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Letter from the Editors:

It is our great pleasure to assume the editorship of *Qualitative and Multi-Method Research* as we bring QMMR to the Southwest. We have long been avid readers of the newsletter, viewing it as an important resource for raising key substantive issues and engaging in the critical debates on how we understand and use qualitative and multi-method research. Such has been its success that said newsletter has now become a stand-alone research publication. We are proud and honored to oversee this transition and feel certain that the insightful contributions within the pages of QMMR, and, of course, their authors, merit this kind of recognition.

We are honored, too, to follow in the footsteps of Alan Jacobs, Tim Büthe, John Gerring, Gary Goertz, and Robert Adcock. It is not lost upon us that their efforts as editors have helped make this the high-quality publication it has now become. Our goal is to follow the precedent that they have set and continue to push QMMR to be on the forefront of scholarly engagement with the approaches to and uses of qualitative and mixed methods.

Toward that end, in this, our first issue, we introduce the new layout for QMMR to further cement the transition from newsletter to research publication. Relatedly, in the final article of this issue, Sebastian Karcher discusses the use of DOIs for providing stable access to articles, and how that has been—and will continue to be—implemented in QMMR. We also announce the launch of QMMR's new website, which can be found at qmmrpublication.com. Readers and contributors can visit the site to find information on the submission process, as well as QMMR updates.

Our first issue includes two series of articles. The first symposium pushes us to reconsider how we undertake comparisons in our scholarship. *Rethinking Comparisons* looks critically at the conventional approach(es) to comparison and notes that these inevitably shape the kind of knowledge we produce. By thinking beyond these conventions, and, indeed, by understanding their limitations, the authors in this symposium offer us a new point of departure for interrogating what, why, and how we compare.

The second set of articles are a panegyric to the remarkable and remarkably impactful scholar, Lee Ann Fujii, who passed away last year. As guest editor, Sarah Parkinson has curated a collection of essays that give voice to memory through their stirring tributes. Lee Ann's passing is, without a doubt, an immeasurable loss for her friends, colleagues, and the discipline. Her scholarship was of the highest quality: nuanced and thoughtful. Her contributions to the field of political science, however, went beyond that. Dr. Fujii challenged all of us to examine the discrimination, hierarchies, and hardships—both explicit and implicit—that the academic world visits upon those from which it has the most to gain. Hers was a voice, as the contributors to this memorial make known, that both acknowledged and empowered the marginalized within academia and outside its “hallowed” halls. Her scholarship, mentorship, and general grace and integrity will be greatly missed. We invite our readers to celebrate her life and remember her work, as it is incumbent upon all of us to continue her legacy.

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Symposium:

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Rethinking Comparison

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The articles in this symposium explore two fundamental questions in the study of politics: (1) why do we compare what we compare; and (2) how do the methodological assumptions we make about why and how we compare shape the knowledge we produce? Qualitative comparative methods—and specifically controlled qualitative comparisons—have been central to some of the most influential works of social science. Controlled comparisons drive studies on phenomena as varied as the preconditions of social revolution (Skocpol 1979), the divergent effects of working class mobilization (Collier and Collier 1991), and the consequences of social capital for state effectiveness (Putnam 1993). Indeed, controlled comparison is such a dominant force in political science methods training that two leading methods scholars note, “Nearly all graduate courses on comparative politics commence with a discussion of Mill’s methods of ‘difference’ and ‘agreement,’” which serve as the foundation for controlled comparative studies (Slater and Ziblatt 2013, 1302).

Yet, even as controlled comparisons have produced lasting insights and continue to dominate research designs, they are not the only form of comparison that scholars utilize. There is little methodological guidance in political science, however, for how to design comparisons that do not rely on control as a central element, and little epistemological insight on why such comparisons might

be compelling. As a result, scholars often eschew research designs premised on non-controlled comparisons and rarely explain the utility of such comparisons when they do. The consequences for knowledge are severe. When we limit the kinds of comparisons we make, we necessarily constrain the questions we ask and limit the knowledge we produce (Ragin 2004, 128).

The articles in this symposium reopen the conversation on comparison by exploring logics of comparisons that are not motivated by control. They ask, what kinds of questions lend themselves to non-controlled comparisons? How should we think through case selection? What kinds of insights about the world are non-controlled comparisons positioned to produce?

Three central components of the comparative method frame this discussion. First, the articles encourage political scientists to rethink what a case is. They do so by challenging dominant geographic conceptions of cases and engaging alternative types of cases, including political processes (how things happen), practices (what people do), meanings (how people interact with symbolic systems), and concepts (how people order the world). Second, the articles expand our notion of what it means to compare. They push political scientists to conceptualize comparison as a method that includes greater attention to the lived experiences of the people we study, the political concepts they deploy, and the ways those experiences and concepts shape their political

worlds. Finally, the papers expand the explanatory goals of political science. While many studies emphasize variations in outcomes (and we often encourage graduate students to think in these terms), these articles expand the possibilities to include variations (or lack thereof) in political processes, practices, meanings, and concepts.

Given how powerful controlled comparisons have been for producing knowledge, why rethink the practice of comparison? Our intention in laying out the value of non-controlled approaches to comparison is not to deny the utility of existing modes of comparison. Rather, it is to begin specifying logics of comparative inquiry that would be available to scholars beyond the already well-defined logics of controlled comparison. In so doing, we would suggest that by expanding modes of qualitative comparative inquiry, social scientists can both uncover new questions and drive innovations in how we answer existing questions. Rethinking comparison may also encourage us to revisit the kinds of sweeping questions that animated scholars as varied as Benedict Anderson (1983), Samuel Huntington (1968), and Charles Tilly (1990), but which do not necessarily lend themselves to controlled comparisons. Indeed, each of these scholars made major contributions to our understandings of politics while eschewing controlled comparisons. It is often difficult to tackle ambitious questions about power and governance while looking for cases that meet the standards of controlled comparison. Comparisons do not merely reflect the field and its subjects, they serve to constitute both. Therefore, if we can expand how we think about comparison, we can expand how we think about the world, and that will improve our understanding of it as a result.

Comparison: Controlled and Uncontrolled

Our interest in rethinking comparison emerges amid a revival of qualitative methods in political science, generally (Wedeen 2002; Mahoney and Rueschemeyer 2003; George and Bennett 2005; Gerring 2006; Schatz 2009; Brady and Collier 2010; Mahoney 2010; Goertz and Mahoney 2012; Ahmed and Sil 2012), and a renewed focus on controlled or paired comparisons, specifically (Snyder 2001; Tarrow 2010; Dunning 2012; Slater and Ziblatt 2013; Gisselquist 2014). Building on foundational work on controlled comparison (Przeworski and Teune

1970; Lijphart 1971, 1975; Skocpol and Somers 1980; Brady and Collier 2010; Slater and Ziblatt 2013), and the closely related strategies of paired (Tarrow 2010) and subnational (Snyder 2001) comparison, this recent work shows how the method can combine the best of both qualitative and quantitative epistemologies. Specifically, controlled comparisons allow scholars to trace dynamic causal processes, while accounting for the effects of confounding explanations, enabling generalizable arguments.¹ Not surprisingly, controlled comparative approaches dominate current best practices in qualitative research. Graduate students and professors alike look to select cases that hold alternative explanations constant or leverage variation in initial conditions or outcomes.² Indeed, Slater and Ziblatt (2013, 1302) note the “enduring ubiquity” of the strategy in qualitative comparative research.

We agree that controlled comparisons have important utility for scholars engaging in small-N work. Contemporary scholars have used controlled comparison to shed light on state capacity (Slater 2010), ethnic violence (Wilkinson 2006), and indigenous mobilization (Yashar 2005), just to name a few. Yet even as controlled comparisons have produced some of our most influential theories of politics, some scholars have been critical of their limitations. Scholars utilizing quantitative analysis have argued that research based on controlled comparison has limited the ability to generalize (King, Keohane, and Verba 1994; Geddes 2003), a problem that scholars utilizing a mixture of quantitative and qualitative methods have tried to anticipate through “nested” research designs (Lieberman 2005). Scholars working from various qualitative traditions, by contrast, have argued that projects deploying controlled comparisons overemphasize their ability to address confounding explanations, while necessarily underemphasizing processes of diffusion (Sewell 1985, 1996a) and interaction (Lieberman 1991, 1994). The concern is that studies that rely on controlled comparisons may not be as predictive and testable as claimed (Burawoy 1989) and may push scholars to ignore research questions that do not immediately evidence variation that can be explained (Ragin 2004, 128).

1 This kind of comparison, often called the method of agreement and the method of difference, continues to reference Mill (1843), although scholars often fail to acknowledge Mill’s own discussion of the limitations of the approach (for an exception, see George and Bennett 2005). Regardless, what are often invoked as Mill’s methods of difference and agreement are ubiquitous in qualitative comparative work (for a discussion, see Slater and Ziblatt 2013) and remain central to the ways in which we question and evaluate comparative case research.

2 The approach to comparison and process tracing that George and Bennett (2005) lay out, and the qualitative comparative analysis methods that Ragin (2014) pioneered, are important exceptions here.

However, even as scholars have developed important critiques of controlled comparisons, they have been less effective in developing alternative approaches to comparison.³ A wide range of approaches to comparison appears in some of the most influential contemporary work across political science subfields. Yet, the logics behind the comparisons at the heart of these studies are rarely laid out and explored. Think, for example, of Benedict Anderson's (1990; see also 2016) important work comparing ideas of power in Javanese and European political thought. Comparison between Java and Europe violates virtually every tenet of how comparison should be executed. Anderson writes across different scales (an island versus a continent), different regime types (a monarchy and subsequent dictatorships versus a wide variety of regimes), and different religious traditions (an Islamic system with animist elements versus largely Christian systems). Yet, despite the lack of control, Anderson harnesses the friction between the different concepts of power to illuminate how ideas differently structure political practice in both settings. Had Anderson approached the comparison through the logic of control, he would not have been able to generate these insights. At the same time, however, it is not clear what the epistemology underlying these comparisons is, or why they are persuasive or insightful.

The important role that non-controlled comparisons can play in shedding light on politics is not limited to Anderson's work. For example, Mahmood Mamdani (1996) shows the similarly perverse legacies of indirect colonial rule through a comparison of South Africa and Uganda; McAdam, Tarrow, and Tilly (2001, Chapter 4) compare the Mau Mau revolt in Kenya to the Yellow Revolution in the Philippines to show how similar causal mechanisms were at work in very different episodes of contention; and James Scott (1998) compares German forestry management practices and the planning of Brasilia, among other things, to illustrate the effects of high modernist politics. Yet none of these scholars engages in a concrete discussion of their comparative strategy and why it makes sense for answering their research question.

This gap leaves graduate students and faculty alike without the tools to explain why their research designs—even absent variation on the dependent variable or intended to control for alternative explanations—will produce important insights. As a result, such work is risky. Because their logic is not broadly understood, such studies are often reserved for senior scholars with

well-established reputations, are published in outlets not necessarily geared towards political scientists, like area studies journals, or are simply dismissed. By elaborating why such comparisons should be compelling and providing scholars with a vocabulary to describe their approach, the articles included in this symposium begin to provide a foundation for expanding the possibilities of comparative inquiry.

Thus, even as we recognize the strengths of controlled comparison, the articles collected in this symposium make the case that non-controlled comparisons offer compelling theoretical contributions to our understandings of politics. They also take initial steps toward developing some of the different logics that might drive this kind of analysis. Where existing critiques focus on the challenges that controlled comparisons pose for researchers looking to generalize findings or to embrace causal complexity in historical analysis (see Slater and Simmons 2010), the articles collected here go one step further by encouraging additional types of comparison that are typically not addressed in comparative research strategies, including comparison of processes, practices, meanings, and concepts. Even as the essays in this symposium offer only initial elaborations of the varying logics driving non-controlled comparison, they make a strong case for recognizing the value of these kinds of comparison. The potential implications for political science research are significant. Not only does attention to different modes of comparison open new paths for political science research for example, by comparing perspectively versus juxtapositionally, as suggested in Schaffer's contribution to this volume—it may allow us to ask new questions altogether.

Expanding the Possibilities for Comparison

Collectively, the articles in this symposium suggest that scholars can rethink comparison by reframing how we understand what is to be compared, how to choose comparisons, and how the comparisons we choose advance our knowledge of the world. That is, scholars could rethink the most basic elements through which comparison is practiced and the ends to which it is pursued. Even as much work deploys non-controlled comparisons to make powerful arguments about politics, this symposium seeks to develop new logics that those not engaging in controlled comparisons can use to describe what their units of analysis are, how they select cases to make relevant comparisons, and what the goals of utilizing these new modes of comparison will be.

3 Tilly's (1984), Sewell's (1996a, 1996b, 2005), Locke and Thelen's (1995), and Ragin's (2004) work are important exceptions here.

What is to be Compared

While we agree that “the dazzling array of divergences and convergences across nation-states in the modern world...has long drawn scholars to the craft of comparative politics” (Slater and Ziblatt 2013, 1302), we argue that we should challenge ourselves to be open to how we think of the kinds of divergences and convergences to analyze. Political scientists often talk of “units of analysis” when they analyze these divergences and convergences—geographical areas and organizations are common examples. However, we would challenge scholars to think about the objects they choose, not simply as pre-existing units waiting to be compared, but as dynamic objects being actively created by the researcher.

In this symposium, Joe Soss poses this challenge most directly by proposing that we rethink the building blocks of comparison: cases. Soss highlights the tensions between the purposive, analytic fashion in which political scientists typically think of comparative cases and the dynamic and iterative process of “casing” that characterizes ethnography and interpretive research, broadly. Rather than conceptualize cases and approach research sites as predetermined variables and values, Soss proposes a more discovery-oriented approach through which scholars can draw on immersive experiences to ask, “what is this a case of?” Identifying what is to be compared through this lens would not only facilitate the more iterative approach that already defines much social science research (Koivu and Kimball 2015); it would also allow scholars to remain open to new concepts, meanings, processes, and outcomes that may enrich their own studies and contribute to scholarly knowledge more generally.

How to Choose Comparisons

Once we have a sense of what we are going to compare, the next step is to think about how we choose comparisons. Typically, political scientists use comparison to explain a given outcome. As a result, they tend to select cases by looking for variation on the outcome of interest or to control for alternative explanations.⁴ Yet, we cannot divorce the characteristics of a practice from the context in which that practice takes place. Indeed, in controlling away context we may overlook factors that play a critical role in producing the outcomes we study or dismiss potentially illuminating comparisons because they are

too different to generate even the illusion of control (see Simmons and Smith 2017). Therefore, we need to think very differently about what it means to select cases and what the relevant comparisons are. Rather than artificially controlling for potentially confounding variables, an expanded approach to comparison could challenge scholars to embrace and exploit tensions presented by complex causal or meaning-making processes.

Htun and Jensenius’ contribution to this symposium, which describes their ongoing research on women’s empowerment, exemplifies this expansive notion of case selection. To understand the consequences of states’ gender equity programs, they examine places where women’s empowerment means very different things (the United States, Norway, and Japan). Htun and Jensenius choose cases not for their variation on independent or dependent variables, but because they represent “extreme” visions of women’s empowerment. Through these “extreme” cases, Htun and Jensenius illustrate how the choice of comparisons can help us rethink conceptual categories and discover the right questions to ask in the first place—here, asking how different meanings of women’s empowerment have pushed countries to pursue radically different policies to achieve these diverse visions. More broadly, this approach provides a blueprint for how to think about case comparisons for the purposes of theory development, particularly when the concepts in question are difficult to operationalize and measure.

This notion of discovering concepts through comparison is the key insight of Frederic Schaffer’s contribution to the symposium. Schaffer reflects on the multiple forms of comparison political scientists deploy. Scholars most often understand themselves to be comparing *juxtapositionally*: placing “similar kinds of things side by side in order to catalog their similarities and differences.” Schaffer shows, though, how (often unstated) *perspectival* comparisons—“[analogies] between different kinds of things as a way to establish a vantage point from which to view one thing in terms of the other”—unwittingly shape how political scientists approach their work. For example, understanding the state as an “artificial man,” as Hobbes does, or examining politics as a “game,” as formal theorists do, carries theoretical judgments of which scholars may be unaware. By elaborating the effects of perspectival comparison, Schaffer challenges scholars to reflect on the implicit analogies that ground the juxtapositional comparisons

4 Some works of comparative historical analysis select cases to understand how different parallel processes happen (see Skocpol and Somers 1980), and many political scientists pay close attention to political processes in time to explain divergences in outcomes (e.g. Collier and Collier 1991; Mahoney 2001; Slater 2010; see also Pierson 2004). Comparatively few, though, take the process itself as the “outcome to be explained.”

they construct. Perhaps even more powerfully, thinking about perspectival comparison as a valuable mode of comparison in its own right may help us *see* politics very differently—for instance, viewing the state as akin to a protection racket that extorts citizens for tax money in exchange for protection (Tilly 1985) or, from the vantage point of many young men of color encountering the police in democratic states, apprehending the state as closer to a giant vigilante organization that extrajudicially punishes them in the service of “protecting” other citizens (Smith 2019).

