Beyond Triangulation: Reconstructing Mandela's Writing Life through Propulsive Facilitation at the Archive

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Abstract

Multimethod studies generally fall under what are referred to as "confirmatory" or "complementary" approaches. Yet in addition to these two approaches, Hammersley (1996) identifies a third, which he refers to as "facilitation." In this paper, we build on Hammersley's observation and argue that methods can be used propulsively, setting the researcher down whole new paths. We first put this observation in conversation with other recent works and pragmatist theorizing. We then use the archive as a point of departure to show the value of such an approach. Specifically, we use our investigation of the archives at the Nelson Mandela Centre of Memory in Johannesburg, South Africa as a site to show how we used propulsive facilitation to engage with and make sense of materials relating to Mandela's memoirs.

Keywords Nelson Mandela · South Africa · Archives · Multimethod research · Propulsive facilitation

Introduction

Multimethod studies generally fall under what are referred to as "confirmatory" or "complementary" approaches (Small 2011). Yet in addition to these two frameworks, Hammersley (1996) identifies a third, which he refers to as "facilitation." Building on Hammersley's idea, we argue that as long as methods are used for the types of questions for which they are best suited, they can be used propulsively, sending the researcher down unplanned for and otherwise unconsidered paths.

We use the archive as a point of departure to show the value of what we call *propulsive facilitation* for producing multimethod qualitative work. Specifically, we use our investigation

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of the Nelson Mandela Foundation archive in Johannesburg, South Africa as a site to show how we engaged with and made sense of material relating to Mandela's presidential memoirs. In our case, we began with a research question that was best addressed by a deep reading of archival materials. In answering this question, a new question, however, emerged, and to answer this question we transitioned and used tools that are traditionally associated with ethnography to consider the archive itself. Our journaling of our experiences in the archive then introduced a new question, which we answered by returning to a different set of archival materials using tools from natural language processing. Once again, our results then introduced more new research questions, which we concluded were best answered through interviews. The bulk of the paper uses our case to demonstrate how our approach works in practice.

In what follows we first discuss how propulsive facilitation relates to more commonly used techniques for multimethodological qualitative research and the natural affinity between what we propose and pragmatist theorizing. We then turn to the promises and pitfalls of archival work, and how archives can serve as field sites through which to engage in multimethodological qualitative work. We then discuss our case and how we propulsively facilitated across different questions and research methods to answer those questions. We conclude with a discussion of the generative potential of this approach, directed to those who might do future archival research.

Multimethod Research

What if a qualitative researcher studied our own productions of knowledge, tracing our articles or books all the way from start to finish (Gross and Fleming 2011)? After actually watching sausages get made, would any qualitative scholar worth listening to conclude that our research processes were as sensical, full of foresight, or as methodologically pure and clear of mind as we so often present them to be after the fact? We believe that if we subjected our own knowledge making to the same degree of scrutiny we apply to our interlocutors and field sites, we would get a much different story: a version of qualitative research in real life that is more messy, full of false starts and wrong turns, and generally less linear than we make it sound down the road. We believe this to be particularly true for multimethodological work. As noted by Hammersley (1996, 164), in reality, most of our research practices most of the time encompass a diversity of approaches "that cannot be reduced to a dichotomy [of methods] without serious distortion."

A good case-in-point can be found on the topic of methodological triangulation, which dominates discussions of mixed-methods qualitative work, and even in so-called inductive analyses. To engage in what Spillman (2014) calls the "thin description" that can be useful in placing the thick description of ethnographic work in broader context, about 5% of Denzin (1978) is concerned with triangulation, whereas over 50% of the nearly 20,000 citations to the book contain explicit mention of the technique by name. As a "confirmatory" multimethod approach, triangulation is, by definition, deductive. This should not be surprising given that our metaphorical usage of triangulation comes from parallax, a geometric technique used in navigation and astronomy to measure non-directly observable distances. As the technique was first summarized in social scientific research, "If a proposition can survive the onslaught of a series of imperfect measures, with all their irrelevant error, confidence should be placed in it" (Webb et al. 1966, 3).

While Denzin (1978) questions the value of what he refers to as *within-method* triangulation and advocates for *between-method* triangulation, others question the value of *between-method* triangulation and advocate for limiting oneself to *within-method* triangulation (Sale et al., 2002). Our goal in noting this is not to enter our vote in debates over what is and is not the best way to triangulate, but rather, to forward the claim that when discussing multimethod work, "confirmatory" approaches like triangulation end up taking up a lot of the oxygen, with triangulation in particular having "become central to much discussion of mixed methods research" (Hammersley 2008, 22). Yet triangulation is not the only multimethod approach available in the qualitative scholar's toolkit and may limit the researcher to findings that are produced with the first method deployed.

The oxygen that remains in discussions of multimethod qualitative work is taken up by what are referred to as "complementary" approaches (Small 2011). Unlike confirmatory approaches, complementary approaches may lead to different findings about the same basic phenomenon, such that a small N finding (derived through one method) is more generalizable across a bigger population (derived through another method), or a bigger population finding is also robust across smaller subgroups of that population. Yet rather than just being concerned with replicability across different levels of analysis, complementary approaches can also add new findings that complement each other across similar phenomena. For example, in his analysis of support for and opposition to marriage equality, Hart-Brinson (2018) uses age-period-cohort models to untangle what caused shifts in support for marriage equality, and then uses interviews with college-aged young people and their parents to document the shifting cultural schemas across cohorts around both homosexuality and marriage, getting at some of the underlying meaning structures that are missed in the survey research. Some of the most interesting findings in complementary approaches occur when methodological approaches *do not* triangulate, leading us to new insights, such as the differences between what people say they do and what they actually do (e.g., Pager and Quillian 2005). With others, we believe that complementary approaches can side-step thorny epistemological questions that arise from confirmatory approaches (Sale et al. 2002), while also treating different methods in multimethod frameworks in a sensible way: as better for answering some types of questions than others (Brewer and Hunter 2006).

