualitative research and Ginzburg's contention that qualitative, "conjectural" ways of knowing require interpretive élan that cannot be bottled and distributed. But there is a tension. It's a tension in the same way that great writers never follow all the style guides' rules. Indeed, even the writers of style guides often do not follow their own rules. Compelling writing and "noteworthy results" marry established traits and guidelines with something additional that is untaught and creative. So alongside Small and Calarco's list of indicators I'll reserve some space for the "flexible" part of flexible rigor. It reminds us that there are parts of qualitative research that people have to figure out for themselves, and that this is an opportunity rather than just a potential for bias or error.

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AUTHOR'S RESPONSE: On Process, Discrepancy, Pursuing the Non-Obvious, and Not Missing the Forest

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The best gift an author can receive is a serious reader's attention, and here we find much reason to be grateful. Van Meegdenburg, Lombard, and Anria have each read the book with care, reported faithfully what we were trying to do, and offered much by way of feedback.

They all saw that we were trying to do something uncommon; a guide to evaluating, not conducting research; an attempt to redress the fact that, in a world where many qualitative researchers will have been evaluated by reviewers without that expertise, precious few guides exist about how the latter might do so, about what criteria they should bring to bear on the problem of distinguishing good from ineffective field studies. Our reviewers saw, too, that we sought to write a book at once thoughtful but accessible, unambiguous about its points precisely because of the seriousness of its subject, since a book on qualitative methods comprehensible only to insiders would fail at the precise task of cultivating qualitative literacy. The reviewers, finally, were generally favorable in their comments. No author can complain to hear that one's book provided a "wonderful basis" for its "meritorious" aims (van Meegdenburg, this symposium), that it is "very useful" and "rejuvenating" (Lombard, this symposium), or that it is "excellent and much needed" (Anria, this symposium). Still, though believing that the book largely succeeded in its aims, all reviewers

pointed to infelicities, missed opportunities, or places for expansion.

All had more to say than we can cover in these pages, but a few of their points deserve special mention. Van Meegdenburg (this symposium) notes that the indicators we listed as signals of good interview and observational research can be applied to a core technique often applied to archive-based case-study research: process-tracing, which the author describes as "a method relying on in-depth case studies to expose the *causal mechanism(s)* that brought about a given outcome" (italics in original). The idea of tracing the processes through which a cause produced an effect has been an important proposition among many seeking to identify precise ways qualitative research is important to social science at large, and particularly to causal analysis. In her review, van Meegdenburg proposes that both our precondition, exposure, and our five indicators—cognitive empathy, heterogeneity, palpability, follow-up, and self-awareness—could all be used to evaluate the effectiveness of a process-tracing study.

Van Meegdenburg argues that process tracing requires "hours spent searching for data," since "the only way to achieve saturation is through adequate exposure." In a similar way, she makes a case for each of the indicators, showing quite convincingly that some version of each was applicable. She argues, for example, that a core element of process tracing is concretely connecting each event or phenomenon to its consequence, an undertaking that, indeed, seems a great deal like what we call palpability. We confess that we had not considered examining process tracing in this light and would ultimately have to agree with the author that doing so represents an appropriate extension of our work.

Van Meegdenburg's final comment is intriguing: "the book really begs for a companion: *Qualitative Writing: A Guide to Writing-Up Ethnographic and Interview Research*. Because—as also occasionally shines through in the discussions—the way that research is written affects whether reader-reviewers can recognize the standards that informed the practice." What van Meegdenburg is suggesting here is that a qualitative researcher might achieve cognitive empathy with their participants, produce highly palpable data, attend to heterogeneity, follow up when necessary, and engage self-awareness throughout the research process but then, ultimately, fail to demonstrate that high level craft in their written work. We agree that this is a plausible scenario, and we hope that *Qualitative Literacy*, although written for reader-reviewers, will also be of use for qualitative scholars who are seeking guidance on how to demonstrate the quality of their craft. That said, we would also welcome a companion piece like the one that van Meegdenburg proposes, and we are certain that it would be of great use to budding researchers.

In an elegant essay, Lombard (this symposium) makes a case that, to our list of five indicators, one could add at least two. The first, which we may call discrepancy, is that "[e]thnography should compare what people *say* and what they *do*, or in some cases the *ideal* and the *actual* practice. Do people do what they say? Or do they do something different?" The contrast between words and action has long been a core focus of qualitative research, and social science more broadly, for many years (Deutscher 1966; Deutscher et al. 1993; Jerolmack and Khan 2014; Small 2017; Small and Cook 2021). In fact, one of us has recently pursued that line of inquiry, examining the discrepancy among low-income African Americans in urban neighborhoods between how they describe their connections with others and what they actually do (Small et al. 2022). So, we are inclined to agree with Lombard that discrepancy can matter a great deal. However, we would probably consider it less central than some of the others, as uncovering such discrepancies may not be important to some research projects, including many in the interview traditions. For example, a study of the differences across people in what they believe about a recently elected president may reasonably be more concerned with meaning and perception than with action, such that uncovering a discrepancy might derail the project from its objectives. In general, for the body of work in which perception as such is what matters—as in, for example, the vocabulary of motives tradition (Mills 1940) and in much of interview-based research—discrepancy as such may be of secondary import.