Building Knowledge through Comparison

Rethinking units of analysis and relevant comparisons may also involve rethinking explanatory goals of comparison. Typically, scholars use comparison to explain variation in a given political outcome. The forms of comparison that this symposium explores, by contrast, seek to expand the conceptual and theoretical categories through which politics are thought in the first place. Put differently, instead of being variation-seeking, the goals of comparison might be concept-seeking (Schaffer 2015).

In this symposium, Seawright elaborates the range of methodological objectives that might ground qualitative case-comparison, which include efforts to “sharpen conceptualization and measurement, allow exploration of the prevalence of a particular arrangement of causal capacities, and provide the raw materials for the construction of theories of causal moderation” (this issue, 12). While a critical methodological literature focuses on the deficiencies of qualitative case-comparison for causal inference, Seawright contends that it has ignored the crucial role qualitative comparison plays, not only in refining concepts, but in delineating the “micro-components” of causal explanation—that is, the “configurations of entities and causal capacities” which are bound to differ across contexts. In so doing, Seawright offers a valuable way to frame the utility of in-depth qualitative comparisons: they can shed light on the causal sequences operative in a case, and “no evidence from other cases need ever trouble that conclusion” (this issue, 13).

Read’s article places this broader discussion into an empirical context that reflects unique opportunities for creative comparison: the cases of China and Taiwan. Though Taiwan’s political transformation disrupted possibilities for control in cross-Strait research, Read contends that these changes present a chance to rethink how scholars frame political phenomena and build concepts. Read illustrates this through several

examples of cross-Strait research, including his own on neighborhood organizations. He found that, despite differences in organizational accountability, there were broad parallels between resident perceptions in China and Taiwan because of “similar webs of interpersonal networks” and a shared “vision of the proper state-society relationship” (this issue, 36). This discovery led Read to reframe his study as one on “administrative grassroots engagement”—a phenomenon he would not have developed had he eschewed the China-Taiwan comparison due to regime differences. Read thus illustrates how political transformations can present fruitful opportunities for conceptual and theoretical innovation, rather than exacerbate concerns of conceptual stretching and the violation of control. Social, political, and economic transformations may disrupt comparisons based on conventional categories of analysis, but they can also provide scholars opportunities to derive new insights from previously unexplored forms of comparison.

Rethinking the Building Blocks of Comparison

Rethinking what we compare, how we compare, and the concepts upon which we compare has potentially revolutionary consequences for the study of politics. In *The Spectre of Comparisons*, for example, Benedict Anderson (1998, 2) describes a moment early in his fieldwork where he encountered the Indonesian leader Sukarno’s interpretation of European history for the first time and how the seeming strangeness of Sukarno’s view of European leadership and nationalism induced “a kind of vertigo” in which, “For the first time in my young life I had been invited to see my Europe as through an inverted telescope.” This moment fundamentally restructured how he understood European politics. It is the type of vertigo-inducing encounter that the authors in this symposium may have had in conducting their own studies and which likely pushed them to shy away from “traditional” modes of comparison. Yet, even as scholars use un-controlled comparisons, there is a lack of language to describe the comparisons they deploy, nor are there clarified logics to guide future researchers looking to make similar comparisons. The goal of this symposium is, therefore, to start articulating why non-controlled comparisons can be compelling, when non-controlled comparisons might be helpful, and how non-controlled comparisons can be conducted.

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Beyond Mill: Why Cross-Case Qualitative Causal Inference Is Weak, and Why We Should Still Compare

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Qualitative cross-case comparisons were once widespread and respected enough to be described as “the comparative method.” However, the current wave of research on qualitative methods has seen cross-case controlled comparisons fall substantially in esteem. Early criticisms based on selection bias by Geddes (1990) and King, Keohane, and Verba (1994) have been disputed and no longer receive sustained attention in the qualitative methods literature. A more recent argument is that qualitative comparison fails for purposes of causal inference because the required assumptions are simply implausible and because statistical methods are superior tools for the same purpose. Sekhon (2004) argues that comparisons based on Mill-type methods will always be susceptible to probabilistic alternative hypotheses, which generally cannot be reasonably evaluated using qualitative cross-case comparisons. George and Bennett (2004, 151–79) argue at length that “practically all efforts to make use of the controlled comparison method fail to achieve its strict requirements,” and that various within-case qualitative methods are simply more usable than qualitative cross-case comparisons. Collier, Mahoney, and Seawright (2004) characterize many forms of qualitative cross-case comparisons as a form of “intuitive regression” that acts inferentially as a weaker and problem-laden equivalent of statistical analysis. Seawright (2016, 107–9) argues briefly that a potential-outcomes formulation makes evident that qualitative comparisons are exceptionally weak tools for causal inference.

This critical tradition coexists with sustained use of paired and otherwise grouped comparison in qualitative research, as Slater and Ziblatt (2013, 1302–3, 1307–10) demonstrate. This essay argues that the existing critical literature has been insufficiently attentive to the range of justifying assumptions that qualitative scholars might make in thinking about qualitative cross-case comparison, but also that careful consideration reinforces the view that such assumptions are generally implausible. It then goes on to argue that cross-case controlled comparisons in qualitative research have real value for other

methodological objectives, value that has not been fully articulated or respected in the existing literature.

Comparison for Causal Inference

For many contemporary definitions of causation, there is no inherent, logical connection between the method of comparison across cases and the goal of causal inference. Some traditions of thought about causation, dating at least back to Hume and including a sequence of thinkers up to Baumgartner’s (2008) contemporary work, focus on regularities across cases, arguing that causation is nothing but a certain pattern of predictable relationships between variables across a population of cases. Given this definition, it is clear that methods for causal inference would need to rely centrally on comparison. Indeed, it seems that such a view would rule out any methods other than comparison, as any kind of within-case analysis seems to be, at most, weakly related to the existence of reliable cross-case patterns.

However, influential alternative perspectives are available. Quantitative and statistical thinking about causation in the social sciences is currently dominated by a synthesis of counterfactual- and manipulation-based approaches (Rubin 1974; Holland 1986; Woodward 2003). Here, causation is not inherently about differences across cases, and cross-case comparison is at most a contingent tool for causal inference rather than part of the definitional core of the concept. Instead, causation is ultimately, if perhaps unobservably, about what would have happened within a single case had the treatment (or main independent variable) of interest been manipulated to take on a different value than it, in fact, did. That is, causation is always about the difference between what happened and what would have happened had a particular, well-defined choice been made differently.

A common mathematical notation has emerged around this way of thinking. The inherently counterfactual nature of causation in this framework is captured by the creation of multiple versions of the dependent variable for each case. If case i receives the treatment (which is an often arbitrarily chosen value from the range of the

main independent variables), the dependent variable that occurs is Y_{iT} . On the other hand, assuming a binary independent variable for simplicity, if case i receives the control, the observed value of the outcome is Y_{iC} . The true causal effect of the main independent variable for case i is, therefore, $Y_{iT} - Y_{iC}$.

Differences that Balance within a Group

The qualitative controlled comparison is sometimes analogized with statistical matching methods for causal inference (Seawright and Gerring 2008; Nielsen 2016). Hence, it is worth looking closely at the assumptions for causal inference made when using these methods. Are they a viable justification for qualitative cross-case comparison?

Matching methods, like most statistical techniques, rely on an analogy to experimental research designs for causal inference. In experiments, the combination of random assignment and the law of large numbers guarantees that the average value of Y_{iT} in the treatment group will be very similar to the average (unobservable) value of Y_{iT} in the control group. The same logic holds true for Y_{iC} . Hence, the difference between the observed group average values of Y_{iT} and Y_{iC} is close to the true causal effect between the two groups.

The comparability of the treatment and control groups within a social-science experiment does not arise because each case in the experiment is interchangeable. Individuals, who are usually the cases in experiments, obviously differ from each other in unlimited ways. These differences are accommodated because they balance out on average. Causal inference works because random assignment, on average, balances individual differences within the treatment and control groups. Furthermore, significance testing of various kinds offers a framework for handling the inevitable real-world imbalances that arise in experiments with finite sample sizes.

Matching methods cannot appeal to random assignment to guarantee that differences across cases will balance out within the treatment and control groups. Instead, scholars using matching make a brute-force assumption that, after creating group balance across a fixed set of control variables, all other differences will balance out within each group. This assumption is difficult to justify; unmeasured or neglected variables seem to routinely fail to balance. Nonetheless, causal inference is possible with these methods as long as all relevant differences either (a) are included in the set of measured control variables for matching, or (b) happen to balance out within the treatment group and within the control group.

This logic, fragile as it is, is all but unavailable to qualitative scholars. In a paired comparison, the treatment and control groups each consist of a single case. Obviously, nothing can “balance out” statistically within a single instance. If the treatment case is, for example, unusually liberal, then the treatment group will simply be unusually liberal.

Thus, paired comparisons must seek some other justification for causal inference. Small-group qualitative comparisons are likewise obliged unless the cases under comparison are exceptionally simple. Finally, significance testing cannot offer to mitigate these problems, given that small-N controlled comparisons virtually never feature enough cases for such tests.

Differences that Balance within a Case

How, then, might scholars justify such qualitative comparisons? If the treatment case is just irreducibly different from the control case in ways other than the main independent variable, is causal inference possible?

In a paired comparison, causal inference revolves around $Y_{1,T} - Y_{2,C}$, where case 1 is the treatment case and 2 is the control. This quantity will correctly describe causal matters for case 1 if $Y_{1,T} - Y_{2,C} = Y_{1,T} - Y_{1,C}$. Thus, the inference requires that $Y_{2,C} = Y_{1,C}$. This is a strikingly stringent requirement: the two cases simply cannot differ from each other in terms of the outcome they would experience under the control. Indeed, while various quantitative approaches to causal inference require assumptions that are difficult to meet, or that are even implausible, this assumption is more restrictive than those required for any widely-used quantitative technique. Regression analysis, for example, allows for causal inference in the face of random measurement error or omitted cases that are not confounders; this assumption cannot succeed in the face of either of these problems. Thus, controlled comparison requires the same kind of assumption as regression, but a much stronger version of it.

One assumption that might meet this condition is that the differences between the treatment and control cases balance each other out within each of the two cases. If one unusual feature of case 1 adds, say, three points to $Y_{1,T}$ and $Y_{1,C}$, but the second (and only other) unusual feature subtracts three points, then taken as a whole, case 1 poses no problems for causal inference. Of course, there is no special reason to expect that differences within a case will tend to balance, as opposed to accumulate.

Differences that Barely Matter

Perhaps the most frequent informally expressed justification for causal inference via qualitative comparison involves the idea that two cases may not be identical, and the distinctive features of each case may not internally balance, but that causal inference will still work if the consequences of the differences are modest enough. Perhaps $Y_{2,C}$ does not equal $Y_{1,C}$, but the causal inference will still be acceptable if $Y_{2,C}$ is very close to $Y_{1,C}$ —a condition that is met if the causal effects of differences between the cases on the outcome of interest are all quite small. What counts as “very close” is relative to the size of the true causal effect: any other differences must have effects that are a tiny fraction of the effect of interest, or the inference will be meaningfully distorted.

This setup can seem reasonable. Surely case experts are likely to focus on important differences, and may well have the knowledge necessary to pair up broadly similar cases. Nevertheless, this argument results in disturbingly fragile causal inferences. Because the causal inference is only approximately correct if the causal effect in question is large and the effects of all other differences between cases are small, it will only be persuasive to scholars who are already firmly convinced that the main independent variable is the biggest cause of the outcome. Any readers who are instead open to the alternative hypothesis that there are some other causes of comparable importance to the main independent variable cannot avoid worrying that the causal inference is biased by the differences between the cases.

No Differences

Finally, and most starkly, qualitative causal inference will succeed if there are simply no differences between the cases under comparison. If the treatment case and the control case are exactly identical in every way that is causally relevant to the outcome, then the causal inference will succeed. This condition, known as causal homogeneity, seems to capture a common interpretation of what J.S. Mill intended with the method of difference. In some kinds of physical science laboratories, careful procedure can more or less achieve exact interchangeability between a treatment and a control sample, allowing a direct pairwise comparison to support causal inference. It seems self-evidently problematic to identify a pair of human beings, let alone any larger social aggregate or institution, as comparably interchangeable.

Billionaires and the Causal Role of Public Pressure

To illustrate the problems with these four assumptions justifying qualitative comparison for causal inference, consider a paired comparison between two politically conservative American billionaires, David Koch and John Menard, Jr.¹ In a broad perspective, these individuals have an enormous amount in common. They are immersed in the shared political culture of the 21st-century United States. They share an elite socioeconomic position. They have overlapping social networks and quite convergent political views, as evidenced by Menard’s participation in a series of seminars sponsored by Koch and his brother.

These and many more similarities notwithstanding, Menard and Koch have crucial differences that matter for understanding American billionaires’ participatory strategies. While both Menard and Koch are heavily invested in conservative economic politics, Koch’s views have received substantially more public attention and indirect defense through his foundations, support for scholarship, and even a handful of public statements. Menard’s political perspectives, by contrast, have not been given deliberate public airing—and instead have emerged via investigative journalism and legal action. What explains this contrast in the two billionaires’ willingness to engage in stealth politics (Page, Seawright, and Lacombe 2018), i.e., participatory strategies that evade public scrutiny and offer little or no deliberative defense of one’s policy preferences?

One interesting explanatory possibility is that the difference is accounted for by the extent to which the two billionaires’ wealth depends on public-facing businesses and brands. Menard’s wealth is founded in the success of his eponymous chain of home improvement stores. As such, publicly visible political action might carry the risk of boycotts or general consumer distaste of a sort that could hurt Menard economically. Koch, by contrast, draws his wealth in substantial part from the energy industry, but also from a number of behind-the-scenes investments. Because Koch’s wealth mostly comes from industries that sell to other industries, and depends little on his personal brand, he has limited economic exposure to boycotts or other forms of consumer rejection. Hence, it may be unsurprising that Koch is willing to participate in more potentially visible ways than Menard: Koch simply has less to lose.

The key issue for this essay is, of course, not whether this explanation is true or false, but rather whether any of the four assumptions characterized in the previous

1 Material in this section draws on Page, Seawright, and Lacombe (2018).

section are applicable and can justify causal inference with this comparison. It should be immediately clear that the first assumption, that differences will balance within groups, is not applicable. There is only one billionaire within each group—and while it would certainly be possible to expand the analysis to include more billionaires, reaching the sample sizes that would justify use of the law of large numbers essentially precludes qualitative treatment of the comparison.

What of the assumption that differences balance within cases? Even evaluating this assumption would require exceptional prior causal knowledge. Potentially relevant contrasts between Menard and Koch are numerous and varied. The two billionaires differ in terms of family backgrounds, with Koch coming from a successful family with an established (if not yet world-dominant) business, while Menard was born to solidly middle class parents. They differ in birthplaces, as well, although perhaps in ways that will only be legible to Midwesterners: Menard was born in Eau Claire, Wisconsin, while Koch comes from Wichita, Kansas. They depart substantially in terms of their current places of residence and cultural interests. Menard still lives in Wisconsin, and has long sponsored a team that competes in the Indy Racing League. Koch, by contrast, lives on 740 Park Avenue in Manhattan, and is famous for his substantial philanthropic gifts to support cancer research, the New York and Washington, D.C., arts and museums scenes, and a public broadcasting foundation.

Do these and other differences between the billionaires exactly cancel out? While it would be fortuitous if they did, the fact is that even determining the answer would require remarkable prior causal knowledge. Does an affluent as opposed to middle class origin predispose a billionaire to greater public political visibility, or does the causal effect run in the other direction? Would Koch's philanthropic efforts create connections with Manhattan social life that increase the potential costs of visible political activism, or would philanthropy create a buffer against criticism? The issues involved in even deciding whether the assumption is met are immense and probably, at present, insurmountable.