Yet despite the emphasis on confirmatory and complementary mixed-methods approaches, they do not use all the tools in qualitative scholars' multimethodological toolkits. In his argument for "methodological eclecticism," Hammersley (1996) outlines three types of multimethodological approaches. Hammersley's second approach, which he somewhat opaquely refers to as "facilitation," is the subject of the remainder of this work. We first outline Hammersley's definition and explain why we add an adjective to it. We then discuss an affinity between this methodological approach and pragmatist theorizing. We spend the bulk of the paper presenting examples of the approach in action from our work on Nelson Mandela's memoirs.

Propulsive Facilitation

Hammersley (1996, 160) defines facilitation as an approach in which one method "acts as a source of hypotheses or as a basis for the development of research strategies in the other." While this definition is concerned with two total methods (methods "A" and "B") and one total step (method "A" leads to a finding, which introduces a new question explored through method "B"), just as facilitating across more than two methods is possible, facilitating across

more than one step is possible. In short, the findings generated from one methodological approach *facilitate* the activation of new questions and methods, such that as findings emerge, they can set the researcher, the method being used, and even what the question is, down new paths. Those new paths may be longer or shorter, lead to dead ends or new discoveries, and perhaps even some confirmatory or complementary findings along the way, depending on the directions in which they lead.

We add the adjective "propulsive" to Hammersley's term not because we seek to change his definition, but rather, to further clarify and refine the type of multimethodological work that we are talking about. While to *facilitate* means to aid or expedite something, adding the metaphor of propulsion to the aiding and expediting of findings is useful for two reasons. First, as a term, it evokes movement: one is moving *across* methods and findings as they go, although not necessarily in a linear fashion. Second, like all propulsors, getting started is hard, but once you are in motion it is easier to stay in motion.

To be clear, our goal is not to invent a new idea. Instead, it is to reapply an old idea and hopefully make it stick. In describing this idea in detail, we first rely on a tool that is regularly used by sociologists to try to make ideas stick: a figure (Silver 2019; Swedberg 2016).

In Fig. 1, in order to highlight the differences between multimethodological approaches, we first present a graphical representation of triangulation. Here, a finding is derived by Method A by however that method derives findings, and then the deductive, confirmatory work of triangulation begins: Methods B and C are used to confirm the finding of Method A. It is worth acknowledging that when methodological triangulation is evoked, findings almost always seem to triangulate.[1] Our takeaway from this should be one of two things: (1) we do not actually need to bother with triangulation because whenever we do it things work out in the end, or (2) there are likely failed triangulations lurking among our non-triangulated findings, and we should probably borrow from Psychology's replication crisis and start pre-registering our triangulation attempts.

Propulsive facilitation is not saddled with these concerns, however, as it is not a deductive approach, and therefore does not rely on the magic trick of the researcher having been right all along. Instead, dead ends are allowed to die of their natural causes, and the researcher is allowed to chart new paths as they go. To use a metaphor from quantitative sociology, for the qualitative scholar the difference between triangulation and facilitation is like the difference between frequentist and Bayesian inference. Triangulation, like frequentist inference, is deductive; a fact (or method) is established and confirmed. Facilitation however, like Bayesian inference, is inductive; knowledge emerges and is built upon in the construction of new paths. This type of iterative thinking is already familiar to qualitative scholars, be it for iterating across theory and method as in abductive analysis, iterating across settings or cases as in analogical theorizing or analytic induction, or iterating across the construction of codes, categories, and concepts in constructing theory from data as in grounded theory. While those using all these approaches do at times incorporate multiple methods, as noted above, multimethod work overwhelmingly seems to return to the hypothetico-deductive "confirmatory" approach of triangulation. Our contribution then is to take all of the familiarity and gusto in iterative thinking that is already familiar to qualitative scholars and *expressly* apply it to a multimethodological framework.

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[1] Exceptions are when *not* triangulating is in service of a larger empirical (e.g., Pager and Quillian 2005) or theoretical (e.g., Deacon, Bryman, and Fenton 1998) point.
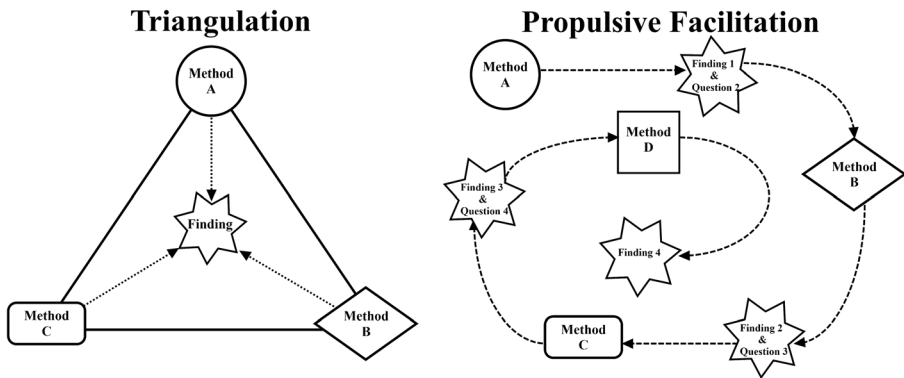
Fig. 1 Triangulation and Propulsive Facilitation