Lombard's (this symposium) second indicator is non-obviousness, which refers in particular to uncovering something that the people one is studying do not already know. Lombard's perspective stands in contrast to the notion that the ethnographer merely reports to outsiders what insiders know deeply. She makes the point with a wonderful analogy, to a language scenario:

> By definition, native speakers have perfect linguistic judgment … However, if you ask a native English speaker the precise adjectival order for attributes of origin, color, size, and age, he will likely reply with a blank stare. But if you give him the adjectives and a noun he will immediately produce "big old red American car" and not "red American old big car" or some other variation. The social knowledge produced through ethnography is not as technical as what comes out of linguistics, but in a similar fashion there are modes of knowledge and insight to which the research interlocutor has privileged access, and there

> are capacities that the researcher brings that help draw out an analysis that sounds right to the people being described but that is likely different from what they would have written themselves.

The point is excellent: there are aspects of a social order that the ethnographic analysts is privy to that is not obvious to—and therefore not reported by—those observed. Indeed, this possibility gave birth to structural anthropology (Levi Strauss 1963), whose core objective was to uncover the structural rules underlying the observed social order.

At the same time, this indicator seems to tell us something more about the quality of the analysis than of the craft in data collection. Recall that *Qualitative Literacy* expressly devotes itself to addressing indicators of high craft in the collection of data, not analysis (while acknowledging that a strict separation of the tasks is not always feasible) (Small and Calarco 2022). Part of our motivation in setting analysis aside is scope: there are far more issues, perspectives, and debates—including heated contentions—than can be covered in one short book. Lombard (this symposium) is surely right that, if everything in a study is self-evident, then something is probably missing, since the study of society is nowhere near advanced enough that all aspects of any social context would have been not only fully understood but also absorbed by both researchers and insiders. But what is missing likely lies not in the data collection but in the analysis: not in the words "big," "American," "car," and "red," but in the fact that their common order reflects a rule that the research must uncover. In this sense, we agree, and believe it is perhaps among indicators for the task we set aside, for how to distinguish effective from ineffective analysis.

Anria (this symposium) begins his essay with precisely the kind of predicament we fear many qualitative researchers face. A senior political scientist, having read Anria's early work, was prompted by the graduate student for an assessment of whether it was ethnographically good: "I would not know how to tell—as you know, I'm more drawn to quantitative analyses. But it's well written." Young Anria was shaken, and the passage could have served as introduction to our book.

Perhaps the most important of Anria's points involves the relationship between theory and exposure. Anria argues that some of the questions to ask when evaluating the totality of a work in "are how well a theoretical explanation holds up empirically and whether its explanatory power is greater than that of rival explanations." It would be difficult to disagree with that statement, though we note that the statement could be said about any kind of work, rather than distinctly qualitative research. Still, he argues that such theoretical orientation is needed to make effective use of exposure, because without it a researcher may in fact be lost in the figurative weeds: "What if, for example, a researcher has spent too much time immersed in the wrong field site? Can exposure ever work in the opposite direction and even have detrimental effects?"

Qualitative Literacy is clear that exposure is an insufficient condition for good work: "Exposure is the foundation. But it is not a guarantee" (Small and Calarco 2022, 20). Nonetheless, asking whether too much exposure can hurt, rather than help, is an interesting question. Anria (this symposium) points to an example, the character in a story by Jorge Luis Borges:

> The story tells the challenges of Irineo Funes, a boy with a prodigious memory who could think of nothing but details. He was too trapped in the weeds at the expense of abstract thinking. Exposure was his virtue; it produced heterogenous and palpable evidence. It was also Funes' curse; he was incapable of linking detail to general patterns.

Perhaps a researcher spends so much time in the field that they are unable to see the figurative big picture.

We believe the problem here is not exposure as such but deeper issues that exposure merely brings to light. The scholar "incapable of linking detail to general patterns" is not suffering from exposure but from an inability of finding general patterns. To be sure, the scholar already facing difficulty with general patterns will not be helped by more time in the field. But the point is that a scholar without that limitation would merely use the greater exposure to either deepen their understanding of the patterns or identify new ones. The heart of the issue, therefore, is to cultivate the capacity to identify general patterns, not to reduce exposure.

Still, Anria's comment points to something that van Meegdenburg and Lombard also implicitly note: that *Qualitative Literacy* is much less an end than a beginning, less an attempt at a final say than an introduction to a debate. We appreciate what the reviewers have added to our work, and we hope that others do so as well.

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Notes from the Field

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Reconciling Changing Positionalities: Reflections from my Fieldwork in Nigeria

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In the second year of my PhD, I went on my first extensive fieldwork trip. As a Nigerian woman studying in the Netherlands, I was enthusiastic about travelling across Nigeria to conduct interviews and gather data for my dissertation. I conducted in-depth interviews, producing qualitative evidence about why and how election violence happens. Focusing on the subnational level, my work leverages differences in elites' organizational capacity to explain variation in the actors, targets, and scale of election violence. My theory explains the role of elites and non-elites in the organization of election violence and its implications for subnational patterns of violence. I selected respondents based on their knowledge of the subject. I interviewed local politicians, local journalists, civil society members, and party supporters.