The exact same challenge destroys any potential applicability of the third assumption, that the differences between these two billionaires barely matters. I might invite the reader to believe that public- versus industry-facing primary sources of wealth have much more powerful effects on political participatory strategy than do social networks, family histories, and so forth. Yet what of the inevitable reader who disagrees? For any reader

who sees social networks as potentially as important, the comparison crumbles. A scholar might respond by selecting a comparison between billionaires with similar social networks—but this problem will remain as long as any difference whatsoever persists between the billionaires. At the ultimate limit, imagine a pair of identical-twin billionaires raised in the same household, and residing in the same condominium building, but with one of them for some reason heavily invested in consumer-facing enterprises and the other not. Even though these hypothetical billionaires are similar to the point of fantasy, it nonetheless remains certain that they will have subtle, but potentially relevant, differences. Their social networks will not be identical. They will have slightly divergent sets of politically relevant information. Some life experiences will not be shared, and so forth. To claim that these differences *must* have smaller effects on participatory strategies than the public- vs. industry-facing contrast is to assert *a priori* that the causal effect of interest is relatively large. Thus, this assumption becomes uncomfortably close to circular.

Finally, the discussion over the last paragraphs should absolutely suffice to reject any notion that Menard and Koch are identical in all ways other than whether their businesses face the consumer public. Billionaires are not chemical samples, and there is simply no prospect for interchangeability. Thus, it would seem that prospects for causal inference from a paired comparison between Menard and Koch are grim. One might tinker at the margins by selecting slightly more similar pairs of billionaires, but the basic issues encountered here will persist.

Yet if qualitative comparisons are a hopeless strategy for causal inference about billionaires, prospects are surely grim for virtually any other application of this design in the social sciences. Among the overall population of humans, after all, American billionaires are a remarkably homogeneous group with exceptional similarities in culture, class, and context. Simply put, qualitative comparison appears to have little to offer as a tool of direct causal inference because the required assumptions are implausible, at best, in the social sciences.

The Value in Comparison

Of course, it certainly does not follow from this argument that qualitative comparison is useless, or that existing qualitative work featuring controlled comparison needs to be discarded altogether. Rather, the value of comparison arises from goals other than direct causal inference. Here, I will highlight three ways that qualitative

comparisons make social-scientific contributions. Such comparisons are valuable because they: sharpen conceptualizations and measurement; allow exploration of the prevalence of causal capacities; and provide raw materials for the construction of theories of causal moderation. It makes sense to reread existing studies of this sort along such lines, even perhaps against their authors' intentions, and it is emphatically reasonable to design future qualitative comparisons with these alternative goals in mind.

To begin with, as Slater and Zibblatt (2013, 1312) note, comparison facilitates conceptualization and measurement by providing empirical content and grounding for theoretical contrasts. There is value in using qualitative comparisons to understand the meaning and real scope of possible different outcomes with respect to a dependent variable. Qualitative comparison can allow inductive discovery related to that scope in ways that are hard to replicate with other methods.

Slater and Zibblatt (2013) argue at length for another advantage of qualitative comparison: providing external validity for causal inferences based on (qualitative or quantitative) single-country analysis. Their argument demonstrates decisively that scholars routinely use qualitative comparison for this purpose, and that such studies are often well received by their respective research communities. Yet there are certain tensions involved in the discussion that result from conceptual messiness related to the idea of external validity.

External validity is sometimes discussed in terms of sample-to-population statistical inference. It is deeply unclear that qualitative comparison could ever provide external validity in a statistical sense. Qualitative cases are rarely randomly sampled, and even if they were, it would be exceptional for qualitative analysis to include enough cases to acquire attractive statistical properties.

In order to speak of external validity in the context of qualitative comparison, a reframing is needed. Slater and Zibblatt (2013, 1314) helpfully reformulate the concept: "If an argument deriving from a controlled comparison is stated in terms of general variables and can be shown to shed explanatory light on specific cases outside the original sample, then the original argument can be said to enjoy external validity." Here, external validity becomes a sliding scale: an argument scores higher to the extent that it applies to more cases, and also presumably to the extent that it throws a brighter "explanatory light" on each case.

What does "explanatory light" consist of? By usage in the quoted passage, and by Slater and Zibblatt's deployment

of related terms throughout, "explanatory light" appears to be a relation between an explanation and a case. At one point, they gloss this feature as involving "verisimilitude on causal mechanisms." Unfortunately, this might mean a number of different things: highly detailed theories of causal pathways; extensive and persuasive pattern-matching (Campbell 1966); evidence that a given case exemplifies a theorized causal pathway; or evidence justifying an overall causal inference regarding an entire theoretical model's causal correctness vis-a-vis a given case (Waldner 2015), to name a few.

On the supposition that Slater and Zibblatt are referring to causal pathways, i.e., sequences of variables with causal linkages that may serve to fill in steps between the treatment and the outcome within a given case, there is another difficulty. Two cases might have very different results even if they experience the same causal pathways in the sense that each is affected by genuine causal linkages from the treatment variable, through one or more shared mediator variables, to the outcome. This is because the size of the causal effects involved in each relation need not be constant across cases. Background facts about a given case may render it more or less susceptible to a particular causal effect, and thus may change the magnitude of causal patterns without altering their form. Such changes in magnitude should probably be seen as altering the degree to which a theory explains a given case, as would differences in the nature of the observed mediator variables. But such sequential causal inferences are difficult at best. It is not clear that qualitative research is a powerful tool for quantifying causal magnitudes, and it is also unclear how one might trade off between magnitudes of effects versus identities of mediators in evaluating explanatory fit.

Ultimately, I am unpersuaded that there is value in applying the concept of external validity to qualitative cross-case comparisons. If a causal theory is correct for a given case, then it cannot be made untrue by results in another case; nor can a theory that is false for a given case be made true by its performance in other cases. Consideration of a theory's explanatory value outside the core case or cases of interest is thus neither a matter of statistical generalization, nor of testing the theory's validity.

Contemporary theories of causation provide a helpful set of tools for reframing this issue in ways that are arguably compatible with, although more extensive than, the potential-outcomes framework adopted earlier (e.g., Seawright 2016, 29–30; Cartwright 2009). Cartwright (2007), in particular, pushes us to think of causation as

context-specific arrangements of objects, institutions, actors, and so forth in such a way that the well-known capacities of each specific entity interact to generate the outcome of interest. Because configurations of entities and their causal capacities differ across contexts as a brute fact, there is no reason why valid causal arguments should be expected to be universal, or indeed even to be valid more than once. Thus, in Cartwright's view, good causal theories are those which identify relevant entities and correctly describe their causal capacities and arrangements. These micro-components of a causal explanation can be real and theories about their capacities can be true, but because of the prevalence of difference in arrangements across cases, generalized causal theories or laws are false (Cartwright 1983). Rather, the kinds of causal findings that result from an experiment, a case study, or most other approaches are true or false relative to some given "nomological machine" or specific arrangement of causal capacities (Pemberton and Cartwright 2014).

From such a position of central concern for entities and their causal capacities, Slater and Ziblatt's concern for external validity can be helpfully recharacterized as understanding the prevalence of the causal capacities central to a theory across a range of cases. In-depth qualitative and/or quantitative analysis of a single case might give us good reason to believe that a particular causal arrangement is operative in that case, and no evidence from other cases need ever trouble that conclusion. Yet it remains instructive to ask whether there are other cases in which the same entities demonstrate the theorized causal capacities. While a causal explanation can be valid and nonetheless unique to a single case, there is an obvious gain to credibility when explanations involve common, easily demonstrated capacities. For this task, qualitative comparison can contribute.

Of course, social scientists are rarely satisfied to note that one set of cases is simply causally different from another. If causal capacities are arranged one way in a first domain, and another way in a second, it is reasonable and perhaps compelling to ask why the domains differ. This kind of second-order problem of causal theory involves the project of understanding relations of causal moderation, i.e., the background variable or variables that cause cases to differ in terms of the main theory's network of causal relations and capacities. Building (and perhaps to some extent testing) theories about moderation is in itself a highly valuable goal.

Furthermore, it is easy to interpret much of the use of qualitative comparison in the comparative-historical

literature in political science and sociology as carrying out (to varying degrees) careful within-case causal inference and then using comparison to structure theory-building about moderation. Consider, for example, Collier and Collier's (1991) study of labor, parties, and regimes in early- to mid-20th-century Latin America, a classic that serves as one of Slater and Ziblatt's motivating examples. In that study, the treatment variable is labor incorporation (i.e., the inclusion of labor unions as part of the legal political system), and the outcomes involve patterns of party-system formation and certain trajectories of regime dynamics. The central argument of the volume is, in fact, that labor incorporation, a shared event across the eight countries in the study, does *not* have the same effects or activate the same causal dynamics across the region. Instead, Collier and Collier argue that certain background characteristics have a tendency to cause countries to incorporate labor in different ways, and that these different modes of incorporation produce divergent arrangements of causal capacities. The bulk of the study consists of careful within-case analysis that attempts to establish the actual causal effects for each case, and the cross-case comparisons that frame the volume can easily be understood as building a theory of causal moderation (a theory which is, in part, also tested using the within-case analysis).

Conclusions

This essay has argued that none of the assumptions which could justify causal inference via paired or more elaborately grouped qualitative controlled comparison are likely to be even remotely plausible in social-science applications. Thus, it is a mistake to attempt to justify a qualitative comparative research design by claiming that the design will achieve causal inference via control, correspond with Mill's Methods, or even meaningfully rule out a given explanation (which might after all be probabilistic or interact in complex ways with background variables).

Yet the conclusion is not that qualitative researchers should abandon comparison. Such research designs make real contributions in terms of conceptualization and measurement, exploring the prevalence of causal capacities, and building theories of causal moderation. These contributions all retain their value, independently of the methods used for within-case causal inference. The optimal comparative designs for these purposes should be a lively topic for future research within the qualitative methods community.

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Two Ways to Compare

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In taking up the task of “rethinking comparison in the social sciences,” we might gainfully ask a basic, but not-too-often posed, question: What are the different ways to compare?

Or to rephrase the query more precisely: What are the different ways in which we ordinarily use the word “compare”? My aim in posing this question is to bring into clearer view a way of comparing that, despite being both common and integral to the social sciences, often goes unnoticed. By drawing attention to it, I hope to provide social scientists with a set of starting points to think more clearly about the comparisons they make and to expand their imagination about the kinds of comparing that are possible.

Juxtapositional vs. Perspectival Comparison

A cursory look at most any grammar book reveals, in fact, two distinct uses of “compare.” On the one hand, to compare can mean to juxtapose similar kinds of things in order to estimate or catalog their similarities and/or differences.¹ We can thus compare one person with another and describe how they stack up against each other with regard to wit, height, wealth, education, or the like. One can similarly compare one country with another, one revolution with another, or one social movement with another. We might call this way of comparing “juxtapositional.”

On the other hand, to compare can mean to draw an analogy between two different kinds of things, to liken one kind of thing to a different kind of thing. When we compare the moon to Swiss cheese, we show how one kind of thing (the moon) is like a different kind of thing (Swiss cheese). One might similarly compare the moon to a traveler who journeys across the sky, a parent who tucks a child into bed, or a beacon that shines at night. Sometimes when we compare dissimilar things,

the analogy is made explicit by the use of simile (e.g., the moon is *like* a piece of cheese). At other times, the analogy is more hidden or implied and takes the form of metaphor (e.g., the moon *is* a piece of cheese). To speak of politics in terms of carpentry—as Max Weber ([1919]1946, 128) did when he famously described politics as “a strong and slow boring of hard boards”—is but one example of comparing dissimilar things by means of metaphor.²

When we compare dissimilar things, we show the relationship between them. What Kenneth Burke (1941, 421–22) wrote about metaphor holds for comparing dissimilar things more generally: “It brings out the thisness of a that, or the thatness of a this.” In comparing the moon to Swiss cheese, we bring out the cheese-like qualities of the moon. In comparing politics to carpentry, Weber brings out the carpenter-like dispositions that someone with the vocation of politics should possess. Because comparing dissimilar things involves bringing out the thisness of a that or the thatness of a this, we might say that it establishes, to again borrow from Burke, a “perspective” (1941, 422). It uses cheese as a vantage point from which to view and gain perspective on the moon or carpentry as a vantage point from which to view and gain perspective on politics. For this reason, we might call this way of comparing “perspectival.”

We have, then, two ways to compare. To compare juxtapositionally is to place similar kinds of things side by side in order to catalog their similarities and differences. To compare perspectively is to draw an analogy between different kinds of things as a way to establish a vantage point from which to view one thing in terms of the other. When “compare” is used in its juxtapositional sense, we can say either compare “with” or compare “to.” When the term is used in its perspectival sense, we more typically say compare “to.” When astronomers

1 Here and in what follows I use “things” in a colloquial, encompassing sense. The term includes not only material objects but also events, actions, processes, practices, experiences and the like.

2 The comparative dimension of metaphor becomes more evident when contrasting metaphoric and literal utterances. The metaphoric “The moon is a piece of cheese” can be reworded as “The moon is like a piece of cheese” and still make sense. The literal “Red is a color,” in contrast, cannot be rendered as “Red is like a color”—such an utterance is nonsensical (Carston 2002, 358). The point is that metaphors can be turned into explicit comparisons whereas literal utterances cannot. This is not to claim that metaphor and simile function identically, but only to call attention to the fact that both involve comparison. For more on the comparative dimension of metaphor, see Perrine (1971). On the differences between metaphor and simile, and the more complicated relationship of metaphor to comparison, see Glucksberg and Haught (2006).

juxtapositionally compare the moon with planet Earth, they estimate the similarities and differences between the two celestial bodies with regard to mass, diameter, chemical composition, gravitational pull, and the like. When the poet Nicholas Vachel Lindsay (1913) perspectively compares the moon to a city, he brings out the cityness of the moon, with its “yellow palaces upreared upon a glittering ground.”

The difference between the two ways of comparing is also revealed in the popular warning against comparing “apples to oranges”—or “grandmothers to toads,” as the saying apparently goes in Serbian. Whatever the language, this idiom admonishes against juxtapositionally comparing things that are deemed to belong to different categories. Such a warning would be misinvoked when comparing perspectively. Indeed, a perspectival comparison *requires* comparing things deemed to belong to different categories—comparing, say, the moon to cheese, or grandmothers to toads.

I repeat “deemed to” because, as we have learned from Nelson Goodman, “anything is in some way like anything else” (1972, 440). Depending on the interests of the person doing the comparing, the same pair of things can be deemed to belong to either the same or different categories. Apple farmers concerned with the yields of assorted varieties of that fruit might deem oranges to belong to a different category that holds little interest to them. For them to juxtapositionally compare apples to oranges might make little sense. Nutritionists, in contrast, may well want to juxtapositionally compare apples to oranges so that they can estimate the vitamin content of various types of fruit that might be included in school lunches. We could also imagine evolutionary biologists taking an interest in comparing juxtapositionally the DNA of toads and grandmothers, or astrobiologists wanting to compare juxtapositionally the molecular compounds present in both cheese and the moon. There is, to repeat, nothing intrinsically similar or dissimilar about any two things. We must be careful, then, not to reify or naturalize our categories: we come *up* with similarities, not *across* them. But once we deem things to be similar or dissimilar, to belong to the same or different categories, juxtapositional comparison requires comparing like to like, whereas perspectival comparison requires comparing like to unlike.

Juxtapositional and Perspectival Comparison in the Social Sciences

Social scientists who offer methodological advice about comparing usually focus their attention on

working out how to compare juxtapositionally. Much of this advice comes from scholars operating within the positivist tradition. To ensure that apples are being compared to apples and oranges to oranges, that whatever is being compared actually belongs to the same category, some positivist methodologists develop guidelines for creating categories that are well-demarcated, a task that often goes by the name of “concept formation” (see, e.g., Sartori 1970). To increase confidence that the particular cases chosen for study actually belong in those categories, that clear procedures exist to help correctly categorize this piece of fruit as an apple and that piece of fruit as an orange, other positivist methodologists offer guidance on “operationalization” and “measurement” (see, e.g., Schoenberg 1972). To provide reasoned criteria for deciding which particular apples, among the entire universe of apples, are selected for juxtapositional comparison, still others attend to “sampling” or “case selection” (see, e.g., Ebbinghaus 2005).

Advice from interpretivist scholars on how to compare juxtapositionally often differs. Some interpretivists, for instance, argue that the construction of well-demarcated concepts is not necessary for juxtapositional comparison (see, e.g., Schaffer 2016, 59–64). Other interpretivists offer guidance on case selection that overlaps only partially with the advice given by positivists (see, e.g., Simmons and Smith 2017). Yet others argue that juxtapositional comparison can or should rest not on a selection of cases posited to exist in the world, but rather on a constructive casing of the world (see, e.g., Soss 2018 in this symposium). But whatever differences exist in how positivist and interpretivist scholars typically approach juxtapositional comparison, the fact remains that this way of comparing is crucial to both.

