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.

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?

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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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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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