This note reflects on the fieldwork and data collection process in three locations: the states of Lagos, Rivers, and Plateau. Data collection spanned the course of four months from February 2022 to June 2022. During the interviews, respondents were open and willing to share their observations and experiences with election violence. Although violence is a sensitive topic in many contexts, Nigeria is an exception. Nigerians discuss political issues candidly and publicly, our partisan identities are non-conspicuous, and political grievances and election violence are publicized on media platforms. As a Nigerian, I am aware of Nigerians' disposition to talk about election violence. I was therefore not surprised when respondents freely shared information about election violence, although some of them were understandably hesitant to reveal incriminating information during interviews.

Before starting fieldwork, I was optimistic about my ability to succeed in finding respondents due to my familiarity with and knowledge of the local context. Because Nigerians are often eager to connect with foreigners, I knew my affiliation with a foreign university would endear me to respondents. Yet, I had some pre-fieldwork concerns. Although I am aware of my multiple identities such as gender, race, sexuality, occupation, education, income, and immigration status, I could not accurately predict which would have the most effect on the way my respondents engaged with me. I was concerned in particular regarding my identity as a female researcher. Given that I would be interviewing men in a highly patriarchal society, I wondered if male respondents would accept and respect my interview

requests. Fortunately, I was lucky to meet respectful respondents who were willing to create time for interviews despite their busy schedules. Contrary to my expectations, it was my positionality as a Nigerian "insider" with an "outsider" perspective that aroused interest and triggered reactions from my respondents. In this note, I reflect on how I navigated respondents' perceptions of my "insider-outsider" positionality, as well as their impact on the research process.

Before heading to the field, I knew my affiliation with a foreign university would provide certain advantages such as access to more respondents and easy of entry into public offices. Because Nigerians are hospitable to foreigners and people associated with foreign institutions, I fully expected the warm reception I received. But even though I was aware of the advantages of being a Nigerian "foreigner," I initially struggled with accepting the "outsider" aspect of my identity. This was particularly true in one of my fieldwork locations, where I strongly connected with my insider identity. Coming to terms with respondents' perceptions of me as an outsider even though I identified as a Nigerian insider concerned me because I feared it would negatively affect the quality of my interviews. Eventually, my dual positionalities proved to be more of an asset than a liability and those concerns faded away.

Other scholars have also struggled with the "insider-outsider" dynamic in the field (Fujii 2018; Soedirgo and Glas 2020; Srivastava 2006). Merton's (1972) definition of "insiders" as members of specified groups and collectivities or occupants of specified social statuses, and of "outsiders" as non-members, resonates with how I identify on the positionality spectrum. On the one hand, being Nigerian and having the same nationality as my respondents qualified me as an insider. On the other hand, living outside Nigeria and being integrated into the Netherlands meant that for many I was effectively an outsider. Moreover, despite being a Nigerian insider by nationality, I was still an outsider in places where I did not share the same ethnicity or regional identity as my respondents. In this sense, fieldwork served as a reality check: it helped me recognize positionality, which Merriam et al. (2001) define as where one stands in relation to "the other," and in my case, where I stood in relation to my respondents.

My positionality in relation to my respondents was constantly changing in the various locations. In Rivers state, where I grew up, my positionalities constantly clashed, and I struggled with being treated as an outsider even though I felt like an insider. In Lagos state, where respondents treated me like a Nigerian foreigner, my insider-outsider positionalities peacefully co-existed. By contrast, in Plateau state my foreign affiliation reinforced my outsider status. For context, Plateau is in the North Central region, and as a Southerner, I am considered an outsider. My foreign affiliation amplified this outsider status. I was not only treated as a Southerner but also as someone who lives abroad. Although I initially struggled with navigating my changing positionalities—at times an "insider" with knowledge about the Nigerian case and at others an "outsider" with the perspective of the Nigerian diaspora—recognizing and accepting them ended up being advantageous for the data-gathering process.

My Positionalities in Rivers State

I started the interviews in the ethnically diverse and religiously homogenous Port Harcourt, the capital city of Rivers state in the South-South region. Prior to relocating to the Netherlands, I lived in Port Harcourt for several years. Here I was an insider on several dimensions, including origin, religion, ethnicity, familiarity, and knowledge of the logic and patterns of election violence. Being a native, I thought of myself as one of the respondents, as an insider. I was aware of Rivers state's notoriety as a hotspot for crime, cultism, and election violence. Using my insider knowledge, I avoided sketchy areas and refrained from asking direct questions that could compromise my respondents' safety.

After my first few interviews in Rivers state, I became aware of respondents' varied perceptions regarding my positionality. Because of my connections outside Nigeria, some of the respondents did not view me as Nigerian as they viewed themselves. To them, living in the Netherlands meant that I could no longer relate to their experiences. As a result, my understanding of their context was flawed. Because I value my integration into a new country, I did not expect respondents to see it as a problem. For example, before an interview with a local politician, he said to me: "I will tell you how we do things in Nigeria, I don't know how they do it where you came from." In saying this, the respondent implied that I knew more about Dutch than Nigerian politics because I no longer live in Nigeria, despite knowing that I was a Rivers indigene and that I had only recently relocated.