Because John Stuart Mill’s *Logic of the Moral Sciences* has become a reference point for many social scientists who today write about comparing, it is perhaps worth pointing out that his method of difference and his method of agreement are both modes of juxtapositional comparison. It is not the case that the method of difference is juxtapositional and the method of agreement perspectival. Both methods compare things that are deemed to belong to a single category. In Mill’s own example, the category was that of “nation.” To see whether the wealth of nations is a result of their commercial policies using the method of difference would require, he explained, finding two nations—one rich and one poor—“which agreed in everything except their commercial policy,” while the method of agreement would require that the nations “agree in no circumstance

whatever, except in having a restrictive system and in being prosperous” (Mill [1843]1988, 68, 70). Whether we use the method of difference or agreement, we compare one nation with another. If we were to compare a nation perspectively, in contrast, we would need to compare it to something we deem to belong to a different category. We might thus perspectively compare a nation to a family, human body, or melting pot.

Why Perspectival Comparison in the Social Sciences Warrants more Attention

Working out how to best compare juxtapositionally is a worthy project because juxtapositional comparison is so common across the social sciences. Yet we must acknowledge that social scientists routinely compare perspectively as well. Game theorists conceive of politics or economics as games with rules and predictable outcomes. Process tracers think of society as a kind of machine in which various sorts of causal mechanisms are at work. Interpretivists—or at least some of them—see culture as a web that suspends people in meanings they have spun, to paraphrase Clifford Geertz (1973, 5). Sometimes such analogies are self-consciously crafted and invoked. More often they are implicit and taken for granted and thus go unnoticed. Consequently, perspectival comparison has been the subject of far less methodological reflection in the social-science literature on comparing than its juxtapositional cousin.

There are at least four reasons why perspectival comparison deserves more time in the spotlight. For one, it often serves as the foundation for juxtapositional comparison. To juxtapositionally compare, say, the causal mechanisms that produced both the French and Russian revolutions requires first conceiving of a revolution as an ordered sequence of events produced by processes that are analogous to the operation of parts in a machine. If our thought is not to be guided in unexamined ways, we need to be more attentive to how the perspectival comparisons that we make shape the way we understand the phenomenon in question, frame the way we conceptualize what is going on, and guide our choices about those facets of social reality to compare juxtapositionally. One reason why social scientists sometimes fail to notice the perspectival comparisons which lie beneath their inquiries is that such comparisons are in many instances already embedded in the ordinary language that they draw upon to conceptualize their studies. A perspectival comparison can seem so obvious and natural that it is not even recognized as a perspectival comparison. Notice, for instance, how I slipped in

a perspectival comparison three sentences above by likening social reality to a cut gem. Like the multiple polished faces of a diamond, social reality too may be thought of as having facets—an idea that has become so commonplace that it is rarely even noticed as metaphoric.

The second, related, reason why perspectival comparison deserves more time in the limelight is that any similarities generated by means of it are only partial. Politics is like carpentry in some ways *but not others*. A nation is like a family in some ways *but not others*. Any perspectival comparison channels our attention toward some things and away from others. As Max Black (1962, 41–42) explains:

Suppose I am set the task of describing a battle in words drawn as largely as possible from the vocabulary of chess. These latter terms determine a system of implications which will proceed to control my description of the battle. The enforced choice of the chess vocabulary will lead some aspects of the battle to be emphasized, others to be neglected.

Perspectival comparisons specify and disregard at the same time. To point out the chessness of battle is to both focus on the move-countermove strategy of battle and take no note of its horror. To point out the carpentryness of politics is to both shed light on the place of passion and perspective in politics and overlook the possibility that politics might not always be a matter of treating people like lumber to be drilled, that it can instead involve people coming together as equals. To borrow the language of Hannah Arendt (1958, 220–30), we might conceive of politics in terms of “action” rather than “making.” Social scientists who rely on a given perspectival comparison need to be aware of where that comparison directs attention and what it removes from view if they are not to let the comparison channel their thought in ways that are both unrecognized and unduly constraining. By recognizing the place of perspectival comparison in our thinking, we become more aware of the ways in which particular perspectival comparisons potentially box in our thinking. Such an awareness can open space for more expansive understandings and make us more alive to surprises that have the power to disrupt our commonsense.

The third reason why perspectival comparison merits attention: it is a powerful tool for making sense of ourselves, our experiences, and our world. To characterize what love “really is,” we might compare it to a delicate dance or smoldering fire or wild rollercoaster ride or

even hand-to-hand combat. Indeed, our understanding of love takes shape, in part, by means of the multiple perspectival comparisons that we make, by viewing love from not one, but many vantage points. As Burke put it more abstractly, “It is by the approach through a variety of perspectives that we establish a character’s reality” (1941, 422).³ If we want to establish what kind of thing, say, modern democracy is, we might perspectivally compare it to a game (see, e.g., Bobbio 1987), a market (see, e.g., Downs 1957), a family drama (see, e.g., Lakoff 2002), and a theater production (see, e.g., Edelman 1988), among other things. It is by viewing modern democracy from multiple vantage points that we build up a textured understanding of what it is.

We come, finally, to the fourth reason. If perspectival comparison establishes reality, then making a fresh perspectival comparison can establish a different reality, can inaugurate new ways to understand the world. When Thomas Hobbes ([1651]1998, 7) persuaded readers of *Leviathan* to view the state as an “artificial man,” he changed their conception of it. They came to view the state as both human and a work of artifice, as something that could be engineered by means of social contract. When Paul Samuelson convinced readers of *Foundations of Economic Analysis* to conceive of economics in terms of thermodynamics, they came to see the operation of physical laws, such as Le Chatelier’s Principle, at work in the market (1947, 36). Samuelson’s imaginative use of analogy, not incidentally, helped secure his place as the “father of modern economics,” to quote one historian (Parker 2002, 25). It is a curious fact that while many scholars celebrate analogical reasoning for its contributions to creative scientific thinking (see, e.g., Brown 1976), few recognize it as a distinctively comparative approach or conceptualize it as such.

Methodological Questions about Perspectival Comparison

Recognizing perspectival comparison as a distinct and pervasive mode of comparative analysis within the social sciences raises a host of methodological questions. Here I offer initial thoughts on just two.

The Question of Procedure

The first methodological question I wish to take up has to do with procedure: How can we generate fresh perspectival comparisons? Where do they come from? Acknowledging that comparisons are made, not found, that similarities are created, not encountered, means

that there is no instrument, no machine capable of unearthing a perspectival comparison as if some already existing similarity were buried in the ground just waiting to be dug up. It would perhaps be more fruitful to ask instead how we might cultivate habits of thought to render moments of imaginative comparison-making more rather than less likely to occur. As C. Wright Mills put it, a certain “playfulness of mind” is surely necessary because, like the sociological imagination that he was describing, perspectival comparison has an “unexpected quality...perhaps because its essence is the combination of ideas that no one expected were combinable” (1959, 211). Such playfulness requires exercising those parts of the brain where the sparks of imagination ignite—trying one’s hand at writing poetry, for instance. It also requires nourishing one’s thinking with a rich and varied diet. To see the carpentryness of politics, the chessness of battle, or the grandmotheriness of toads, after all, requires knowing something about both carpentry and politics, chess and battle, grandmothers and toads. Put another way, intellectual nourishment requires gardening outside the walls of the academy and reading omnivorously beyond the confines of one’s discipline.

The Question of Truth

The second, thornier methodological question has to do with judgments about truth: Are any perspectival comparisons “true,” or at least, “better” than others? How one answers this question depends in good part on how one understands the relationship of language to the social world. If one believes that the social world exists independent of language, then perspectival comparisons might be conceived of as models or hypotheses about that independently existing social world which can be tested (see, e.g., Landau 1961, 334–35). Statements like “Politics is carpentry” or “Society functions like a machine” become propositions that can be verified or falsified through observation of a world taken to be freestanding.

But such a view of how language relates to the social world is, I believe, mistaken. Our social world is built up by the words we use. Battles, politics, nations, and grandmothers do not stand apart from the language we use to engage, create, or relate to them. As Charles Taylor puts it, language is “constitutive” of the social world (1971, 25). This claim should not be taken as a naive argument for linguistic determinism. Taylor clarifies:

³ Note that Burke has an expansive understanding of “character.” He defines it as “whatever can be thought of as distinct (any thing, pattern, situation, structure, nature, person, object, act, rôle, process, event, etc.)” (1941, 422).

The situation we have here is one in which the vocabulary of a given social dimension is grounded in the shape of social practice in this dimension; that is, the vocabulary wouldn't make sense, couldn't be applied sensibly, where this range of practices didn't prevail. And yet this range of practices couldn't exist without the prevalence of this or some related vocabulary. There is no simple one-way dependence here. We can speak of mutual dependence if we like, but really what this points up is the artificiality of the distinction between social reality and the language of description of that social reality. The language is constitutive of the reality, is essential to its being the kind of reality it is (1971, 24).

Words and the world, in short, are inseparably dependent upon each other.

If the words we use are constitutive of the world, and if we make comparisons, not find them, then there is no freestanding world “out there” against which to test statements like “Politics is carpentry” or “Society functions like a machine.” Indeed, it is by means of such perspectival comparisons that we build up our understanding of what that world is. Here we would also do well to remember that the world is multivalent. We can think of democracy in terms of a game, a market, a family drama, a theater production, *and many other things besides*. We are not locked into a single metaphoric scheme (Lakoff 1987, 304–37). Given this metaphoric polysemy, given the multiple ways that we have of conceiving the world, how should we think about truth? Philip Wheelwright (1962, 172–73) offers what I take to be sage advice:

The best we can hope to do is catch partisan glimpses, reasonably diversified, all of them imperfect, but some more suited to one occasion and need, others to another....The metaphoric and the mythic are needed elements in the intellectual life of an individual and of a community; only, when serious questioning begins, one must deal with the proposed answers not by outright acceptance or rejection but with limited and qualified consideration, murmuring with the Hindu gurus of the Upanishads, “*neti neti*”—“not quite that, not quite that!”

The “goodness” of a particular perspectival comparison depends on the occasion and the need, and even so, it can offer only a glimpse of something which is not entirely that. To sharpen Wheelwright's point, we might add that a good perspectival comparison within the social sciences—with its particular occasions and needs—might be one that, among other things, is fresh enough to generate genuinely new insight about what the social world is or what takes place within it; while a good social-science use of perspectival comparison is one which acknowledges that any one comparison, however original and profound, still only offers a single vantage point among many.

Concluding thoughts

Perspectival comparison is foundational to the social sciences. Acknowledging its central role should both expand and complicate our understanding of the ways in which we compare and the constitutive role played by language when we compare. Much of the interesting work begins once we realize that reality is not given and overt but—to again quote Wheelwright—“latent, subtle, and shy” (1962, 173). Put more prosaically, our reality is often metaphorically constructed, typically in ways that we fail to notice, by means of perspectival comparison. It is time to move this more hidden mode of comparison out of the shadows.

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On Casing a Study versus Studying a Case

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Most methods texts encourage students to define some site or event as a case (noun) that they will go out and study (verb). Cases are defined as real members of a general conceptual class: They exist “out there,” in a sense, before we even arrive. Surveying available cases, researchers are encouraged to ask a series of analytic questions (which ones? how many? how likely? how typical?) and then select in a purposive way to answer a specific research question.

Valuable as it is, this approach has often felt foreign to practitioners of interpretive research. In his introduction to *What Is a Case?* Charles Ragin (1992, 6) recounts how his co-editor Howard Becker “persistently pulled the rug out from under” consensus along these lines:

From his perspective, to begin research with a confident notion of... what *this*—the research subject—is a case of... is counterproductive. Strong preconceptions are likely to hamper conceptual development. Researchers probably will not know what their cases are until [later in the process]. What *it* is a case of will coalesce gradually, sometimes catalytically, and the final realization of [how the phenomenon is to be cased] may be the most important part of the interaction between ideas and evidence.

This sort of discomfort is the impetus for the current essay. In immersive research, we often enter research sites for practical and political reasons, or for reasons related to language, culture, funding, or something else. Our research strategies prioritize discovery and embrace changes in research goals and questions. For these and other reasons, we often wind up with an emerging study (noun) that we need to case (verb). As we learn in the field, we repeatedly encounter the challenge of how to conceptualize social action on broader analytic terms. Wrestling with what we are studying, we ask “what should I treat this as a case of?”

In this essay, I explore a critical yet underappreciated way case study methodologies may differ. “Nominal” approaches to casing, I suggest, offer a valid and vital alternative to the prevailing “realist” model. From a realist perspective, fluid and uncertain efforts to case a study, pursued in a shifting and ongoing way, may appear

ad hoc, suspect, and even “unscientific.” Pressures to meet realist standards may dissuade researchers from pursuing strategies more appropriate for their project. At the writing stage, scholars may distort aspects of their study as they try to shoehorn what they’ve done into the realist model widely accepted as an ideal. By clarifying the nominal approach, we can promote a more pluralistic discipline, improve methodological guidance, and advance the goals of honesty, reflexivity, and transparency.

The Realist View

In political science today, most scholars conceive of case studies in a realist manner. In methods texts and faculty advisors’ offices, the realist view tends to enjoy a taken-for-granted status. For most, it operates as a kind of common sense, deployed and taught without much reflection on its distinctiveness or felt need to justify its assumptions. It is simply woven, without notice, into matter-of-fact, how-to lessons for good practice in the social sciences.

The realist stance, as Charles Ragin (1992, 8) explains, posits that “there are cases (more or less empirically verifiable, as such) ‘out there.’” Realism positions the researcher as an outside observer who *identifies and selects* from cases made available by the real world. Classes of cases should be defined to correspond with reality, “carving nature at its joints” to clearly specify boundaries of generalization. Given a well-specified “universe,” phenomena either do or do not qualify as a particular sort of thing—a kind of event (e.g., revolution), institution (e.g., slavery), organization (e.g., political party), relation (e.g., colonial), actor (e.g., judge), activity (e.g., deliberation), belief system (e.g., Vedanta theology), or some such.

To employ the field’s prevailing language of case *selection* is, in a sense, to adopt this realist position on the relationship between observer and observed. Cases exist in the world, in this view, as objects that correspond to a given category and, thus, as comparable units of analysis rightly analyzed together. Researchers choose among cases that exist, independent of the individual observer, as instances of a general social kind. They may do so in larger numbers for “extensive” analysis or subject a

smaller number to “intensive” analysis (Eckstein 1975). A case study, then, to quote John Gerring’s (2004, 342) influential definition, is “an intensive study of a single unit for the purpose of understanding a larger class of (similar) units.”

From this perspective, sound case study design requires careful attention to the risks of misclassification. In particular, researchers must avoid any sort of “conceptual stretching” that might distort the boundary of a general class and, thus, extend generalizations across non-comparable instances (Sartori 1970). To avoid this pitfall (that is, to ensure that concepts “travel” only to new cases that truly lie within a shared set of scope conditions), some realist texts encourage researchers to make use of different “levels” on a conceptual “ladder of abstraction” (Sartori 1970). Other accounts depart from the assumption of “crisp” classical categories to accommodate “family resemblance categories” (where varied traits define members of a shared category) and “radial categories” (where subtypes may be needed to preserve the integrity of comparisons) (see, e.g., Collier and Mahon 1993; Collier and Levitsky 1997). In all such variants, the realist stance urges scholars to make sure conceptual boundaries correspond to real-world differences and only comparable-in-reality cases are grouped together for analytic generalization.

Realist case selection, then, is a purposive activity with both theoretical and empirical aspects. A theory in use among scholars “covers” only cases that fall within its conditions, so little can be gained by selecting a case outside its scope. Among the theory-relevant options, cases should be chosen to leverage differences in their predicted outcomes and expectations about how a social phenomenon works. John Gerring (2008, 645—6) provides a concise statement of this perspective, emphasizing the shared logic of case analysis across research traditions:

The case(s) identified for intensive study is chosen from a population and the reasons for this choice hinge upon the way in which it is situated within that population. This is the origin of the terminology—typical, diverse, extreme, et al. It follows that case-selection procedures in case-study research may build upon prior cross-case analysis.... Sometimes, these principles can be applied in a quantitative framework and sometimes they are limited to a qualitative framework. In either case, the logic of case selection remains quite similar, whether practiced in small-N or large-N contexts.

As Gerring suggests, the realist view plays a key role in the logic of controlled comparison (e.g., Przeworski and Teune 1970). In its most stringent form, realism prizes “unit homogeneity,” treating cases as equivalent occasions to observe how causal factors covary with an outcome (e.g., King, Keohane, and Verba 1994; Geddes 1990). It is a mistake, though, to imagine that realism appears only in this guise. The realist approach is put to good use in within-case analyses that focus on “process tracing” and “causal process observations” (e.g., George and Bennett 2005; Brady and Collier 2010). It is compatible with efforts to engage the empirical richness of individual cases in ways that value case diversity (Slater and Ziblatt 2013). Theory-relevant cases may be defined in part by their distinctive conjunctions of conditions (e.g., Ragin 2000). Recognizing that factors may combine in multiple ways to yield a given outcome, generalizations may be built cautiously and piecemeal, under assumptions that not all cases work in the same way (Goldstone 2003).