As seen in Fig. 1, when incorporating propulsive facilitation in multimethod research, Method A leads to Finding 1. Suppose, however, that Finding 1 also introduces or makes apparent a new research question. In this scenario Method A may be equipped to answer that new research question (which would lead to a return to Method A in search of an answer to Question 2), but importantly, maybe Method A is *not* best equipped to study that new research question, which leads to the deployment of Method B. In this scenario, the researcher has used propulsive facilitation. She started with a research question and used a method to derive a finding that generated a new research question, the answer to which would be best derived through a different method. While the story could stop there, it does not have to stop there, as it can also extend out to more methods and more findings, some of which may even end up triangulating or complementing other findings along the way. Figure 1 presents a four-question/four-method scenario of propulsive facilitation, not because the technique can only be used across this many (or few) steps, but because it mirrors our own multimethodological process, which we describe below.

We see propulsive facilitation as having a natural affinity with pragmatist theorizing. As Emirbayer and Maynard (2011) point out, pragmatism holds that obstacles in our experience, whether everyday activities or even working in the archives, lead to "efforts in creative problem solving." Dewey (1991, 220) would characterize this obstacle as a moment when "there is something the matter" and Addams (2002, 31) would term it as "perplexity." It can perhaps best be understood, following (Follett 1924, xv), as a "mystery moment" where how to move forward is unclear. These moments aren't a call for mere trial and error, however. Rather it calls for deep thinking, particular in the case of research work—what Dewey (2007, 111–12) might call a "careful survey." Methods and findings that emerge out of propulsive facilitation are similar to Dewey's (1991, 155) "ends-in-view." Dewey (1991, 15) tries to explain the processual in action: "An end-in-view is a *means* in present action; present action is not a means to a remote end." Dewey (2007, 104) could almost be speaking of the research process and the construction of social objects when he further explains: "we do something to the thing and then it does something to us in return: such is the peculiar combination."
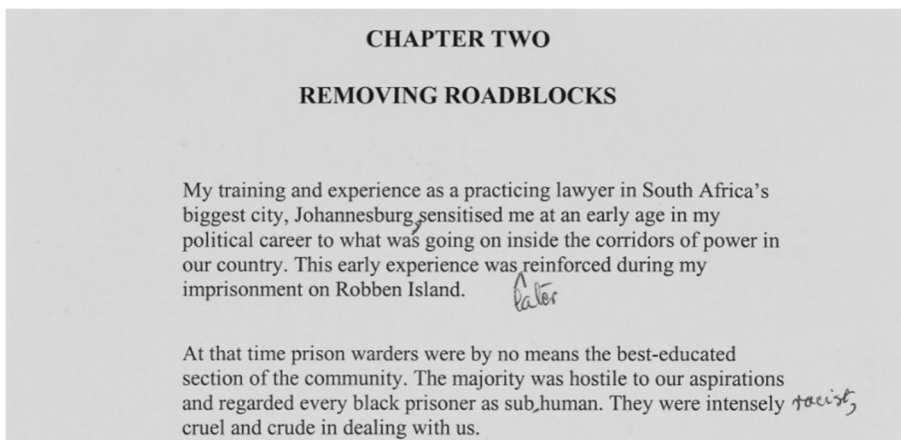
Swedberg's (2012, 2016) recent work on theorizing, grounded in the pragmatist ideas of Peirce, is helpful in understanding the overall process of propulsive facilitation we propose. The researcher steps back from their data during this moment of rupture, takes a look around, and finds a new, perhaps different way to move forward. It is a constant process across the research. In stepping back, the researcher moves into a creative mode, finds a likely way

forward through theorizing, and then moves back to more data collection. Put another way, we might consider propulsive facilitation as the methodological side of abductive analysis (Timmermans and Tavory, 2012, 2014), in which rather than theory and data being preordained in a sequential way, the researcher ping pongs back and forth between theory and data collection as they go. This fits with recent understandings of how qualitative research might be understood in general terms to be "an iterative process…achieved by making new significant distinctions resulting from getting closer to the phenomenon studied" (Aspers and Core, 2019: 155). Srivastava and Hopwood (2009: 77) liken this process to a "progressive focusing." We further discuss in the conclusion what we believe to be this natural affinity between abductive analysis, increasingly focused iteration, and propulsive facilitation. Before discussing how we used propulsive facilitation as a multimethodological approach for a project that centered around the archives at the Nelson Mandela Centre of Memory, we first briefly discuss archives as field sites more broadly, as well as our entry into our particular field site, as both are necessary to contextualize the multimethodological story that is to follow .
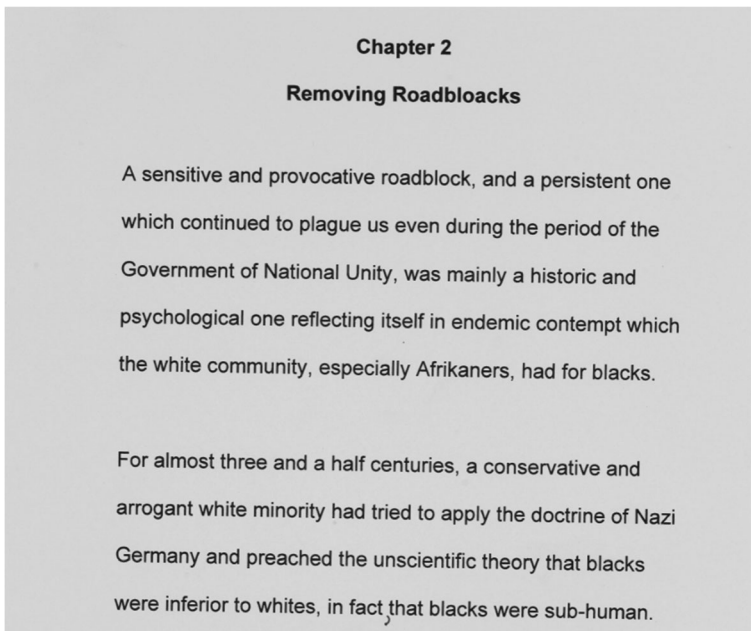
The Archive