This, and more subtle statements like "*you dey talk like oyibo*" (You are speaking like the Caucasians), clearly alluding to my outsider status, weighed on me. At first, I dismissed them and attributed them to their lack of familiarity with me. Later, I started to feel dismayed by my lack of Nigerian-ness, obsessing over questions such as: Did my two years abroad make me less Nigerian? Will this affect my ability to connect with respondents? While battling with such genuine concerns, I was faced with another problem: respondents who overly recognized

my insider status assumed that I already knew too much about the context and therefore did not feel the need to tell me what I "already know."

Stuck in between those who thought I knew too much and those who believed I knew too little was problematic. For the respondents who perceived me as an outsider, I found myself overcompensating, feeling the subtle pressure to gain their acceptance by exaggerating my Nigerian mannerisms and bringing Nigerian slang into our conversations. For those who regarded me as an insider, I consciously downplayed my knowledge of the subject, feigning ignorance when they described instances of election violence I already knew about. With limited time on the field, I did not have the luxury to slowly untangle the positionality dynamic. Choosing to focus on my most important concern which was gathering accurate information from the interviews, I started to recognize and acknowledge my two positionalities, aiming to understand how they shaped the research process. Ultimately, I decided to connect with my Rivers respondents in whichever way they chose to connect with me as an outsider or insider.

Lagos State

After I concluded the interviews in Rivers, I went to Lagos state in the Southwest region. In Lagos, respondents were less mindful of my dual positionality, but like those in Rivers, they too wanted to associate with my research. Perhaps because Lagos is a melting pot of ethnicities and identities, almost everyone there is, in a sense, an outsider. What my respondents lacked in time, they compensated for in their knowledge of Lagos politics and their willingness to share it with me. It was in Lagos that a local politician revealed his party's complicity in hiring a popular organization to engage in election violence, explicitly stating that: "They are our instruments, we use them for the violence." Such open and honest revelations characterized most of the interviews and were deeply appreciated by my outsider self who knew little about subnational politics in Lagos. I attribute the openness of Lagos respondents to their awareness of my outsider status as a foreign student and respect for my insider status as a Nigerian. They tackled the interview questions with ease, comparing the patterns of election violence in Lagos to other states. Many of my respondents even asked me to describe how Lagos differed from Rivers, a clear indication that they saw me as someone with legitimate knowledge of my home state. Perhaps living in Lagos, the most diverse state, has exposed them to multiple personalities, making my layered identities a non-issue. Lagos was the only location where my positionalities could just be, and respondents addressed me as a Nigerian foreign student.

Plateau State

From Lagos, I travelled to Plateau state in the North Central region, where subnational patterns of violence are vastly different. Here I chose to leverage my outsider status. Plateau state is geographically and politically different from Rivers and Lagos. Establishing the logic of violence was like piecing a complicated puzzle together. Geographically, the hilly topography was a pleasant contrast to the flatlands to which I was accustomed, and the temperate weather was a welcome change from the warmth and humidity of Lagos and Rivers. Politically, unlike in Rivers and Lagos states, ethnoreligious cleavages rather than partisanship are salient triggers of election violence. I therefore spent time learning about the associations between ethnicity and election violence, seeking to understand the implications of such linkages for my argument.

In light of the volatility of ethnic violence in Plateau state, I embraced my outsider status, relying on local contacts for information about safe and unsafe areas to visit. Adhering to the advice of my local contacts, I did not schedule interviews in riot-prone neighborhoods. While interviewing respondents, I attempted to connect informally with them by speaking the Nigerian vernacular pidgin English. But their preference for the Hausa language, which I do not speak, served as a reminder of my positionality. It was in Plateau that I realized the dichotomy between Northern and Southern Nigeria. Respondents accepted my outsider status, viewing it as an opportunity to educate me on the context and to clarify some misconceptions about Plateau and the North Central region, in the hopes that I would relay such information to my Southern friends. I thus connected with respondents on two outsider levels: as a Southerner, and as a Nigerian foreign student.

In the three locations, there were moments when people expressed admiration for my outsider status, such as the lady who excitedly hugged me while expressing her admiration and "pride" for what I was doing, and the gentleman who offered to recommend respondents after he became aware of my foreign affiliation. For many of those locals, my outsider status was admirable, they regarded me as one of the Nigerians making progress in the diaspora. Similarly, my insider status presented its own advantages, allowing me to blend into new areas, meet people without drawing attention, and understand respondents' verbal and non-verbal cues, including slang and mannerisms.

Active Reflexivity and Lessons from the Field

During fieldwork I engaged in a deep introspective self-reflection in the form of active reflexivity. Actively

reflecting on my positionalities shaped my contextual perceptions of them, and the assumptions I was making about my respondents. As Soerdirgo and Glas (2020) suggest, active reflexivity is necessary for recognizing and responding to positionality in research practice. I thus came to accept my positionalities, allowing me to make the most of my identities depending on the context. First, because my affiliation with a foreign university endeared me to people, it was easy to find respondents. Perhaps respondents thought that my research would have more value abroad than at home. This meant that during interviews some respondents occasionally strayed from responding to questions to asking about life in the Netherlands. When this happened, I had to politely redirect the conversations back to election violence.