None of these deviations from “classical” categories and controlled comparisons require a departure from realist tenets. Indeed, consider how one of the leading texts on process tracing, typological theorizing, and case diversity states the “requirements” a case study must meet to avoid being “nonscientific, noncumulative” and “atheoretical:”

First, the investigator should clearly identify the universe—that is, the ‘class’ or ‘subclass’ of events—of which a single case or a group of cases to be studied are instances. Thus, the cases in a given study must all be instances...of only one phenomenon.... Second, a well-defined research objective and an appropriate research strategy to achieve that objective should guide the selection and analysis of a single case or several cases within the class or subclass of the phenomenon under investigation. Cases should not be chosen because they are ‘interesting’ or because ample data exist for studying them (George and Bennett 2005, 68—9).

Researchers can and do select cases for interpretive research in a realist manner. As Erica Simmons and Nicholas Smith (forthcoming) rightly note, “meanings, processes, and practices [can function as] the core drivers of case selection.... [C]ases can... refer to political processes, meaning-making practices, concepts, or events.” Realism can also accommodate discovery in the field. Over the course of a study, one may encounter

unexpected features of the case that further specify or revise its status as most or least likely, deviant or typical. Corrective specifications grounded in the observable features of a case are wholly consistent with, and even recommended by, a realist approach (Ragin 2000).

That the prevailing approach *can* be extended in these ways, however, does not mean that it is the only game in town, nor always best. Neither the method nor the substance of what we study should be seen as an inherent reason to reject the realist approach. Rather, the problem lies in the field's elevation of this approach to a status of orthodoxy—a singular canon of correct practice that is violated when researchers deviate from it. In so doing, we deter the pursuit of valid alternatives that, for some projects, may be more fruitful. We also pressure researchers (especially graduate students) to distort the reporting of their work so that it conforms to the prevailing disciplinary model.

The Nominal View

“Social actions are comments on more than themselves,” Clifford Geertz (1973, 23) famously observed: “Where an interpretation comes from does not determine where it can be impelled to go. Small facts speak to large issues, winks to epistemology, or sheep raids to revolution, because they are made to.” Needless to say, Geertz is not suggesting that if we are interested in revolutions in general, we should select the local sheep raid as a theory-relevant case and design a study around it. His comment speaks instead to the possibility that we might “see a world in a grain of sand,” as William Blake put it. The humble goings-on at a local research site can be framed in broad conceptual terms and “impelled” to speak to even the largest of scholarly, social, and political questions.

All case studies aim to advance knowledge of the world. But where the realist approach prioritizes ontological questions (Is this really a case of X? Does it truly exhibit the defining features of the population we are sampling from?), the nominal approach places greater emphasis on epistemological questions (What can be learned by treating this phenomenon as a case of X?). From a nominal perspective, “casing” is an ongoing research activity in which we seek to advance insight, understanding, and explanation by conceptualizing the particular in more abstract terms, as an instance bearing on something more general.

From this perspective, phenomena in the social world do not exist, inherently and really, as a case of any social (science) kind. They are ambiguous occasions

for meaning-making, for researchers as much as for the participants who experience them in everyday life. Their relationship to general analytic categories is a question worth puzzling over and playing with—and thus, an opportunity for intellectual creativity. Experienced realities emerge as bona fide *cases* of something only, to echo Geertz, “because they are made to.” As Charles Ragin (1992, 10) rightly notes: “At the start of the research, it may not be at all clear that a case can or will be discerned. Constructing cases does not entail determining their limits [as in the realist view], but rather pinpointing and then demonstrating their theoretical significance.”

Nominal casing may be pursued at any point in a study, from the planning stages through fieldwork and into the writing process. Within a single study, the same bit of social action may be cased in different ways for different purposes. Each casing allows the researcher to put the study into dialogue with a different set of empirical phenomena, creating new standpoints for interpretation, new paths for generalization, and new terms for relational, processual, or comparative analysis. Studying a local one-stop center for social services, for example, I may initially case it as a “welfare agency” but then put it into dialogue with other cases defined by categories, such as “state bureaucracies” (like the police department), “policy-implementing organizations” (which include market firms), or “sites of citizen demand making” (such as the court or voting booth). I may case the social transactions at my site in terms of “disciplinary power” but then reframe them as a kind of “interaction ritual” where “presentations of self” get negotiated in ways that have significant consequences for politics and policy.

A published study built around a broad “umbrella” casing may move through analyses that frame site-specific relations or episodes in terms of different conceptual classes. In general terms, for example, I may frame my book as a case study of “domination and legitimation.” Within this frame, my chapters may shift from interpreting social action as a case of “how identities get constructed through relations of exploitation,” to analyzing it as a case of “how hegemony works through game-like logics.” Each maneuver positions the study in relation to a different body of knowledge with its own tale to tell about why this thing matters and works in a particular way. Each specifies a different scope of analytic generalization, its own path for insights that travel. As I re-case my study, I create new interpretive opportunities

by putting the same social action into dialogue with different theories and empirical sets.

Of course, not all the things we conceptualize in our research rise to the level of a “case.” Casing occurs only when we use an abstract concept to define a fundamental category and standpoint of analysis. It happens when we frame what we are studying in relation to a general type and forge a dialogue in which instances of this type become the basis for insights into one another. On one side, the concepts, theories, and empirical studies that prior scholars have used to illuminate the general type become interpretive resources for making sense of our study. On the other side, our empirically grounded insights become grist for interventions that may elaborate, contest, or revise conventional understandings of the class, as a whole.

In this regard, a nominal approach highlights the critical and disruptive potential of casing in a way that the realist view does not. As social science conventions, classes of cases operate as regulative norms and as lenses that naturalize particular understandings of phenomena. The realist approach encourages researchers to take these terms as given (established and warranted for reasons that correspond to real-world differences) and then to work within their parameters by selecting cases that belong in the conventional class. By contrast, a nominal perspective encourages scholars to approach the established casing of a phenomenon as an earlier intellectual (and perhaps political) act and, thus, as a site for critique, contestation, and reformulation.

By treating the question, what we might imagine this thing to be a case *of*, as contested ground, it is possible to denaturalize what may be taken for granted in our field. New casings of familiar social kinds, Howard Becker (1998, 6–7) rightly argues, “suggest ways of interfering with the comfortable thought routines academic life promotes and supports. [They] suggest ways to turn things around, to see things differently, in order to create new problems for research, new possibilities for comparing cases and inventing new categories, and the like.” And insofar as the knowledge we produce matters for the production and governance of subjects in the broader world, as critical theorists suggest, such interventions can have significant implications for power and practice in societies as much as scholarship.

The nominal view does not counsel against purposive selections of where and what to study, nor does it treat casing as an inductive process to be deferred until entering the field. From a nominal perspective, creative efforts to case the study may be central to the research design phase, providing an explicit basis for the selection of where and what to study. This *preliminary casing* is both valuable and provisional. It helps position and focus the initial stages of research, defining the first steps in an ongoing process. A preliminary casing helps to orient the researcher but also stays in play as an object of reflection, critique, and re-specification. If the initial casing persists to the end of our study unaltered, it is not for lack of trying and is in no way a failure.

Unlike the realist view, however, a nominal approach does not treat explicit casing as a prerequisite at the research design phase. Here, it is important to distinguish casing from positioning within a site. Realist approaches often define sites as cases in the first instance, and base selection on a locale’s properties in relation to a specific theory and population. Other reasons for selection are acknowledged as secondary at best.¹ From a nominal perspective, however, uncertainty about casing may be embraced as we prioritize other grounds for deciding where and what to study and how to position ourselves at a site. We may go to a site (or focus on a historical event) because we feel called to confront an injustice. We may choose based on our language skills, familiarity with a culture, social contacts, or the ways our identities are likely to be construed. We may be influenced by the body of available evidence, the costs and distances of locales, our ability to live comfortably in a place (and thus, carry out research for a long period), funders’ priorities, and much more. In light of such concerns, it may be “best practice” to position ourselves at a site with a reasonably clear sense of our research interests and how to pursue them, but with little certainty about what our study will ultimately be a case *of*.

This openness to possible casings should not be confused with entering the field as a *tabula rasa*, devoid of ideas about what kind of case is at hand. Social science disciplines are disciplinary in the Foucauldian sense: We are trained to understand ourselves and others as particular kinds of subjects and to see phenomena in the world as “obviously” being of one kind or another. Our

1 John Gerring (2008, 679), for example, takes up this possibility only after delivering his core prescriptions for case selection—as a secondary consideration that may influence selections *within* the group of cases identified as meeting more primary analytic criteria: “I have also disregarded pragmatic/logistical issues that might affect case selection. Evidently, case selection is often influenced by a researcher’s familiarity with the language of a country, a personal entrée into that locale, special access to important data, or funding that covers one archive rather than another.”

socialization, professional and otherwise, instills a kind of *habitus* that structures our predispositions to notice, perceive, and classify what we encounter in field or archival research (Brubaker 1993). Thus, nominal casing efforts should always be understood as *reflexive* practices aimed at our existing conceptions of the world—efforts to question and rework elements of our own feel for the kind of social action in play.

Nominal casing, then, should not be misread as an inductive counterpart to allegedly deductive realist procedures. Rather, it entails an evolving dialogue of fieldwork and framework in which site-specific experiences and observations are put into conversation with broader understandings of theory, history, and social structure (Sanjek 1990; Hopper 2003). As we go along, we consider alternative frameworks and try them on for size, thinking about various ways we might move from what we are studying toward larger analytic questions and generalizations. Shifts in casing frequently emerge from an *abductive* process in which experiences of puzzlement and doubt generate opportunities for reframing social action (Locke, Golden-Biddle, and Feldman 2008; Schwartz-Shea and Yanow 2012). The path to a catalytic moment of re-casing will often be paved by frustrated suspicions that something is just not right about how our framework fits together with our fieldwork. Indeed, one of the most productive suggestions an advisor might make to an exasperated dissertator is: “Maybe you’re puzzled by what’s happening because you’re thinking about it as a case of X, rather than something else entirely.”

Re-casing is, then, not so much a shift in research question as a maneuver in our efforts to explain and generalize. When Lisa Wedeen (2007) cases qāt chews in Yemen as political deliberation in the public sphere, for example, she asserts that these events can yield insights into other instances of deliberation, that the idea of deliberation can illuminate qāt chews in important ways, and that lessons drawn from a study of qāt chews can call general theories of deliberative democracy into question, justifying critiques and revisions. When we deem some bit of social action to be a case of the free rider problem—or a case of gender performance, classification struggle, the productive power of the gaze, or whatever—we assert something explanatory about how we think it works. When Timothy Pachirat (2011) cases action at a local slaughterhouse as an instance of the general relationship between visibility and power, for example, he immediately declares the relevance of theories associated with figures such as Zygmunt

Bauman, Norbert Elias, and Michel Foucault, hailing them onto the scene as explanatory possibilities.

As these works by Wedeen and Pachirat suggest, nominal approaches to casing may *encourage* “stretching” concepts to unlikely instances that, in the commonsense of the field, seem misplaced. In *Unwanted Claims*, for example, I studied interactions with the welfare state as cases of political participation (Soss 2000). Political participation was among the most well-established areas of study in the discipline, defined and measured according to a clearly bounded set of research sites and behaviors. Welfare participation was not among them. Most political scientists took it as obvious that people in welfare programs were doing something quite different from the bona fide acts of political participation that served as “citizen inputs” in electoral-representative processes.

In field essays, I found realist explanations for the scope and content of “political participation,” detailing why such voluntary acts (mostly aimed at selecting or influencing government officials) should not be confused with other activities people might like to think of as “political” in some way. Joseph LaPalombara (1978, 167, 188), for example, famously argued that sound generalizations about “political participation” require “careful and precise empirical denotation [and a] restricted scope of empirical reference.” Responsible scholars must avoid the “indiscriminate and indiscriminating extension of concepts” to activities, institutions, and polities that do not truly (empirically) fit pluralist conceptions of liberal democracy. Citizen-initiated claims on the welfare state did not constitute a theory-relevant case, and poor people who made such claims received attention mainly for their *lack* of political engagement.

To case my comparative study of welfare participation on political terms, I had to intentionally *stretch* the concept of political participation. To do so, I looked beyond the behavioral literature to insights developed by feminist and participatory-democratic theorists. The casing that resulted was not persuasive to everyone. In a book review of *Unwanted Claims*, one prominent political scientist called it “an abuse of language” to classify and analyze welfare claiming as a case of political participation (Mead 2001, 676). But the casing of my study on these terms—born as I drafted the research design, developed and refined throughout my field research—underwrote virtually all of the book’s scholarly contributions. It allowed me to challenge the field’s tidy distinction between social citizenship and political citizenship. My field study of welfare participation became the basis for

a critical analysis of political participation in general, both as concept and practice. Theories of political participation directed my attention in the field and guided my empirical analyses of how welfare claiming emerges through a political process, how policy designs structure voice and quiescence, and how participatory experiences produce political subjects and patterns of status, belief, and action.

In retrospect, the casing pursued in *Unwanted Claims* was unusual only in its particulars: My *approach* to casing fell comfortably within a long and vibrant *nominal* tradition of case study research in the social sciences. By casing phenomena together in new and creative ways, scholars construct counter-intuitive standpoints for interpretation that draw new social and political dynamics into view. At the same time, we create empirical foundations for new critiques and revisions of concepts and theories in the field.

Conclusion: Reflecting on What We Teach

By clarifying the nominal alternative as a valid approach to casing, it is possible to see more clearly the downsides of faculty committees that pressure graduate students to define their cases at the outset and use them as a basis for nearly every aspect of their research design and strategy. *A priori* casing is valuable for the purposes of many projects. But deep investments in a casing, prior to entering the field, also carry substantial risks of lock-in and tunnel vision. In many research projects, settled and enforced case definitions can work to foreclose insights

and constrain research in rigid and undesirable ways. Graduate students who have been pushed in this manner may be particularly likely to experience their casing as an almost-inescapable trap. Having built the entire edifice of a project around a particular casing, and having won a go-ahead from the committee only on this basis, is it any wonder that a dissertator might not relish the prospect of declaring that what they are studying should probably be thought of as a case of something else? Really, who would want to open *that* can of worms?

The nominal alternative is to encourage graduate students to try out different ways of casing their study from the get-go. By advising students to see their preliminary casing as a provisional standpoint, adopted for now as one possibility among many, faculty advisors can legitimate and foster research practices that embrace generative doubt and make casing into an ongoing subject of reflexive critique. In so doing, we can begin to bridge the gap that students encounter as they move from positivist and realist texts on case study design into more interpretive texts that emphasize open-ended processes of discovery and abductive reasoning. Frustrated by confusing tensions between the two, many scholars who pursue interpretive and critical studies avoid the language of case study research altogether. A more fruitful path forward is to be transparent about working within an alternative and equally valid case study tradition, seizing the analytic opportunities that only reflections on general kinds can provide.

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Comparative Analysis for Theory Development: Reflections on a Study of Women's Empowerment

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Comparison is, directly or indirectly, *the* defining characteristic of political science. Comparison enables us to discover what is unusual about any given individual, event, group, process, or context. Comparing sharpens our awareness of assumptions that underlie our theoretical thinking, makes it clearer how concepts should be defined and operationalized, and may change what questions we ask. How do we choose what to compare and how can we defend our decisions? As the introduction to this symposium points out, methodological texts have focused overwhelmingly on designing controlled comparisons aimed at testing causal theories across a small number of cases. But comparative studies often have other goals, such as developing theoretical arguments, particularly so if they form part of a multi-method project. There is a discrepancy between the types of research designs scholars typically teach and the type of research they actually conduct (George and Bennett 2005, 10).

In this piece, we respond to the editors' call for a clearer articulation of methodological choices related to comparative research designs by reflecting on our choices in an ongoing study of women's empowerment and what they may reveal about comparative work more generally. We begin by discussing how our project developed theoretically and how our research design changed in response. As our research moved from one stage to the next, new questions emerged, and different designs became more appropriate. In the current stage of our project, our aim is theory development rather than theory testing. Specifically, we want to gain a better understanding of the concept of "empowerment," including its nuances and boundaries, in different parts of the world. Consequently, we have conducted fieldwork in three countries: Norway, Japan, and the United States. These countries are not "cases"—understood as specific instances of a clearly defined class of events (George and Bennett 2005)—but rather *contexts* where we find

interesting variation in our concept of interest. Finally, we discuss how the cultural knowledge we need for qualitative fieldwork should guide, but not constrain, studies aimed at theory development. In our work, we have found that deliberately building a multi-cultural research team helps build local knowledge and leverage insider and outsider advantages across different contexts.