While much scholarly knowledge is built on the contents of archives, the practice of retrieving and examining archival data is one that researchers often learn on their own (Manoff 2004; Rubin 2019). Few methodological "how to" guides discuss archival research, and when they do, they assume that it suffices to be equipped with a clear focus and a standard set of analytical tools to analyze the documents that will be obtained (Gilliland and Mckemmish 2004). The archive, as Becker (1998, 107) points out, is mistakenly treated as a place where the social scientist can simply show up to "get it all." Thus, through methodological omission the practice of archival research is rendered as straightforward—devoid of the uncertainties, risks, and conundrums that call for the "tricks of the trade" that are essential to other lines of research.

As a discipline that drew its boundaries around the study of contemporary social dynamics, sociology treated archival research as the domain of historians (Moore et al. 2016). Even historical sociologists, whose research is linked to records and recollections of events, used to
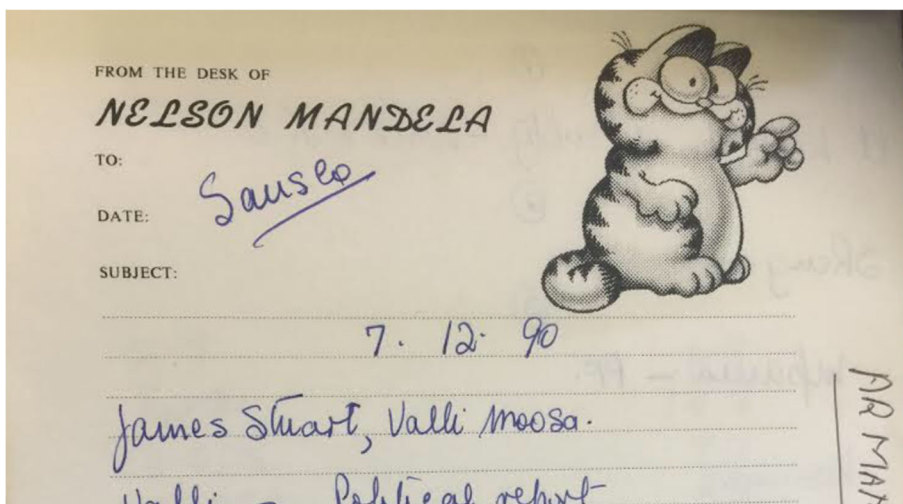

Fig. 2 The First Two Paragraphs of an Earlier Draft of "Removing Roadblocks"

Chapter 2

Removing Roadbloacks

A sensitive and provocative roadblock, and a persistent one which continued to plague us even during the period of the Government of National Unity, was mainly a historic and psychological one reflecting itself in endemic contempt which the white community, especially Afrikaners, had for blacks.

For almost three and a half centuries, a conservative and arrogant white minority had tried to apply the doctrine of Nazi Germany and preached the unscientific theory that blacks were inferior to whites, in fact that blacks were sub-human.

Fig. 3   The First Two Paragraph of Mandela's Final Draft of "Removing Roadblocks"

engage with the archive from the distance. Comparative analysis was the "go to" approach for the second generation of historical sociologists, and the large number of cases they analyzed led them to synthesized information presented in secondary texts (Skocpol 1984). The few researchers that relied on primary historical sources had to find their way in the archives with no maps, developing their own sensibilities for the collection, analysis, and presentation of the data they encountered (Shapiro et al. 1998). More recently, there has been growing awareness of the richness of archival sources like letters, diaries, photographs, and memorabilia, and their

FROM THE DESK OF

NELSON MANDELA

TO:

DATE:

SUBJECT:

7 . 12 . 90

James Stuart, Valli moosa.

Valli — Political report

Fig. 4   Mandela's Personalized Stationery in 1990

usefulness to tackle a wide range of questions. Although we believe this point to be uncontested, of late sociologists in areas such as gender (Stanley et al., 2013), organizations (Haveman 2015), race and culture (Brown 2018), and political sociology (Slez 2020) have relied on primary data from archival sources. Yet with archival work outpacing methodological insights on archival work in current sociology, discussions of methodological concerns and approaches to archival work are just recently starting to emerge (Moore et al. 2016; Rubin 2019; see also this volume).

At the most fundamental level, archives are sites that contain a set of physical and digital materials that are collected, classified, filed, and made accessible through technology (Mbembe 2002). At some point in time, some person, group of people, or institution put together a repository of information in some place, whether bricks and mortar or virtual. It may be well-known and oft visited, or it may be a place that is not yet on the research map. In this dimension, much like an Egyptian tomb, even the familiar archive holds the promise of a treasure trove of untouched and unmanipulated facts. While scholars in the past subscribed to the myth of the archives as vessels of objective records, they have for the most part adopted a critical stance (Manoff 2004). Those who practice archival research can attest that on the ground, real archives are much more than complete repositories waiting to be combed through. Archives are instead like field sites, where contingencies and complexities are common.