Second, respondents believed my research was important because of my foreign affiliation. Given the poor reputation of research conducted in Nigeria, they were willing to contribute to my interviews because of the prevalent perceptions that research is more relevant and useful when carried out abroad. As a result, respondents were more forthcoming with detailed and honest information. For example, before an interview with a party supporter, he confessed that he would not have been honest with me had he not been aware of my foreign affiliation. His honesty affirmed his respect for my research; it was his way of building camaraderie by supporting a fellow Nigerian studying abroad.

Third, as an insider knowledgeable about election violence in Nigeria, being too close to the case could have compromised my objectivity. Thankfully, my outsider status allowed me to maintain a healthy distance from the case, which is important to conduct reliable and valid research.

By the end of the trip, I learned valuable lessons which I intend to carry into my future fieldwork. I now understand what Fujii (2018, p.19) means by "no researcher is ever a 'true' insider or outsider." During the research process, many people will fall under both groups at different times, switching between them (Fujii 2018). With such understanding comes acceptance, and I have accepted that being referred to as an outsider is not necessarily a bad omen for my research. On the contrary, it can be quite beneficial. Recognizing and understanding respondents' perception of my positionality and my perception of theirs helps me harness the advantages of my dual positionalities. Doing so does not make me more or less of an insider or outsider.

The boundaries between the insider and the outsider are never clearly demarcated (Merriam 2001). As a result, dealing with my layered identities is always context-dependent and a continuous work in progress. Depending on the places I visit, I might find myself leaning more strongly on one identity over the other. As Bourke (2014) posits, we are shaped by the research we undertake, but as long as we are introspective throughout the process, we will also be shaped both by it and the people with whom we interact.

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Notes from the Classroom

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Dissecting and Reconstructing Case Studies

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Students who want to write a dissertation that includes qualitative case study analysis are typically motivated by wanting to study a specific political phenomenon in a specific context. As soon as they start turning their personal interest into a potential argument to test in their research, though, they face canonical questions such as: What is the logical scope of my argument? How representative are the cases that I want to study? Should I study more cases to test my argument? How many, and which ones? Over what temporal span? How deep should I go in my analysis of each? Is field research necessary? How much of it ? In which of my cases? All of them? Some of them? What is the appropriate mix of primary and secondary sources to support my claims? The same questions may arise down the line when, as often happens, students find that they must rethink or specify their initial research questions.

Methodology lectures and textbooks offer an important foundation for students to think through those research design questions. Textbook knowledge, however, can only go so far. Effectively navigating the practical tradeoffs between the potential research payoff and costs in terms of time, resources, and available skills that the various options entail, depends on the specifics of the research. And the category of "case studies" includes an enormous variety of objects of analysis, sites of research, and of course, types of evidence. The tremendous variety of evidence that can be analyzed in a case study—qualitative as well as quantitative; from historical archives or legal research as well as from interviews, participant observation, or ethnography—and, relatedly, the variety of skills required to collect and analyze it, is an important feature that separates the dilemmas of qualitative research design from those emerging in quantitative and experimental research.

Typically, graduate students tackle these issues in a trial-and-error fashion, drawing on the experience of their supervisor, and through informal discussions at conferences or with other faculty. Several years ago, I came around to the view that something more systematic might help speed up the learning process. The basic idea was to provide graduate students engaged in (or about to embark on) case study research with a workshop-like venue where they could practice these skills by analyzing published research and engaging in guided discussion with their peers. The Oxford calendar offered a good opportunity to hold a short, four-week module at the start of the summer (Trinity) term—convenient in terms of time and duration for students who already have a heavy courseload. So, in 2007, I designed a short graduate seminar aimed at covering these idiosyncratic aspects of the case study research process. Students would have already attended the normal sequence of quantitative and qualitative methods lectures and workshops, which are offered in the first two terms of the Oxford academic year, so they would come to my short course equipped with the necessary methodological knowledge to be able to think through the more practical aspects of their research strategy.

Originally, I expected the course to attract at most five or six particularly motivated graduate students who were writing comparative case study dissertations. My initial idea was that students would choose, in coordination with me, their own text to analyze and discuss in class, and we would focus every week on a different set of practical questions, reconstructing how they had been addressed by the author, what were the plausible alternatives, and what the implications of such alternative routes would have been. There would have been time for broad-ranging and relatively unstructured discussion, in a rather small setting that would have allowed individual students to bring up their own research dilemmas in connection with the class discussion.

In fact, things went quite differently. Instead of the handful that I expected, eighteen students signed up. They came roughly equally from our two-year Master of Philosophy programs in Comparative Government and

European Politics and Societies (in which students write a 30,000-word thesis) and from our doctoral program. A few enrolled from other departments. This led me to reorganize several aspects of the course while keeping its basic structure and purpose. To keep the discussion manageable and still involve everyone, I decided to assign texts for discussion not to individual students, but to groups of students. I adjusted the workload proportionally for individual students so that it would be comparable to that of other courses. I did not let students choose their own texts to analyze and discuss in class but rather provided a list from which each group could choose. Furthermore, considering the variety of training and research experience among the students, it was necessary for me to dedicate some time to reviewing the basic methodological issues that we would be discussing each week. Finally, with a larger group of students, it was not feasible to rely solely on informal assessments and individual students' motivation to keep track of the group's progress over the four weeks of the course. Therefore, I prepared weekly assignments.