A Study of "Empowerment"

In one of our ongoing research projects we aim to understand more about the great variety of state-led efforts to "empower" women. Our goal is to identify mechanisms that can improve women's lives and study their effects. As a result of our prior research, we were somewhat disenchanted with research focused on top-down laws and policies alone. Htun's work on the "rights revolution" for women, for example, does not explicitly analyze whether policy changes to combat violence and harassment, reduce discrimination at work, promote equality in the family, or improve public support for caregiving actually produces changes on the ground (Htun and Weldon 2018). Jensenius' work on marginalized communities in India shows that, despite decades of quotas in politics, educational institutions, and jobs, and a slew of programs aimed at improving their socio-economic status, historically stigmatized groups are still disadvantaged both socially and economically (Jensenius 2017). These combined experiences make it clear that participation in political parties and elected office, formal laws ensuring equal rights, and other state efforts to change entrenched social inequalities are not enough to deliver inclusion and justice to disadvantaged groups.

What does it take for legal changes to lead to empowerment? Existing research has shown that a principal driver of women's empowerment has been their economic agency, which we understand as their disposition and capacity to make autonomous economic choices. Studies show that economic agency enables women to contest oppressive gender norms

and change gender relations from the ground up. For example, women's labor force participation increases their political participation, shifts the division of labor in the household, and improves fertility rates in the Global North, while reducing them in the Global South. Women who control property can exit, or threaten to exit, abusive relationships, and exert more control over institutions that shape gendered ideologies, such as schools, the media, and religious organizations (Agarwal 1994; Hashemi et al. 1996; Hakim 1996; Agarwal 1997; Iversen and Rosenbluth 2008; Rosenbluth 2006; Okin 1989; Duflo 2012; United Nations 2015; Panda and Agarwal 2005).

An Evolving Research Design

To explore the associations between laws and women's economic agency, we worked with the Women, Business, and the Law dataset developed by the World Bank (World Bank 2013), which includes information about a large variety of legal provisions in 143 countries. Following the multi-dimensional approach to gender outlined in Htun and Weldon (2018), we developed indices on constraining and enabling laws, including restrictions on women's legal capacity, discrimination in the workplace, and the extent to which the state promotes work-life balance (Htun et al. 2019). Using these indices, we explored the correlation between legislative choices and various macro-level indicators of women's economic agency: access to bank accounts, participation in firm ownership, participation in the labor force, share of women workers in the informal sector, and the gender wage gap. This analysis reveals, not surprisingly, that countries with fewer restrictions on women's legal capacity tend to have higher numbers of women with bank accounts, more firms where women participated as owners, and higher female labor force participation (Htun et al. 2019). However, the associations between our indicators of agency and laws regulating women's work and publicly-paid parental leave are weaker. Moreover, the great variation in women's agency among countries with similar legal environments makes it clear that laws tell only a small part of the story. A great deal of action shaping patterns of women's agency takes place *within* countries, and is hard to see when we conduct studies *across* countries. We needed to conduct within-country research to understand this variation better.

Our cross-country work thus led us toward a different line of investigation, focused on understanding more about the effects of legal changes and policy interventions on gender norms, and women and men's behavior within

countries. We have separate sub-projects looking at how Mexico's laws to guarantee women a life free from violence influence experiences of violence, perceptions of violence, and women's likelihood of reporting violent episodes to public authorities (Htun and Jensenius 2018); how mandatory, universal sexual misconduct training at the University of New Mexico shapes rape myths, gender stereotypes, conceptions of assault and harassment, and willingness to report violations among students (Contreras et al. 2018); and about how political change in Burma affects attitudes and practices on gender and women's agency (Htun and Jensenius, in progress). These projects are all multi-method studies within countries, and do not involve an explicit comparison across these countries. However, the insights we gain from each of these studies are contributing to overall theoretical thinking about how people's awareness of their "right to have rights" gradually takes hold and compels changes in social relations.

As our theoretical thinking developed through these different studies, we began to consider the implications of our story about the importance of individual agency. If having access to more resources implies that women have greater agency, and the process of gaining agency is empowerment, what happens when women—or a significant share of women—reach the endpoint of having access to a lot of resources? Does this mean they are empowered? The experience of rich countries today shows that even when liberal laws and access to resources bring about considerable economic agency, there are still many challenges. Across the Global North, there is considerable inequality among women: Many on the lower end of the distribution of income and wealth are locked in a struggle to provide for themselves and their families. However, women with considerable economic agency also suffer from sexual harassment and abuse, less pay for the same work, biased and discriminatory treatment, and trouble juggling the demands of family and care work. Moreover, there are many ideas of what an "empowered" life looks like.

These experiences raise the questions: What is empowerment? Is the dual-income household with co-participation in care work an ideal model of social life? And if not, why do we care about legal reform, political participation, social mobilization, and other mechanisms to promote it? Our own confusion—even after many years of research on the topic—led us to decide that we want to do more work on the concept of empowerment, what it means in different contexts, and how these different ideas animate the ways that states and societies

put empowerment into practice. In other words, although our overarching research interest is to study the effects of different state interventions, this particular part of our research aims to understand more about variation in the *meaning* of empowerment, since these different meanings may shape state interventions and their effects.

Comparing to Explore the Boundaries of a Concept

To explore differences in the meaning of women's empowerment—which may matter for the main causal patterns we are interested in—we chose a comparative approach. We decided to take a deeper look at three wealthy countries that have few formal restrictions on women's legal capacity and little state-sanctioned discrimination in the workplace, as well as some degree of public support for parenting, but seemingly different discourses about empowerment: Norway, Japan, and the United States. Each country holds a particular appeal. Following much of the gender and politics literature, Htun had long seen Norway as the “paradise” of gender and social equality, due to its extensive social provision, relatively high degree of class equality, and the widespread commitment of virtually all political actors to gender justice. The United States is compelling due to its many puzzles and contradictions, including a stratification of gender equality by class groups (Putnam 2016; Esping-Andersen 2009) and inconsistent progress toward gender justice by issue area (Htun and Weldon 2018). Japan stands out among the rich countries for its seemingly conservative approach and outcomes: a history of official, enforced maternalism, relatively low labor force participation, low fertility, few women in positions of power, and exclusion of women from imperial succession (Estévez-Abe 2013; Htun et al. 2017).

Some people may say our study resembles a controlled comparison of similarly-placed countries that differ in their state approach to empowerment. However, we did not choose to study these three countries because we wanted to make a causal argument about the effects of one state's approach compared to the others'. Nor do we believe they are similar in all other ways besides their legal approach to the empowerment of women.

Following the framework of Gerring (2017, 41), the “case-selection strategy” that most resembles our approach is analysis of a set of cases that are diverse with respect to their “descriptive features,” in order to make primarily descriptive inferences. However, the “cases” in our study are not units from a well-defined universe of a class of events. We picked them because

they offer us vastly different *contexts* that furnish insights into women's empowerment. Our goal is theory development, not “description.” Further, we did not choose our diverse contexts on the basis of values on a single descriptive feature. Our prior knowledge of the many characteristics of these countries made us think that they would provide us with interesting stories about the content and boundaries of the empowerment concept. As such, our choices fit somewhat uneasily within common frameworks describing different types of “case selection.”

Since our goal is theory development, we look for interesting variation to inform our theoretical thinking. Each context we study in this project has helped to bring out particular and unique features of the others, while revealing connections within contexts we thought we already knew well. By conducting fieldwork in each context, we have gained a better sense of the nature and degree of social contestation over women's empowerment. We have seen how gender, class, and race equality relate to one another in different ways. We have learned that people's views on women's empowerment are connected to their views of the good life, as well as how they evaluate the proper role of the state in citizen's lives and as a mechanism for social coordination. We have traced the connection between government goals vis-à-vis women's liberation and other state priorities, and observed how these goals sometimes compete and sometimes complement each other. In addition, the fieldwork has alerted us to our own cultural biases related to women, gender, the state, and the economy (more on this below).

Multi-cultural Research Teams and Collaborative Fieldwork

Koivu and Hinze (2017) emphasize the lack of attention, in methods texts, to the “human element” of selecting what to study based on a researcher's prior knowledge and skill-set. They point out that people see personal reasons for making a selection as almost unprofessional. When it comes to developing a research design to develop theory that takes into account variation in cultural meaning, the opposite is true. How can someone with no prior knowledge of a context conduct fieldwork there and say something important and persuasive?

At the same time, there is great learning in being exposed to new ideas and important advantages to having an outsider's perspective. We consciously designed our study to maximize our insider and outsider advantages.

In the United States, we focused on New Mexico, where Htun has spent much of her life. Jensenius has spent considerable time in the United States, but is quite unfamiliar with the New Mexico context, and her accent makes it evident that she is a foreigner. In Norway, we worked in Oslo which was a new context for Htun but where Jensenius grew up. In Tokyo, Jensenius was clearly an outsider, Htun was somewhat more acculturated due to a one-year fellowship in 2006-2007, and we recruited a Ph.D. student from Tokyo, Melanie Sayuri Dominguez, as a collaborator.

By conducting fieldwork collaboratively, we could leverage both an insider and an outsider advantage in real time. As insiders, we had easier access to sources, command of the native language, and greater understanding of subtle cultural cues. As outsiders, we were able to pose out-of-the-box questions and notice patterns and particularities that an insider rarely thinks of.

Including both insider and outsider perspectives on the same team, at the same time, allowed us to push further in interviews and in participant-observation situations than we would have been able to do on our own. It allowed us to achieve a level of intimacy with our research subjects while also drawing out the narratives people commonly save to explain themselves to dissimilar others.

What is more, this research technique helped us become more aware of and challenge some of our own cultural biases that shape our research. We were, for instance, fascinated to discover the extent to which Norwegians (including Jensenius) take the role of the state for granted when it comes to solving collective action problems and structuring people's lives. Htun, while enamored with the generous welfare policies for working parents, reacted intuitively with more skepticism to the ways in which the state's one-size-fits-all policy solutions limit individual choice. Meanwhile, Jensenius questioned the ideal of a leaning-in form of feminism that Htun takes more for granted, as well as the dichotomous choice—between career and care work—many women face. For Jensenius, an ideal of “empowerment” that implies outsourcing care work to other women, such as low-income immigrant women, seemed unattractive and unjust.

Concluding Thoughts

When scholars decide what to study for comparative analyses, they should make choices according to their particular research goals. Since the publication of *Designing Social Inquiry* in 1994 (King et al.), much of the research design advice passed to graduate students

as part of their methodological training has presumed that their general goal is to test theories about a causal relationship among variables. However, most empirical studies involving the in-depth comparison of a few cases or contexts have the goal of theory development. And theory development consists of many different stages and parts, including building intuition and contextual knowledge, conceptualizing and operationalizing key variables, exploring causal mechanisms, scope conditions of arguments, gaining insight from deviant cases, and so on.

When the goal of comparison is theory development, scholars do not have to stick to only one set of units to compare in a single research project. Different parts of the theory development exercise might call for the comparison of different things. For example, even if one's overarching goal is to develop a causal argument, this does not mean that the selection of what to study in a qualitative part of the study should be designed as a quasi-experiment. Rather, one should aim to find interesting variation that may further the particular goal for that part of the project. Especially in multi-method projects, it is common for the different parts of the study to play different roles, and it is ok that they are designed differently.

What is more, the research design can and should evolve with the theoretical thinking. If your theoretical thinking changes, so should your research design. George and Bennett (2005, 73) note that one might need “some iteration” in the process of designing and implementing qualitative research. This is an under-statement. Very often, researchers start out with a research design based on their theoretical priors, but as they start conducting their work, their understanding of key concepts, important variables, and even what the research question should be, changes. It is important to start out with a plan and to provide a sensible justification for that plan, but it is also fine to modify that plan as one learns more. Whereas a static research design makes sense for some forms of theory testing—the extreme case being pre-registration of data collection and analysis plans for experiments—this would stifle the intellectual contributions from a project focused primarily on theory development.

Scholars don't need to know everything about their theory in advance. It can be rigorous, transparent, and scientific to learn as you go along. Comparative work helps open your mind and extend your horizons. Let its insights enrich your theory.

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Problems and Possibilities of Comparison Across Regime Types

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During the Cold War, comparisons between the Soviet-led state-socialist bloc and democracies sparked scholarly controversy. Today, with China assuming the mantle of the most significant non-democratic regime model, and with scholars pursuing innovative comparisons between China and other political systems (Duara and Perry 2018; Tsai 2016; Zhang 2013), it behooves us to revisit some of the questions that such comparisons pose. Specifically, when is it reasonable to pursue comparisons, what is their purpose, and what do they entail? In this short piece I will address only some of the issues involved.

The main themes of my essay are as follows. Giovanni Sartori usefully cautioned against comparing unlike entities, yet his advice was overly confining. Sometimes gaps or disjunctures between political phenomena in substantially dissimilar political systems provide opportunities for innovation, even if they complicate Mill-style comparison. In particular, such projects can generate intellectual payoffs through the way in which they *frame* a topic of study, specify its universe of cases, and scrutinize the gains and problems of including phenomena from disparate contexts in a common category. Further, they provide opportunities for *conceptual development* by elaborating on and exploring their shared phenomena, explaining how they vary, and so forth. I argue that these, not merely logics of control, are among the most important benefits of cross-regime comparative research that employs in-depth, qualitative analysis. Such comparisons are not always feasible or useful. When successful, however, they hold the promise of new, thought-provoking theoretical and conceptual departures. I illustrate this with an example from a research project in which I compared China and Taiwan.

Comparison across Dissimilar Political Systems

As political scientists we often compare among cases that fit more or less comfortably within a category precisely in order to achieve controlled comparison through a most-similar systems design. Thus, a study might be

framed as “... in modernizing agrarian bureaucracies” or “in transitional democracies” or “in late industrializers.” What kinds of problems might a researcher encounter when stepping outside this common template and comparing among dissimilar political systems? Some obvious stumbling blocks include the possibility that data are unavailable or incommensurate in one or more of the cases to be compared. Another is that inquiry across highly dissimilar systems might merely confirm the obvious rather than turning up anything interesting.

A more subtle set of problems could be called concept incompatibility. In a classic article, Sartori cautioned against comparing systems that are fundamentally dissimilar to one another. Things that one would compare must belong “to the same genus, species, or sub-species—in short to the same class,” he wrote (1970, 1036). He linked this to his conception of the very purpose of comparative politics, which he saw as “a method of control” in testing hypotheses (1970, 1035).¹ Going beyond such “taxonomical requisites of comparability” is precisely what leads to “conceptual stretching,” which generations of political scientists came to recognize as a cardinal sin (1970, 1036). Sartori went on to criticize examples of such hyper-elongation, such as what he considered flawed applications of the terms *pluralism* and *participation*, rooted in “Western democracies,” to non-Western, non-democratic polities. So too, concepts like *mobilization* “originate from a totalitarian context,” and applying them to the West presents a fallacy of “reversed extrapolation” (1970, 1050–52).

Sartori’s article, as well as related critiques in the same era (LaPalombara 1975), reacted against a tendency in structural-functionalist theorizing to paper over deep differences in an effort to apply universal political abstractions. They inveighed against equating particular practices or institutions that have superficial similarities, yet actually work in profoundly different ways.

Sartori’s taxonomical metaphor, with its injunction against comparing across genera, may have seemed like a prudent corrective to problematic scholarly trends in 1970, yet appears too confining for today’s world. (It

¹ We should assess hypotheses and other generalizations against “all cases,” Sartori wrote, but those should be all cases within the relevant taxonomical class.

would seem odd to the modern biologists who compare DNA, evolutionary patterns, and more across kingdoms, let alone across lower taxonomic ranks.) His skepticism that any category might travel effectively from “the West” to “Africa or South-East Asia,” now seems antiquated. Both Sartori and LaPalombara questioned whether political “participation” could happen in communist regimes, yet certain forms of participation, including grassroots protests, are frequent in today’s China. While Sartori objected to using the term “mobilization” in democracies, today it is well-accepted that individuals do not always engage in democratic political action purely on their own initiative, but rather are driven to act by friends, organizations, inspiring leaders, and so forth. One can simultaneously note this and also bear in mind a vital distinction in kind between this and the type of ruling party orchestration that, in autocracies, compels people to cast ballots in sham elections and the like.