We see three key ways in which archives are dynamic field sites, the particularities of which the researcher must always be aware. Active consideration and engagement with these dynamics—or rather, a methodologically informed *approach* to archival research that builds on the prior experiences of those conducting archival research—can help the researcher anticipate and successfully manage potential feelings of disorientation and frustration, encounters with the unexpected, or challenges of preconceptions and the understanding of experiences far removed from one's own that may arise (Rubin 2019).

First, as noted by Derrida (1996), the information that is collected and preserved through time depends on the methods, instruments, and organizational arrangements that are available for these purposes. Documents, accounts, and materials that do not pass the test of "archivability" are excluded from the records and therefore from archivable memory (Mbembe 2002). Thus, much like museums do not display sculptures that are too big to fit through their doors (Becker 1974), the knowledge scholars can derive from an archive is subject to what fits into the mold of the archive they collect data from. Moreover, counter common characterizations of timeless stability, the contents of the "repository" are in flux. Paper goes through a natural process of decay, and as information technology advances, the archives learn to follow. The same is true for digital files that move across software changes and updates, and migrate from floppy disks, to desktop hard drives, to the cloud. While new files are incorporated, others are misplaced or disappear, and breakdowns and malfunctions are routine. For these reasons archival documents can at times behave as "unruly objects" (Domínguez Rubio 2014), and those conducting archival research must be mindful of what Skarpelis (2020) calls the "life of the file" that they are engaging with. More generally absences, knowledge gaps, and transformations are challenges that push the researcher to be flexible and creative in her methods.

Second, archives are situated within organizational structures with different logics and aims. Institutions and individuals keep records for specific reasons and with objectives in mind (Harris 2002; Manoff 2004). Archiving involves a process of selection by which functionaries, archivists, and independent collectors decide on what is worthy to be preserved and which voices are legitimate (Mbembe 2002). As noted by archivist Verne Harris (2002, 63), "far from

being a simple reflection of reality, archives are constructed windows into personal and collective processes. They at once express and are instruments of prevailing relations of power" (63). Most troubling, institutions and individuals may *not keep* records for specific reasons too. As Garfinkel (1967, 189) points out, the interests and priorities of those in charge of recordkeeping and those of researchers do not always align, so what could pass as a casual omission could instead be a case of "'good' organizational reasons for 'bad' clinical records." Rendering the constructed frames of the record visible—and perhaps exiting these confines of official institutions to explore alternative archives—is necessary to reckon with the knowledge that we can produce (see also Luft 2020).

Third, archives are maintained—and sometimes gatekept—by a diverse set of actors and situated in the midst of social dynamics that the outsider must learn to navigate. The researcher's ability to build relationships, gain familiarity with the constellation of interests underlying the organization, and even sometimes ability to make strategic exchanges determines what is made available to her. Archivists are oftentimes the most central actors and they shape researchers' engagements with the data. Interactions with the archival community can present researchers with valuable research opportunities as they can work as gatekeepers, exploration guides, key informants, and translators (Clement et al., 2013); the types of roles already familiar to qualitative scholars (e.g., Morrill et al., 1999). In short, and as discussed in more detail by Mayrl and Wilson (2020), rather than just a repository, the archive is itself a social world, and conducting archival research is rife with phenomena already familiar to qualitative sociologists.

In response to this layered social world that conventional approaches fail to address, scholars such as Rubin (2019) propose thinking about and even conducting archival work as one would an ethnography—that is, with an analytical attunement to context and to self (see also Lennartsson 2012). Rubin (2019) advises fellow researchers to take note of the full experience from the details of the building and interactions with the staff, to their personal routines and emotional responses. Additionally, she recommends being open to changing the course of one's inquiry in the face of new discoveries. Accounting for the full scope of one's experiences in the archives is important not only because of their impact on the data but also because they can lead to interesting insights.

As we discuss below, in our archival research at the Nelson Mandela Centre of Memory, we used Rubin's (2019) approach to archival research to productive ends. We say "approach" here to make clear that we are not suggesting a full-on organizational ethnography as part of archival data collection, but rather, suggesting that incorporating some of the methodological tools traditionally associated with ethnography to document one's experiences while working in the archive can be used. For the purposes of our work, taking jottings while in the archives and converting those jottings to quasi-field notes of our experiences there led to a different quality of mind. Rubin's insights were particularly useful as we stumbled face-first into the concerns and considerations addressed above in our discussion of the epistemology of the archive. Rather than a repository of data waiting to be uncovered, we entered into a world of archives, non-formally archived "shadow archives," and dynamic social relationships that not only dictated our ability to answer our research questions, but also fundamentally reoriented what our research questions even were. Recounting the dynamic, interactive, and flexible exchange between a facilitative multimethodological approach and archival research itself takes up the remainder of this article.

Mandela's Archives

The true origin stories of research projects are often obscured by ex post facto bullshit. Our case is no different. The front-stage version is a strong but vague assertion about a plan to study unpublished handwritten notes to fill a gap in knowledge about Nelson Mandela and the history of South Africa. Our messier, backstage version is that Schneiderhan had a meeting about international student exchange in Cambodia with a vice president of our university when talk turned to South Africa. The administrator mentioned that in his recent visit to the Mandela Foundation in Johannesburg, staff there had mentioned the existence of President Mandela's hand-written notes on a yet-to-be-published presidential memoir. In addition to helping the foundation accomplish its goals through an international student exchange, what a cool research opportunity this might be, the administrator mused. Several emails and phone calls and one year later, we were on a plane to South Africa with only a vague idea of what we might actually find once we arrived.