With these adjustments, it was possible to scale up the course and achieve the original purpose of providing a forum for the systematic discussion of questions of research design and overall research strategy that students would encounter in their own work. Despite class discussions systematically going over our two-hour slot in that first year, the course went well—so much so that the following year I had more than 20 students sign up, and I had to cap attendance. The year after, I split the course in two streams to accommodate demand and to allow for less-hurried discussions. This is how I have been teaching the course ever since. The course's popularity did not decrease during the Covid lockdowns, even though the online format made class discussions and collaboration of groups of students on homework assignments more difficult for some students.

The course is completely hands-on. Students work in groups of two or three to take apart and reconstruct the design of a recently published article, and to propose plausible strategies to improve on the research at each step, in ways that might reinforce, modify, or possibly reject the original argument. Student groups choose their article from a list that I provide. I choose articles on the basis of three criteria: 1) they were published in top political science or political sociology journals; 2) they were published in the last 5-6 years; 3) they consist of (or contain) fully-fledged single or comparative case studies that rely on qualitative evidence. As we know, top professional journals now publish qualitative comparative case studies quite rarely. Yet it has always been possible to find a sufficient number of recent articles to assign in the course. For example, this past year (April-May 2022), we discussed articles published in *World Politics*, *Comparative Political Studies*, and the *American Journal of Sociology*. The oldest piece was published in 2017. All others were published in 2020 or 2021. In previous years, articles from *International Organization* and *American Political Science Review* have appeared on the list as well.

Typically, the articles that we discuss in class present a marked variety of case study designs, world regions, types of questions, and evidence. Again, to take last year's course as an example, the articles discussed included four-, three-, and two-case comparisons, as well as single-case studies; a few of the latter compared two different periods within the same country. Some articles explicitly aimed to modify or integrate existing theories, for example by analyzing outlier cases; others had a theory-testing purpose. Substantively, the regions covered included Western Europe and North America as well as countries as different as Syria, China, and Mexico. The subfields of the articles ranged from political economy to the analysis of mass protest to international cooperation, among others; and the types of evidence analyzed were accordingly different.

Each week, I give student groups an assignment that poses a dozen questions, all targeted at one key aspect of research strategy. The short duration of the course only allows focusing on four such aspects: the relationship between case and theory; research design and case selection; process-tracing and within-case analysis; and use of primary evidence and secondary sources. Each week, the groups answer the questions in a short memo. Their memos are read, discussed, and critiqued in class. This way, each group, each week, puts under a magnifying lens one specific aspect of their article, reconstructing how the author made certain choices, what other choices could in principle be made if one were to study the same problem, and what the consequences of such choices would be.

For example, in week two we discuss research design and case selection. The focus is on the pros and cons of changing the case selection strategy, always keeping in mind, on the one hand, the scope conditions implied by (or made explicit in) the author's argument, and on the other, the skills, time, and resources that would likely be needed to analyze more (or different) cases. In the assignment, student groups are encouraged to explore different candidate cases. Drawing on their individual strengths and experiences, within each group students should acquire enough knowledge to discuss the likely implications of different case selection strategies. In other words, the tradeoffs of adopting a different case selection are not discussed in the abstract, but with reference to specific "candidate" cases. The different purposes of the articles analyzed (such as theory developing or testing),

as well as their different research designs (e.g., small-n versus single-case; the presence or absence of temporal variation) pose different challenges and make students think in a practical way about dilemmas of research design in the context of the analysis of very different problems. In subsequent weeks, we use the same approach to analyze strategies of within-case observation and the use of primary and secondary sources. I use the last 15-20 minutes of each class to give students a "refresher" on the methodological literature concerning the aspect of research strategy that will be discussed the following week.

The final assignment for the course builds on the weekly assignments. Each of the groups must write a 5,000-word memo (a "half-paper") that criticizes the assigned article, proposes a precise strategy that could potentially improve on it by adopting a different research strategy on one or more of the choices analyzed, and documents *why* the strategy in question could be successful. If the memo argues that the original argument could be modified, strengthened, or rejected by analyzing a different set of cases, by focusing on different observable implications in the same cases, or by mobilizing different types of evidence, the students must make a convincing case for the plausibility of their proposed strategy. This task requires them to do enough thinking and preliminary research (read up on different countries, locate specific data, and so on) to confront the choices faced by researchers when designing and conducting a case study project. Working in groups forces the students to plunge themselves into research questions that are often quite different from their own, as at least some students in the group would be working on rather different themes from that of the article analyzed by their group; at the same time though, working in groups allows them to go much further in unpacking the nodal points of research strategy than would be possible individually. Furthermore, the variety of studies analyzed gives us the ability to discuss different types of case study design, and different types of evidence that entail specific questions and dilemmas.