The key point here is that we can investigate related phenomena across contrasting political systems, without losing sight of nuances, frictions, and the possibility that they have radically different meanings. Indeed, assessing conceptual fit with care and attention to context is a significant purpose and contribution of comparative work. It is by doing so that we guard against thoughtlessly and misleadingly assimilating unlike things, a danger that Sartori was right to warn against.

Making the case for some degree of comparability across highly dissimilar systems is a crucial part of an investigator’s task in such research. A general approach is to argue that an area of politics exists that follows its own distinct rules and patterns, perhaps somewhat isolated from other aspects of the political system, or at least not wholly reducible to it. Zhang, for example, does this in her analysis of the politics of urban preservation, examining how different kinds of governmental fragmentation in Beijing, Chicago, and Paris dictate which historical buildings are protected and which are bulldozed (2013). Another example is Thomas J. Christensen’s *Useful Adversaries*, which remarkably compares the United States and China from 1947–1958. He argues that in both countries, leaders stoked low-level conflicts in order to rally the public for long-term security strategies, and that such frictions can spiral into unwanted wars, such as the Korean War (1996).

By their nature, comparisons among highly dissimilar political systems practically require the researcher to confront deep conceptual issues—and qualitative research has an important role to play in so doing. If, as Gary Goertz writes, “a concept involves a theoretical

and empirical analysis of the object or phenomenon referred to by the word,” it is natural that those working at and around conceptual boundaries will carry out much analytical work (2006, 4). Often the posing of the comparison itself requires or stimulates such efforts in at least two different forms.

Framing the Comparison and Evaluating Fit

In designing a cross-national comparative project, the researcher identifies *what* is to be compared. He or she establishes a universe of relevant cases. Much methodological advice addresses the question of how to select cases once that universe has been defined, but the prior step may be more a matter of creative perceptiveness than of following rules and prescriptions. In Zhang’s book, for example, the mere juxtaposition of Beijing with, say, Chicago is itself a startling and intellectually disruptive act for readers accustomed to thinking of these places in completely different theoretical contexts. Particularly when considering widely varying political systems, these “scope” decisions may require substantial research and can be consequential contributions in themselves. As with framing processes more generally, these decisions create an “interpretive schemata that simplifies and condenses the ‘world out there’ by selectively punctuating and encoding objects, situations, events, experiences, and sequences of actions” (Snow and Benford 1992, 137). Having done this, the researcher scrutinizes the crucial attributes that include certain cases in a common set and exclude others. These characteristics, on which so much hinges, require ongoing evaluation in relation to the purpose and justification for the comparison.

Concept Development

Comparison across dissimilar systems often provides opportunities for conceptual innovation and development even as it poses risks of “stretching.” Wide-reaching comparisons can, of course, draw on existing conceptual definitions, but I argue that they are relatively more likely to create opportunities for new departures. This need not, and should not, take the form of haphazardly extending concepts to places where they do not fit. Rather, it can mean defining or discovering categories of empirical phenomena that differ from what is already known and accepted—whether or not they are so novel as to constitute “unidentified political objects” (Jourde 2009, 201). These might be tangible, such as a particular type of organizational structure, or intangible, such as a kind of dynamic within a social movement. Tsai’s comparison of “cosmopolitan capitalism” in localities in India

and China, for instance, extends the usually domestic-centered concept of state-society relations to encompass transnational migrants and diasporic communities.

Such possibilities for innovation should be recognized as an important part of what cross-system comparison accomplishes. (The reader will note a kinship with the “casing” process discussed by Soss in this symposium, and to Htun and Jensenius’s points on conceptual development.) Innovations through framing and concept development are separate things and need not co-occur in the same project, yet the two are related to one another. As we shift focus away from the familiar and towards less-similar cases, our attention is drawn to conceptual aspects of the cases that went unnoticed or seemed unimportant in other perspectives. (“Unlike the other cases, Indonesia never had A and instead had B, yet it is similar to the others in terms of C, and I wonder if that operates through the same mechanism ...”). At the same time, spotlighting different features of the concepts may catalyze efforts to find related, heretofore unexamined, cases that share those features. (“I wonder if Malaysia has something like that ...”).

Comparing Ultra-local State-sponsored Organizations in China and Taiwan

Taiwan’s period of authoritarian rule under the Nationalist Party (KMT) from 1945 to the early 1990s was akin in various ways to authoritarian China under the Chinese Communist Party (CCP). A number of most-similar systems studies have compared the two.² Since Taiwan’s democratization, however, the two have diverged politically. Even as the opposite sides of the Taiwan Strait retain cultural, social, and linguistic commonalities, present-day comparisons now present special challenges, as well as opportunities.

A project of mine that examined state-society interactions at the very most local level of cities in China and Taiwan illustrates the kind of work entailed in navigating such challenges and opportunities. I sought to explain ways in which citizens looked upon and interacted with government-structured neighborhood organizations, which bring state power and authority into the ultra-local sphere of residential communities. I compared specific institutions in (mainly) the capitals, Beijing and Taipei. In the former, my subject was the official neighborhood organizations, known as Residents’

Committees (RCs; *jumin weiyuanhui*), that the Chinese state has maintained since the early 1950s. In the latter, it was the state-sponsored neighborhood offices (*li bangongchu*) that date to the KMT’s arrival in the mid-1940s.

What was the basis for comparing these? They have a number of things in common. Both are part of a nationwide network that covers all urban space.³ While organizational details vary somewhat by locality, they are mandated in national law and correspond to a unified template. A neighborhood has no choice whether or not to have such an office. In both countries, they handle a very wide range of responsibilities. They serve as what might be called all-purpose contact points for state agencies at the community level, for instance helping the welfare bureaucracy to determine households’ eligibility for assistance programs by drawing on their local knowledge of residents’ circumstances. They also field a seemingly endless variety of queries and demands from their constituents. The similarly statist structure in which they are embedded facilitates the comparison. Comparisons to more liberal settings are harder.⁴

Taiwan’s institutions certainly had significant differences from China’s. Taiwan’s neighborhoods have but a single leader, a warden (*lizhang*), who is partnered with a civil servant (*liganshi*). China’s RCs, as their name indicates, are larger committees of 3–7 people, and have become parts of even larger “community” organizations. The two variants differ dramatically in how their leaders are chosen. Taiwan’s wardens are selected in open, fair, and usually competitive elections every four years, whereas triennial elections for China’s RCs are heavily stage-managed. Also, while neighborhood bodies in both places cooperate closely with the police, in China the police do not merely fight crime but also tamp down and root out dissent. In Taiwan, much more than in China, residents are free to organize independent community groups of their choice. Moreover, while neighborhood Party committees embody China’s firm insistence on the CCP’s monopoly of political organization, in Taiwan, neighborhood leaders can affiliate with any political party (or run as independents, as many do).

Given this constellation of similarities and non-trivial differences, does comparison across the two commit a taxonomical error in Sartori’s terms? A key task in such research is determining in *which aspects* might one fruitfully compare them, and *what benefit* might come from doing

2 I review such comparisons in “China–Taiwan Comparisons: Still Promising Though Not ‘Ideal,’” Harvard Workshop on Chinese Politics, February 23, 2018, http://cnpoliticsworkinggroup.org/wp-content/uploads/2018/02/Ben-Read_China-Taiwan-Comparisons.pdf

3 In China and Taiwan alike, these organizations also have rural counterparts.

4 One article compares Shanghai’s RCs to Los Angeles’s neighborhood councils. This results in some astute observations, but mainly a catalog of structural contrasts (Chen, Cooper, and Sun 2009).

so. To blithely take the Beijing “elections” at face value, for example, would indeed conflate categories and risk stretching concepts beyond meaning. Or, to give another example, comparing these institutions in terms of their relationship with political parties would produce little or no new insight; they differ just as we would expect of organizations in a single-party authoritarian regime and a pluralized democracy.

But if we think of them in terms of the role that they play in ordinary people’s lives as intermediaries between state and society, a more productive basis for comparison emerges. One might expect that their facilitation of administrative and policing work would be strongly and universally disliked in China, in particular, given the Chinese state’s repressive nature. This was not the case, providing one aspect of a puzzle. Thus, I undertook the task of explaining variation in residents’ opinions of and interactions with these organizations in both Beijing and Taipei.

On one level, this comparison involved a logic of control, exploring how regime type affected or did not affect outcomes. People’s perceptions of grassroots-level governance indeed *differ* along certain lines. For instance, residents in China largely acknowledge that their elections are rigged, whereas those in Taiwan appreciate the agency they have in voting for favored warden candidates or against a poorly performing incumbent. More surprising were broad *parallels*. Residents of Beijing and Taipei had many similar patterns of opinions and perceptions of their neighborhood leaders, it turned out. Those who didn’t like them, or found them unimportant or irrelevant, did so for the same types of reasons (for instance, young, childless professionals whose lives had little connection to the neighborhood often felt this way). On the other hand, those with more favorable views (often, for instance, elderly residents, or those with businesses in the community) looked to neighborhood leaders for help with similar kinds of problems, and often appreciated their keeping an eye on the locality. In a nutshell, I traced this to, first, the similar webs of interpersonal networks that linked people with their community chiefs through various kinds of social structures, activities, and services, and second, to a shared and widely prevalent vision of the proper state-society relationship as being close and intimately cooperative. Looked at in this way, a set of common “inputs,” X_a , led to a similar distribution of values of Y despite differing contexts, X_d .

Yet the nature and benefits of the comparison went beyond this basic “method of agreement” logic.⁵ The very process of working across systemic differences rooted in parallel but divergent histories reframed how I thought about the topic itself. It forced me to re-confront the question “what is this a case of?” Originally I had thought of the RCs as part of a category of “mass organizations” common in other Communist systems. What became clear through the process of reframing, however, was that, in fact, this topic was one of broader relevance found not merely in the cases of China and Taiwan, but in a diverse set of political systems.

My term for the phenomenon under study was administrative grassroots engagement, or institutions “in which states create, sponsor, and manage networks of organizations at the most local of levels that facilitate governance and policing by building personal relationships with members of society” (Read 2012, 3–4). Establishing this category did not happen overnight, but emerged from an extended process of research: conducting more than 20 months of immersive fieldwork in China and Taiwan, reading widely, working with other scholars, making a field research trip to a third country, and ultimately co-editing a book on related cases (Read and Pekkanen 2009).

Countries featuring such institutions included Japan, South Korea, Vietnam, Singapore, and Indonesia. In today’s world, they span a range from autocracies to democracies; regime type seems less important than having a certain kind of statist and corporatist orientation in which the state proactively structures and draws upon grassroots institutions. Exploring the boundaries and background of this conceptual category brought into focus the historical connections that link certain key cases. Some are fairly well known among specialists, such as imperial China’s *bao-jia* institution. At the same time, not all variants can be traced to direct historical predecessors, nor is the phenomenon entirely confined to East and Southeast Asia. States develop and maintain their own forms of grassroots engagement institutions in response to varying imperatives and pressures.

The point here is to emphasize the important aspects of comparison that involve framing the cases and defining the subject under study. The comparison, relatedly, enabled different and deeper forms of conceptual work than would have been possible in a single-country study. For example, while administrative grassroots institutions are always distinct from autonomous civil society groups,

5 The project involved comparisons at multiple levels: among different residents within a neighborhood; among different neighborhoods (with different kinds of leaders, housing, and demographics); and among different cities. Here I focus on the cross-national dimension.

they pursue related activities and, in some ways, mimic the functions, activities and rewards of voluntary groups. So too, while the organizations in question overlap and have some commonalities with Gregory Kasza's administered mass organizations (1995), they also buck this categorization in various ways, from the voluntary nature of citizens' participation to their persistence in political systems that have transitioned from authoritarian to democratic regimes. They thus hold a number of conceptual surprises.

In carrying out this comparison, extensive field research involving interviews and ethnographic site visits played an essential role. This was certainly true for the Millian aspect of the comparison. Interviews, for example, were crucial for assessing how residents perceived their neighborhood organizations (the Y variable), and for delving into and evaluating possible reasons behind those perceptions (the X variables). Yet these up-close techniques were just as important, if not more important, for the conceptual work involved and for understanding how the Chinese and Taiwanese institutions were similar to or different from each other and how they related to other country cases. Immersion in each locale made it possible to navigate the dangers of interpreting Taiwan in overly Sino-centric terms, and of conceptually stretching by assimilating it too much to China, or by equating state-sponsored organizations with independent ones. It allowed me to parse the commonalities and divergences, and fine-tune an assessment of the kinds of power that these local authorities hold (Read 2018).

Conclusion

Comparing things that are unlike each other in far-reaching ways indeed has pitfalls, as Sartori pointed out. Yet his critique should not deter us from considering ambitious and creative juxtapositions. Comparison

across dissimilar systems does not necessarily mean committing errors of conceptual incompatibility or blurring categories to the point of meaninglessness. The key to avoiding trouble is not to confine oneself only to comparing political systems of the same "species." Rather, it lies in remaining aware of the full meanings and contextual dependencies of concepts one is working with; staying alive to the danger that the comparison is putting square pegs in round holes. At the same time, if there is some reliable path to innovation, it must be simply and constantly stepping back and reconsidering the fit between phenomena under study on the one hand, and existing social science concepts on the other. It requires a combination of thoughtful caution and initiative.

How should one preempt or respond to a reviewer's Sartorian critique? The best advice seems to be not to duck the potential problems or issues that your comparison raises, but address them squarely and up-front. Explain why what you are studying spans taxonomic categories in meaningful ways. In the style of a devil's advocate, adopt the skeptical perspective, work through the possibility that you are "getting it all wrong," and then explain to the reader where that perspective fails and what important truths it would miss.

Finding novelty while avoiding "stretching" is facilitated by up-close, in-the-field analysis. Immersion, whether in textual evidence or in field sites, gives the researcher continual opportunities to sharpen his or her sense of how best to conceptualize the political world (Kapiszewski, MacLean, and Read 2015, 20–26). One needs this kind of sensibility to navigate between the errors of a taxonomical stay-in-your-lane rigidity on the one hand, and pell-mell conceptual conflation on the other.

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“An Ode to her Revolt:”¹

Remembering Lee Ann Fujii

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The Dignity of Complexity: Being Human in Political Science

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The last conversations I had with Lee Ann Fujii had nothing to do with violence, research methods, or political science. Called unexpectedly to her mother’s deathbed in Seattle after a week of meetings in Washington, DC, Lee Ann was out of clean clothes and preparing to catch a hurriedly-bought flight. And so, on a chilly, drizzly grey morning in Baltimore, I left her in my apartment with a laundry card and ran off to a meeting. Returning home a few hours later, I opened the door to find Dr. Lee Ann Fujii—academic heavyweight, author of burn-it-down speeches, and potential future tenure-letter-writer—decked out in snake-print yoga pants and cheerfully folding *my* laundry as well as her own. Laughing at my abject horror, Lee Ann explained that she found folding freshly laundered clothes calming. And indeed, as our conversations turned to family dynamics surrounding acute illness, the #MeToo movement, and the intricacies of real estate acquisition, the smell of clean laundry soothingly lingered around us as rain pattered against the windows.

Though Lee Ann was never one for formality, the sheer humanness of the moment encapsulates much about her approach to her scholarship, as well as her life, in general. So much about Lee Ann’s professional existence was deadly serious—her in-depth explorations of violent performances; her commitment to ethics and ethical practices in research; her insistence that political scientists recognize and ameliorate racist and sexist structures in the discipline—that her bubbly, frank, accessible personality surprised some. Lee Ann’s intellectual genius rested within her embrace of this

apparent tension, indeed, it rested upon the complex intertwining of these seemingly incompatible traits.

Informants, Subjects, Participants, People

Lee Ann was a superb scholar of violence precisely because she embraced her own humanity and the humanity of those around her: research participants, students, mentees, colleagues. Her ability to recognize and appreciate the significance of banal human interactions gave her unparalleled insight into people’s relationships with each other and with power. Specifically, Lee Ann had an ability to *notice* (Bond, this issue) how simple moments—such as folding a mentee’s largely worn-out clothes—fed into larger, more complex relationships. In the case of the laundry, Lee Ann’s laughter resulted from both my facial expression and from the innate pleasure she took in inverting our profession’s most revered hierarchies. The effect was not simply to remind me that she was human, but that I was, too.

This acuity regarding the mundane nuances of relationships made Lee Ann uniquely equipped to engage with the complexities of mass killing and structural racism. She insisted on treating even the most elite individuals—whether a chaired professor or the former leader of a genocidal militia—as inherently *ordinary* people, and worked from that extraordinarily simple foundation to unravel why they behaved the way they did. Doing so allowed her to analytically underscore links between social relations and violence, especially by exploring the performative aspects of killing (Fujii 2017c). It also underpins the work she did to unmask the many faces of privilege in the discipline, perhaps

1 Monet, Aja. 2017. “wit.” *My Mother Was a Freedom Fighter*. Chicago, IL: Haymarket Books.

most superbly via a discussion of the disparate treatment of white and black mediocrity in the sanctified halls of academia (Fujii 2017a). In a profession that tends to study extremes and to view its members as necessarily exceptional, I continue to see Lee Ann's spotlighting of mediocrity as a turn of brilliance.