Our combined backgrounds in biography, politics, cultural production, and African studies gave us some confidence that we could handle whatever we came across. But we were going in mostly blind. Beyond some assurances about some of the available materials from the archive staff, we did not know if the third- and second-hand accounts of what the archives contained were actually what they did contain. And despite our repeated references to reciprocal exchange and helping the foundation with student support, we had not yet fully put together why the responses to these portions of our emails did not seem to generate much response. We note this because the genesis of this project—a conversation that was formally about international student exchange—also shaped its directions and the decisions we made, both with regard to the work we conducted and our overall approach to it.

Sitting at the terminal in Pearson Airport while waiting the two hours for our flight take-off, we talked through the upcoming work, both with regard to the data that might be accessible to researchers and the forms it might come in, and with regard to how we could make ourselves useful to the foundation and support its work. Down two columns we wrote a list of best- and worst-case scenarios of what could be accomplished during our nine-day research trip. Without getting too much into the weeds on these details, suffice to say, all of our "best-case" scenarios regarding the data available to researchers and the forms it came in panned out within 36 hours of landing. This left us with a week's worth of undirected time to further explore.

Going in, here's what we knew: Mandela's final autobiography, *Dare Not Linger*, had recently been released. It was co-authored by the South African poet, activist, and journalist Mandla Langa, and based on drafts for ten chapters that Mandela had written between 1998 and 2004 for a book at that time titled *The Presidential Years*. The archives held multiple drafts of Mandela's writing of these ten chapters, which we intended to study. We had been told that some of the chapters had been revised up to five or six times (true), and that some drafts had editorial feedback in the marginalia from global leaders at the time, like Bill Clinton (not true). We were interested in these archival documents for what they could tell us from the theoretical perspectives of auto-deliberation, creativity, and cultural production. From these vantage points on the banks of a project, in the sections that follow we trace our path across the lily pads, highlighting both how our research questions, and the methods that best facilitated them, have transitioned along the way.

The First Step: A Deep Reading of the Archive

We approached the archival materials—and in particular Mandela's multiple revisions across draft chapters of *The Presidential Years*—with a theoretical interest in deliberation and auto-deliberation (Schneiderhan and Khan 2008; Schneiderhan 2015). By looking across drafts of Mandela's writing could we find evidence of him deliberating and changing his mind, and if so, on what topics was he deliberating? This was, in effect, our research question. We quickly found evidence to this effect in revisions of Mandela's first two paragraphs of "Removing Roadblocks," the second chapter of *The Presidential Years.* In an earlier draft he begins the chapter as seen in Figure 2.

In this earlier draft of relaying his thoughts, Mandela opens by noting he had previous experiences interacting with power, which were reinforced by his experiences as a prisoner on Robben Island. In the second paragraph he discusses the hostility, cruelty, and crudeness of his prison guards, and attributes it to their lack of education. Rather than just being cruel and crude, however, in revision of this draft he attributes *a cause* to their cruelty and crudeness by inserting the word "racist." Yet none of this text made it into Mandela's final draft of Chapter 2. Instead, as seen in Figure 3, through revision the first two paragraphs of "Removing Roadblocks" became entirely different.

During early drafts, Mandela opens his second chapter by relaying his personal experience with cruelty in prison. In revision he marks this as racist cruelty, but attributes it to a lack of education among his captors. In revision, however, he widens the social, historical, and structural scope of the message he intends to convey; what was true of poorly educated white prison guards is not unique to them—and, in fact, in revision those poorly educated guards have disappeared entirely—as instead, South Africa is "plague[d]" across centuries with an "endemic contempt" by an "arrogant white minority." The only words that survive across these drafts is that blacks are viewed as "sub-human." Tellingly, in the earlier draft there is the qualifier of "black prisoners," who are regarded as sub-human, whereas in revision, this is not about prisoners, but about the treatment of all South African blacks across centuries. As evidenced by this example, we engaged in a deep reading of archival material on the hunt for signs of deliberation and change across Mandela's drafts, and instead of finding them tucked away or hidden from view, they were screaming out at us: in revision Mandela had transformed a biographical observation into a clear-throated indictment of history.

Yet in beginning to answer what we believed at the time to be our central research question, from this deep reading of Mandela's archive we also developed new questions. An unintended positive consequence of poring over the daily records of Mandela's life is that we were unblinded from his "star image" (Wilson 1998), and—and we are not proud to admit this—forced to reconcile our impressions of him through his internationally celebrated image with the plain reality that he was in fact a real person independent of that image. The clearest example of our lack of understanding of Mandela's complex personhood was our audible shock at seeing his personalized stationery from 1990, as seen in Figure 4.

If the reader, like us at the time, has primarily understood Mandela through his constructed star image and not as a real person, she too might feel confusion, pleasure, or even glee about his Garfield stationery. Yet this must be reconsidered. Imagine sharing the anecdote, "I knew someone who in 1990 had a picture of Garfield on their stationery." This would be an incredibly poor anecdote because the fact that there was a picture of Garfield on someone's stationery at the height of the branding of Garfield is neither revelatory nor interesting. Yet that it was *Nelson Mandela* who was the person who had Garfield stationery is however treated as

interesting simply because of the incongruity between him being a real person and his international star image.

It was in reconciling this for ourselves that we realized that our own understandings of Mandela, like many people across the globe, had been generated through decades of cultural production; we had been the recipients of decades of work of an international Mandela memorialization project, and only understood him through that lens. Considering it that way, the Nelson Mandela Centre of Memory where we were doing our archival work was, we suddenly realized, not simply a storehouse of archival documents, but was in fact also the *central node in the memorialization project we had understood him through*. Although it seems slight and obvious in retrospect, it was that observation, unintended from our deep readings of archival materials, which led to a new research question that required informally leaning on some of the tools from a different method.