After more than 15 years of teaching the course, three main benefits have emerged in the course evaluations and from informal discussions with students. First, thinking through the dissection, reconstruction, and possible improvement of exemplary research requires that students consider the implications of their decisions for the *whole* research process. Questions such as how many cases they should include to achieve their research goals, over what period to study such cases, and what evidence is necessary and how should it be collected are not asked hypothetically in relation to a research project that still has to be carried out, but concretely, in relation to published studies, where all these questions have been already formulated and tackled by the author. Second, the structure of the course and the assignments allows students to think *creatively* about research choices: the guidance provided by the weekly assignments, the internal group discussions, and the discussion in class provide structure, and the internal division of labor within groups frees energy for concentrating on specific dilemmas of research that may be akin to those that a student faces in her dissertation. Finally, students learn how to navigate *co-authorship*. I explain at the beginning of the course that much political science research is now collaborative, and that collaboration has enormous advantages, but may also entail moments of frustration. I encourage students to divide labor in an efficient way, and most importantly to pull their weight honestly. Student groups write not only the weekly homework, but also the final assignment collectively. The mark received counts as the students' *individual* mark for the course. This exposes students to the free-riding problem in a very direct way, but this has not been a problem. Students attending the course are very motivated, and I involve all students in weekly class discussions. Attendance is compulsory. Indeed, in 15 years of teaching the course to over 250 students, there has only been one instance in which two students in a group of three made me aware (extremely politely and rather indirectly) of the fact that their partner was not cooperating as she should have done.

I always look carefully at course evaluations, and over the years these have consistently shown that students have found the course's hands-on approach very helpful for their own thesis-writing and a useful complement to the normal methodology lectures and workshops. The course seems effectively to "bridge" the theory and the practice of case study research, helping students with the challenge of making their own thesis projects feasible and enjoyable. Former students, who now teach their own version of this course in various universities across Europe and North America, have found the course equally rewarding in context of other graduate programs. It has been certainly rewarding for me.

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Longform APSA Awards (2022)

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Giovanni Sartori Award for Best Book on or using Qualitative Methods

Committee: Rachel Beatty Riedl, Jennifer Bussell, and Laura Blume

2022 Recipient

Paul Staniland. 2021. *Ordering Violence: Explaining Armed Group-State Relations from Conflict to Cooperation.* Ithaca: Cornell University Press.

In *Ordering Violence*, Staniland offers a new conceptual framework to broaden our understanding of state relations with armed actors: *armed orders*. Moving beyond a dichotomous understanding of war, he develops a typology of ways in which governments interact with domestic armed groups, ranging from alliance to total war. In doing so, he reframes how we think about the business of potentially violent politics, which encompasses a significant portion of day-to-day life in many parts of the world. He subsequently provides a theoretical argument for what drives varying government responses to armed groups, highlighting the importance of ideological agendas for both sides. To test this argument, he draws on an impressively rich understanding of South Asian political history, offering nuanced national case studies from across the region and pairing these with analysis of a new and substantial large-N dataset on armed orders. Overall, the prize committee found this book to be a remarkable example of both a thoughtful and innovative conceptual contribution and a rich theoretical argument that is rigorously tested using a mixed-method empirical approach. We commend Staniland on this substantial contribution to our substantive understanding of an important topic and expect that this book will serve as a prime methodological example for the field moving forward.

2022 Honorable Mentions

Martha Wilfahrt. 2021. *Precolonial Legacies in Postcolonial Politics: Representation and Redistribution in Decentralized West Africa.* New York: Cambridge University Press.

Martha Wilfahrt's exceptional book, *Precolonial Legacies in Postcolonial Politics: Representation and Redistribution in Decentralized West Africa,* provides a powerful methodological and theoretical approach to understanding contemporary local government performance. Through an argument focused on institutional congruence, Wilfahrt connects the extent of overlap between today's formal jurisdictional boundaries and social norms within villages that were once home to precolonial polities, which developed collective identities and dense social networks. In the current era of decentralization, these local elites find it easier to cooperate around governance outcomes. This book uses a comparative subnational analysis to show precisely how history matters, and how legacies of collective identity and dense social networks translate across time and through distinctly political channels. Employing archival, interview, original survey data, and a geocoded dataset of village level primary education and basic health infrastructure across rural Senegal, the book provides a model of qualitative and mixed method research that robustly demonstrates the theory across time and space. The book addresses big questions of substantive importance, applies concepts carefully, and marshals unique and innovative evidence to demonstrate the significance of precolonial statehood for collective wellbeing and public service delivery. It is an impressive achievement that will shape future academic and policy debates, defining a future generation of scholarship.

Eduardo Moncada. 2021. *Resisting Extortion: Victims, Criminals, and States in Latin America.* New York: Cambridge University Press.