The Second Step: Borrowing some Ethnographic Tools

As argued by Rubin (2019), when conducting archival research, the researcher might consider not just retrieving and analyzing archival texts, but also gaining insights from some of the tools of ethnography in the collection of those documents. Our version of Rubin's insight was to journal on our process as we collected archival data, first taking jottings while conducting our archival work during the day, and then turning those jottings into journal entries at night. From our journal entries, we came to the realization that we were not just encountering archival evidence of Mandela deliberating, but were also doing so within an organization that was far from neutral in the construction of Mandela's image. Rather than an uninvested third party or just a place that exists for the researcher to show up and "get it at all" (Becker 1998, 107), the archive and its staff were at times tasked with being deeply involved in maintaining a particular version of who Mandela was and what, after his death, was his posthumous meaning.

In reflecting on our journal entries, we also realized that we had come across conflicting institutional logics about what the role of the archive was (Thornton et al., 2012). For instance, if Mandela, in a private conversation, had been recorded saying something interpersonally unkind about someone else on a topic of no real historical significance, should that be included in the archive or not? Similar to some of the findings discussed by Mayrl and Wilson (2020), the answer to that question sometimes depended on who you asked. More generally from our journals we noticed patterns: documents and resources that seemed to appear out of thin air as our rapport with the staff built up, and instances in which there might be "'good' organizational reasons for 'bad' clinical records" (Garfinkel 1967, 189).

It was also in a conversation with an archivist that we realized that we had to more assiduously document changes across drafts of *The Presidential Years* to figure out where the ideas for those changes were coming from, and to understand the potentially collective work on *The Presidential Years,* we had to widen our scope and also study changes across *Long Walk to Freedom* and *Dare Not Linger*. Now with many more drafts and much more text to work with, as a first step we turned to tools from natural language processing.

The Third Step: Natural Language Processing

From our readings of multiple drafts of Mandela's *The Presidential Years*, we began to develop a sense for Mandela's writing style and process. From secondary sources and informal preliminary conversations written up in our quasi-field notes, we learned that his

advisors had been suggesting that he bring in a collaborator to assist on what would come to be internally known as the "sequel" to *Long Walk to Freedom*. Yet Mandela had resisted help and wanted to do it himself. We had also heard the impression that both *Long Walk* and *The Presidential Years* may have significantly changed through collaborative influence throughout the process. The first step in investigating this question was documenting how much the texts had changed across drafts. Here we offer an example from *Long Walk to Freedom*.

From the archives we had three documents: (1) Mandela's original 1976 draft of *Long Walk to Freedom* that had been collaboratively written while a political prisoner on Robben Island, and (2) 697 pages of interview transcripts between Mandela and his collaborator on what would become (3) the version of *Long Walk to Freedom* that was first published by Little Brown in 1994, and would go on to sell over 15 million copies and be celebrated the world over as the definitive account of Mandela's life. Across these archival documents, in this text that had been "worked and re-worked and travelled through institutional and other collaborative processes" (Nelson Mandela Foundation 2014), had anything been lost along the way? Guided by Bail (2012) we turned to WCopyfind, the plagiarism analysis shareware developed by Lou Bloomfield.

While plagiarism software is designed to find repeated passages and phrases across documents, across Mandela's Robben Island draft and the published drafts of *Long Walk,* overwhelmingly what we found was their absence. All told, depending on the range of (reasonable) parameters set, between 1 and 4% of the text strings in Mandela's own 1976 version of *Long Walk* made it into the published and internationally celebrated version of *Long Walk*, as did about 1% of the text strings in the interview transcripts. While Mandela's stories, worldview, and perspective may have survived the publication process, his actual words had not. While this was for us a major finding in itself, it also introduced a new question: if Mandela's words had not come from him, where did they come from? What was the collaborative process by which they were derived? And even if the words themselves were different, how involved was he in the constructing of them? And what was the process by which they came to represent him?

The Fourth Step: Interviews

With a new set of research questions derived from tools from natural language processing, we turned to interviews, which could answer these questions in a way that deep textual reading, a quasi-ethnographic approach, and natural language processing could not. In our interviews we began to document these processes: Who was in which rooms when decisions were being made? Who was saying what, when? What were the topics that were discussed and debated at those times? Given the problems with memory, when using interview methods for these types of questions, triangulating across sources and methods was key, which we were also able to do, as we had picked up a lot information as we were propulsively facilitating along the way. While we were generating both new questions and insights through interviews, as our interlocutors got a better sense for the types of questions we were interested in asking, they also, we believe, got a better sense of us as scholars, and new non-archived archival documents started to emerge. Was this because our questions jogged our interlocutor's memories about documents? Was it because over time they got a sense of our non-declarative practice as serious and invested researchers beyond our declarative statements as such (Lizardo 2017) and that led them to share more? Or was it a bit of both of these? While we suspect it was both of

these and more, the implication was that through our interview work and use of ethnographic tools in the archive (we were talking to people, not *just* retrieving documents), a "shadow archive" of materials began to be made available to us, which again, with the methodological hopping across lily pads we had been doing, was freshly informed by insights from our other approaches.

In total, it was through a deep reading of archival materials that a new research question emerged, which was best answered by bringing tools traditionally associated with ethnographic methods to the archive itself. From that another new question emerged that was better answered by a return to archival material using tools from natural language processing. Yet that method was not equipped to answer some of the new research questions that were introduced from it; so we turned to interviews, the best method available to answer *those* questions. And it was, in fact, in those interviews that we were granted access to a "shadow archive" of non-formally archived materials, which would, ultimately, send us back to deep reading. While our research is still ongoing, these four completed steps in our research serve as an example of one way in which propulsive facilitation can work as a multimethodological research tool in practice.