In *Resisting Extortion: Victims, Criminals, and States in Latin America*, Moncada explores the various forms of resistance to criminal extortion that emerge across Latin America. He makes a crucial contribution to the growing literature on criminal politics and, moving away from the more common focus on homicide in Latin America, draws needed attention to one of the most common forms of everyday violence experienced across the region: extortion. By focusing on the targets of criminal extortion and the varied ways they resist criminal groups despite the immense risks of doing so, Moncada challenges seeing these individuals as powerless victims, and rather shows their agency and incredible resilience. His theory shows how the time horizons of criminal actors, nature of local political economies, and criminal

capture of the police shape the forms of resistance that targets may engage in, ranging from everyday resistance to collective vigilantism. In addition to the theoretical and substantive contribution, the award committee considered this book to be an exemplar of qualitative methods. The book draws on and eloquently combines data from fieldwork conducted in Mexico, El Salvador, and Colombia, including interviews, focus groups, and innovative participatory drawing exercises. We applaud Moncada for this impressive contribution to the field.

2022 Alexander George Article Award for Best Article or Book Chapter on or using Qualitative Methods

Committee: Diana Kim, Eduardo Moncada, and Elizabeth Nugent

2022 Recipient

Nicholas Barnes. 2022. "The Logic of Criminal Territorial Control: Military Intervention in Rio de Janeiro." *Comparative Political Studies* 55, no. 5 (April): 789-831.

The 2022 Alexander George Article Award went to Nicholas Barnes for his article, "The Logic of Criminal Territorial Control: Military Intervention in Rio de Janeiro." In this article, Barnes addresses a timely and pressing question concerning the efficacy of militarized approaches to combating crime. He asks: When do state crackdowns on organized crime elicit violent responses? Barnes identifies the threat from rival criminal actors as the primary mechanism that spurs violent responses in the face of state crackdowns. To answer this question, the author leverages diverse forms of qualitative and quantitative methods to both disaggregate how criminal groups understand territorial control as well as to contrast the various pathways that lead criminal actors to fight, hide, or flee in the face of coercive state interventions. While empirically the piece focuses on the specific case of drug trafficking organizations in the favelas of Rio de Janeiro, Brazil, it contributes to broader questions and theories of the logic of territorial control, competition between criminal organizations, and the relationships between criminal organizations and the state. The findings complicate conventional understandings of the meaning and consequences of territorial control by criminal actors in ways that will inform not only research on the politics of criminal violence but also broader work on order, violence and governance.

While the article's argument is interesting and innovative, the committee primarily chose to recognize this piece because of the author's methodological contribution. The author relies on sophisticated and rigorous mixed method evidence collected during 18 months of fieldwork in Rio de Janeiro favelas to make his argument. Rather than assuming that threats to territorial control look the same everywhere, Barnes leverages local variation, captured using diverse forms of qualitative and quantitative data, to both disaggregate how criminal groups understand territorial control as well as contrast the various pathways that lead criminal actors to fight, hide, or flee in the face of coercive state interventions. By combining detailed case studies and careful process tracing with extensive fieldwork, Barnes produces an exemplary model of comparative ethnography at the micro-level from which scholars from multiple disciplines and subfields of political science can benefit and learn. The appendix to the piece carefully and transparently outlines the assumptions, logistics, and process behind this approach in detail and is a worthwhile read in and of itself.

2022 Honorable Mention

Sarah J. Lockwood. 2022. "Protest brokers and the technology of mobilization: Evidence from South Africa." *Comparative Political Studies* 55, no. 4 (March): 628-56.

The committee also recognized Sarah J. Lockwood's article, "Protest Brokers and the Technology of Mobilization: Evidence from South Africa." In the piece, Lockwood asks why some communities protest to demand that the state provide basic public goods while others do not. The author offers a fresh perspective on foundational disciplinary issues concerning political agency and collective action problems by drawing attention to the crucial role of brokers, defined as "informal, non-state actors," in bridging elites and communities to foster mobilization. Brokers capitalize on their local knowledge, networks, and trust to help elites who seek to mobilize populations overcome the collective action problem. Without protest brokers, elites can struggle to overcome several challenges to successful mobilization, including a lack of credibility and information asymmetries with locals.

The piece relies on a sophisticated mixed-methods approach to data collection, based on over two years of extensive data collection and fieldwork in 12 communities in South Africa. Lockwood expertly combines life histories with protest brokers, 400 elite interviews, focus groups, participant observation and an original media database on protests to analyze patterns of mobilization across twelve communities in South Africa. Case studies, rigorously selected and carefully constructed, illustrate in detail the crucial role of brokers in helping elites successfully mobilize protest as well as the challenges the absence of brokers create that, in turn, undermine collective action.

Kendra Koivu Award for Best APSA Paper on or using Qualitative Methods

Committee: Ajay Verghese, Rachel Schwartz, and Megan Becker

2022 Recipient

Jasmine English and Bernardo Zacka, "The Politics of Sight: Revisting Timothy Pachirat's *Every Twelve Seconds*."

The 2022 Kendra Koivu Award went to Jasmine English and Bernardo Zacka for their paper "The Politics of Sight: Revisiting Timothy Pachirat's *Every Twelve Seconds*." This paper uses a bold reinterpretation of existing ethnographic data to make a new critique about the politics of sight. The piece serves as an important rejoinder to critiques about the extent to which ethnographic fieldwork can be engaged with by those who did not produce the data. The paper offers a blueprint for how this can be done, as well as the promise of doing so

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