Discussion

In this work we argue that in addition to confirmatory and complementary approaches in multimethodological work, facilitation holds unique promises for knowledge generation. Most centrally, facilitative approaches can be used *propulsively*, setting the researcher down whole new paths as new findings generate new questions in an iterative and creative process. As different methods are better equipped to answer different questions, this means that to answer those new questions, the researcher must be prepared and equipped to shift methodological gears on the fly. While it is up to the researcher to decide which of their new questions are worthy of attempts at answers, she must always ask herself, in pragmatist fashion, "Which is the best method to answer the question? And is answering that question worth it to me to deploy that method?" While propulsive facilitation leaves open for the researcher what path she will head down and where the research will ultimately go, trying to satisfice (Simon 1956) by using methods for questions they are not well equipped to answer is a door that is closed. In fact, once one opens themselves to a multimethodological facilitative study, trying to make methods do things they are not best equipped to do can be avoided entirely.

We see this approach as adding an explicitly multimethodological framework to the types of iterative thinking and practices that are already common to qualitative scholars, be it in the construction of theory, the relationship between theory and method, or in qualitative methodological approaches more generally. Given how much of qualitative research is predicated on iteration, we find it noteworthy that qualitative scholarship so frequently relies on the hypothetical-deductive framework of triangulation when incorporating multiple methods. To be sure, in advocating for propulsive facilitative approaches we are deliberately sidestepping some methodological tensions (Small 2011). Rather, our point is merely that in addition to Merton's (1968) "serendipity pattern" being an opportunity to refine or apply a new theory, it may also present an occasion to apply a new or alternative method which is better equipped to making sense of the unexpected datum or phenomena one is presented with along the way.

To be clear, just as we do not believe that all research should be multimethod research (Timans et al. 2019), we do not believe that propulsive facilitation is useful for all multimethod

projects, as there are limitations to what we propose. First, while the researcher need not be a methodological jack of all trades, she must be able to responsibly operate in at least two of them, and probably a master of at least one of them. There is also room and perhaps even justification for more team research. Just as interdisciplinarity teams can generate new insights through finding a meeting point across different theoretical disciplinary standpoints, inter-methodological teams within and across disciplines can achieve the same effect to propulsively study a wide variety of social phenomenon. For this reason, we believe that this approach may be particularly useful for collaborative endeavors, and particularly collaborative endeavors in which the collaborators have different methodological strengths and weaknesses. While this type of intellectual diversity is where new ideas can come from (Burt 2004), the distance between can also go too far (Aral and Van Alstyne 2011), and there also may be a trade-off between this type of multimethodological entrepreneuring and productivity. That said, if multimethodological dexterity is the concern, every research project that triangulates could reasonably have propulsively facilitated as well.

Central to our proposal is that different methods are better equipped to answer different questions. For instance, multiple correspondence analysis is not really a good method for making causal claims, just as traditional forced choice survey items are not a good method for disentangling automatic from deliberative cognition (Miles et al., 2019). In the pragmatist approach we take, the methodological approaches we use also come with opportunity costs: if you value your time, ethnography is not a good method to get answers to questions that could just as easily be captured by a survey, just as the time and expense of launching a representative survey is not worth it if the questions that interest you are better captured through ethnography. In this, we loosely follow William James (1981, 43) who asks, "What difference would it practically make to any one if this notion rather than that notion were true?" Thinking this question through in the context of research, one is asked to consider what appreciable difference it might make if one method were used instead of another. There is a looseness to this approach, a pragmatist "multivocality" (Padgett and Ansell 1993) that enables one to pursue the data as it emerges from inquiry.

Given the centrality of pragmatist theorizing on what we propose, we believe there is a natural affinity between propulsive facilitation and abductive analysis (Timmermans and Tavory, 2012, 2014). In fact, just as abductive approaches allow for propulsive interplay between theory and data collection, what we propose extends this same logic to the data collection side of that exchange. While deductive multimethodological studies are probably best suited for confirmatory approaches and inductive multimethodological studies are probably best suited for complementary approaches, we believe that abductive multimethodological studies are best suited for facilitative approaches. None of this means that the researcher cannot make stops for confirmatory and complementary approaches along the wayand instead we only mean to suggest that precision in theoretical revision along the way may oftentimes requires methodological revision along the way as well.

Good sociology, we believe, rarely (if ever) follows a physics model, even though no shortage of good sociology pretends to have done so after the fact. In a propulsive facilitative approach however—which allows the researcher to not have it all or have known it all; which allows the researcher to be wrong, to hit dead ends, to make mistakes, and to retrace steps—the messiness of the real life of data collection is a feature, not a bug that has to be scrubbed out of methods sections to make findings more publishable. We believe that much how acknowledgement of abductive analysis in the relationship between theory and data has given a name and legitimacy for something that many researchers were already doing, for the relationship

between questions, methods, and findings, propulsive facilitation promotes a name for what a fair number of multimethodological qualitative scholars already *are* doing, that is, creating an allowance for referring to our research processes as something that is closer to what they actually may be. We do so here in the context of archival work, which although increasingly used by sociologists across a wide range of subfields, has received less methodological meta-commentary than perhaps any other widely used sociological data source. Like the much-needed meta-commentary on archival work provided by this issue of *Qualitative Sociology*, we believe that our discussion of propulsive facilitation is taking *what people already are doing*, putting a name to it, and taking it seriously and as worthy of discussion.

Compliance with Ethical Standards

Conflict of Interest The authors declare that they have no conflict of interest.

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