Lee Ann's scholarship impels many critical reflections on humanity, often in ways that political scientists hesitate to accommodate (let alone celebrate). I address two more here. The first involves the extent to which researchers allow their participants the dignity of complexity. In her recent volume on relational interviewing, Lee Ann recounts the story of interviewing an elderly man on Maryland's Eastern Shore who "had a reputation for being one of the biggest racists in town" (Fujii 2017b, 25). According to her contacts, "Old Timer" (as she calls him) was an important interviewee precisely because he was an elite, old, racist, white man; Lee Ann believed he might shed light on the events surrounding a lynching that occurred in 1933. Yet, during her interviews with him, Old Timer both condemned the lynching (for which he had not been present) and refused to discuss it because "[t]o this man, there was dishonor in the telling itself, especially if it meant passing on anything that was salacious or damning about those who gave him the information" (Fujii 2017b, 26). Lee Ann operated within Old Timer's parameters, challenging and stripping away her own assumptions about him as their conversations progressed over the course of four meetings. During their final interview, he confirmed the location of the lynching, a piece of information Lee Ann had long sought.

For Lee Ann, this relationship illustrated how demonstrating respect and boundaries allowed her to build a productive relationship with Old Timer. Yet the interlude also reveals the way that allowing participants' own humanity—in many ways, their ordinary-ness rather than their elite-ness or their key-actor-ness—into research interactions can provide enormous richness in data. By interacting with people *as people* Lee Ann opened new avenues to crucial data. In this way, Lee Ann's work elucidated violent processes in ways that rang true because she centered the fact that it was real people—not data points, not subjects, and not cases—who participated in lynchings, massacres, and genocide. By contrast, many other researchers might ontologically or methodologically foreclose the analytical possibilities posed by the contradictions, hesitations, or even outright dishonesty that naturally emerge over the course of researcher-participant relationships (Fujii 2010). Lee Ann protected the dignity of being human—that is,

of being fundamentally complicated—for her research participants by embracing her relationships with them as forms of data in their own right. The model her work provides is one that should inspire future violence scholars to better situate violence in the complexities of human life, rather than attempting to isolate it from other social processes.

Complexity and Complicity

The second relationship involves the extent to which researchers allow themselves to be complicit in harmful societal and disciplinary power relations. On this note, Lee Ann's contribution to the Spring 2016 issue of the Comparative Politics newsletter (Fujii 2016) is a masterclass in how scholars of color perform immeasurable intellectual and emotional labor for their white colleagues around issues of race and inequality. In it, Lee Ann walks scholars through a hypothetical scenario involving an academic who wishes to publish archival research on lynching. In the scenario, the researcher is asked to provide the journal with the violent images of lynchings that she accessed, in the interest of "transparency."

Lee Ann deftly uses this example to highlight three things that should be painfully obvious. The first is that encouraging context-less viewing of material, such as photos of white-on-black lynchings, in the interest of validating scholarly analysis, renders viewers complicit in the reproduction of spectacular violence. This is particularly straightforward when we consider that the original intent of lynchings was largely to *encourage spectatorship in the dehumanization of black bodies*. Second, to expect non-white scholars to engage with images of this sort on the same terms as white scholars may, for some, offer a false choice between participating in one's own dehumanization by engaging or accepting one's continued marginalization by refusing. Third, and most importantly, both the spectatorship and the false choices are forms of victimization that our discipline frequently replicates in other ways. Indeed, Lee Ann notes: "for those who can imagine ourselves at the end of the rope or chained to the tree, the images live inside as much as on the page" (2016, 25). This single line demands that many scholars recognize not only that *they have never had to imagine themselves at the end of that rope*, but also that *some of our colleagues have*. Scholars' experience and understanding of those images would necessarily differ as a result, and this ought to be reflected in our scholarship without reminder. Lee Ann spent far too much of her career trying to teach people the significance of this simple reality.

Lee Ann was calling attention to the harm that scholars often unwittingly commit against each other and to the people and communities they study (including after death). That powerful “*j’accuse*” moment continues to inspire both my own ongoing scholarship on the methodological and ethical challenges of research in refugee and displaced person communities and other fragile contexts (Lake and Parkinson 2017; Parkinson 2018). Along with many others, my research projects have provided solid evidence that it is ethically and scientifically problematic for the scholarly community to ignore pervasive issues, such as over-research (Sukarieh and Tannock 2013); the damage done by unskilled, irresponsible, or entitled interviewers (Foster and Minwalla 2018); and the underrecognized and sometimes exploitative labor performed by local translators and research assistants (Cronin-Furman and Lake 2018). These and other, related problems all reduce the quality and validity of scholarly data, along with the broader societal value of the research. Lee Ann’s example continues to remind me that in order to be better social scientists, we must also interrogate our own practices and structures, both because doing so improves our ability to contribute to knowledge, and because those practices and structures affect actual people.

Being Human

I don’t remember what Lee Ann said to me when she got on the elevator in my building back in February. Instead, the complex interplay of grief and memory allows that I only vividly recall the brand-new, pointy-toed, leopard-print, calf-haired Chelsea boots that she held out excitedly as she packed in the middle of my living room. Lee Ann’s repeated insistence that I do nice things for myself—a fancy dinner, a massage, a shopping trip—as I revised my book stands as yet another helpful reminder that respecting one’s own humanity in the process of scholarly work often makes it better.

Lee Ann taught us that the surprising tensions and unexpected juxtapositions in human existence—the mentor who does your laundry, the racist who condemns the lynching, the well-meaning scholar who is unwittingly complicit in structural violence—are essential to our understanding of political life. That comprehending the complex dynamics of political violence, racism, or any number of social processes that we study, requires us to admit that we fundamentally study *complicated humans as complicated humans ourselves*. That upholding the dignity of complexity for *all of us* is the first step in becoming better scholars and people.

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Unmasking: The Role of Reflexivity in Political Science

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As a discipline, political science's pace in tackling themes of racialization and gendering in our research methods—or the field more broadly, for that matter—has been glacial.¹ As other scholars note, this is in part due to the working assumption within the discipline that race arises and exists on the periphery of 'real politics' (Hawkesworth 2016; Smith 2004). This disembodied account of politics (Hawkesworth 2016) upholds the myth of 'neutrality' within the discipline. Accordingly, the discipline presumes to operate from a place of racelessness (Fujii 2017; Hendrix 2002). In practice, however, the default subject position of a presumed racelessness is actually whiteness (Fujii 2017; Hertel, Singer, and Van Cott 2009; Mazzei and O'Brien 2009; Townsend-Bell 2009).

The resulting effect is the normalization of a set of ideas, grammars, and practices that reinforce the discipline's epistemological authority, thereby codifying the status quo. Further, it obscures how the structures of political science are entrenched in power relations of dominance and subordination that render invisible the viewpoint and lives of others. For example, with few exceptions, the scholarship is not conversant in the racialized and gendered experiences of Black and Latinx self-defined women in the Americas. A review of the table of contents from the discipline's prominent journals can attest to this lacuna.

For Dr. Lee Ann Fujii, my doctoral supervisor and mentor, addressing disciplinary shortcomings started with two important interventions: the adoption of reflexivity as a central praxis and the diversification of the racial and gender demographic within the discipline. She reasoned that reflexivity, a practice requiring the active and ongoing process of self-examination at the individual and epistemic level, could bring issues of racialization and gendering to the forefront of the discipline's methodological and epistemological considerations.

In a meeting prior to my departure to conduct fieldwork, Dr. Fujii gave me the following indispensable advice:

Who you are, your experiences, your perspective, how you experience(d) the world, and how you make sense of the world has informed your research project.

As a Black woman in Canada you are likely to have a different viewpoint from your white male counterpart.

Use it. Do not shy away from it.

Tell us how that informs how you see the world.

That is what I want to read about.

— Doctoral supervisory meeting,
Lee Ann Fujii to Lahoma Thomas,
November 2016

Dr. Fujii's assertions are a reminder that our research does not emerge absent the influence of our identities. Our subjectivities—that is, the inner ensemble of our affect, thought, knowledge, perceptions, and experiences (Luhmann 2006)—leave an imprint at every step of the research process, thereby shaping the knowledge we are able to, and do, construct (Banks 1998; Collins 1986). Reflexivity thus requires the discipline to seriously take on the process of self-examination as a methodological practice—to explicitly examine how each practitioner's personal history, ideological commitments, beliefs, values, assumptions, and subject location(s) as researchers affect all facets of the research process, from inception to conclusion. In concrete terms, we should be asking ourselves how our positionality informs the conceptualization of the research project, the development of methodological practices, the researcher's interactions with others, the collection of data, the recounting of research informants' experiences, the interpretations of these, and the decisions about what to publish and what to omit.

The practice of *Relational Reflexivity*—an active ongoing process of self-examination, attuned to how power is reinforced through particular subjectivities (Thomas 2018)—has been key to my methodological approach to studying the role of respect in the relationship between criminal organizations and residents living in the communities the former control. A commitment

1 Notable exceptions include Fujii (2015, 2017); Hawkesworth (2016); Alexander-Floyd (2012); Townsend-Bell (2009); and Yanow and Schwartz-Shea (2006).

to *Relational Reflexivity* demanded that I interrogate my subjectivity as a Canadian-born Black woman of Caribbean descent while conducting research in the inner-cities of Kingston, Jamaica, among people with whom I share a sense of racial and cultural affinity. This made me attentive to ways in which one's movement through different geographical, socio-political and relational spaces reconfigures and reinforces the dimensions of one's subjectivity. The analytic richness of this process provided important insights into how relations of respect operate as a site of power that fluctuates in complex and unexpected ways. For example, I became attuned to the role respect plays in the political behavior and strategies of residents, beyond the cost-benefit calculations.

Taking stock of my biography made me acutely aware of the influences on my 'insider-outsider' status as a researcher (Collins 1999; Mullings 1999) and attentive to how the residents I interviewed—primarily black women living in the inner-city communities of Kingston, Jamaica—were not represented in the conventional models of political science.

If we take seriously the proposition that our subjectivities influence how we research and explain the world, we can reason that the dominant theories in political science have emerged from a particular raced and gendered perspective. Acknowledging this fact makes it evident that to develop a full picture of the social world requires scholars with diverse biographies.

Dr. Fujii's advice to me was only one aspect of her deep epistemological and methodological commitment to advancing political science by disrupting its core assumptions and expanding the idea of who the legitimate producers of knowledge are. The case for racial diversity in the field of political science should not be a matter of institutional desire to signal inclusivity; those are often fleeting interventions that can produce informal departmental quotas and overworked racialized scholars. Rather, the diversification of the field of political science is a necessary course of action if the intellectual commitment is to accurately understand and explain our social world.

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Reflexivity and Revelation

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“Paying attention to accidental moments . . . starts with noticing stories and encounters that catch the researcher’s attention, without prejudging how they will relate to her research later” (Fujii 2015, 536).

Professor Lee Ann Fujii was an expert at *noticing*. In my experience, she was just uncannily good at remembering details, sussing out nuance, picking up on shaded distinctions, and hearing the scream in a whisper. This is likely a large part of why she was so skilled at recognizing, parsing, and illuminating the complexities of person-level political violence. Lee Ann was intrinsically committed to understanding how personal interpretations of one’s own relationship to violence influence its production, and vice versa. The source of this sense-making was straightforward to her: individuals make decisions in context, and neither can be well understood without paying systematic attention to the other.

Lee Ann’s encouragement to embrace the “productive potential of . . . those periods when the researcher is no longer trying to ‘control for bias,’ but navigating a social environment . . . from which she cannot fully remove herself” has greatly influenced my own scholarship, which bridges positivist and interpretivist approaches (Fujii 2015, 527). She confirmed for me that, whether by training or by accident, paying attention to the interstitial moments of violence research—sandwiched among episodes of high drama or instrumental procedure—reveals much about how individuals organize their worlds, execute their choices, and process meaning (Fujii 2015, 526–7). She supported me in remembering that violence, its perpetrators, and its victims co-inhabit a universal context with violence research and researchers: Individually and collectively, all embody the material consequences of socially-constructed relationships (Fujii 2018).

“Once the researcher begins to pay closer attention to accidental moments, she might begin to make discoveries. She might become aware of expectations she did not know she had . . .” (Fujii 2015, 527).

It is not controversial for interpretivist researchers, such as Lee Ann, to acknowledge that the interpretation of observational data always reflects how researchers navigate the world we co-inhabit with the individuals, events, circumstances, and systems that we purport to study. It can carry a different charge for those who engage positivist methodology, and particularly for those who work primarily with quantitative data and statistical hypothesis testing.

Minimizing bias—systematic and non-random deviations in our measures, evaluations, and estimates of reality—is particularly important to positivist orthodoxy, in its elemental search for some objective, stable, and fundamentally observable Truth. For scholars who use statistical methods, deep consideration of how well our data-handling strategies help to offset the effects of bias should be, and often is, second-nature. However, too many smart positivist scholars wrongly equate reflexivity—the sort of scholarly *noticing* that Lee Ann advocated—with introducing bias where it previously did not exist. Precisely to the contrary, in quantitative as much as in qualitative research, reflexivity is a way of neutralizing pre-existing bias to improve the scientific value of our work. It invariably adds more and better data that we can use to describe more accurately the breadth, depth, and distribution of social experiences. It is inferentially edifying as it bares the distortionary effects of erasure (observation and sample selection bias), ignorance (omitted variable bias), and misrepresentation (measurement bias).

Reflexivity also helps us to estimate bias’ effects on not only what we observe, but also how we do it. Mere identifications of bias are themselves statements of positionality: For each scholar, they evince whose truths are ineligible to be understood as lies; which experiences have been normalized into standards; and what perspectives stand reified as canon.¹ All observational researchers then rely on these often-unstated assumptions to determine what sorts of information qualifies as data that we can use to represent (someone’s) truth. This is not an objective process, nor can it be made more so simply by choosing one method or another.

1 One excellent example of positivist work on this topic is *Media Bias, Perspective, and State Repression: The Black Panther Party* by Christian Davenport (2010).

“She might notice the different ways that people type her. She might detect similarities in social dynamics across dissimilar sites . . .” (Fujii 2015, 527).

Lee Ann pushed all political scientists to purposefully account for how the bodies and social spaces we occupy and navigate affect our work, from conception through execution and reflection. She particularly saw the perspectives of historically marginalized folks as critical to bolstering the collective rigor of our field. We agree that they are not weapons, nor ought we allow others to weaponize them through insisting on self-awareness as a danger to science. Take me for example: as a Black American woman *inter alia*, the stories, dynamics, processes, and relationships that catch my attention are opportunities to understand the nature and value of political contestation in distinct perspective. My experience-informed intuition tells me that the difference between inclusion and tokenization, and the range of options available to power-holders for disguising it, is often more real than apparent. Perhaps as a result, I collect and analyze large-N data on militant organizations that accounts for the ways that organizational attitudes about sex and gender are codified in official, public rhetoric *along with* information about the actual presence and activities of people of different sexes and genders (Thomas and Bond 2015). The same intuition reminds me that ‘militancy’ is a perceptual quantity that, depending on one’s vantage point, can be understood in systematically different ways. Consequently, I also examine structures of social dominance within these organizations *accompanied by* information on their outwardly-directed activities (Bond 2016). Both are approaches that few others have taken. I have no idea how many others have considered them.

“These smaller, less dramatic moments can reveal patterns, logics, and practices that other, more procedure-driven methods cannot” (Fujii 2015, 527).

Lee Ann and I were continuously “finding revelation in the mundane” of our own lives (Fujii 2015, 526). She ribbed me endlessly on the (what she called boring) titles I give to my work. We regularly debated our exercise preferences. We shared raw honesty about how taxing immersive research on the production of violence can be on the emotions and on the intellect. We compared the sharp pains of racialized and gendered workplace macro-

aggressions and their common recasting as overblown inconveniences. We soon folded all of these conversations into a collaborative exploration of the devastating power of grief, and its curious generalizability. We listed as many ways we could think of through which grief decimates and concretizes one’s past all at once, and how it obscures and illuminates one’s future simultaneously. In the doing, we shared fury and sadness over how our common experience as racialized women never relents.

In the same vein we hated on the volume of emotional labor for which non-white women are often mined; that is, the ways in which we are required not only to notice but also to manage *others’* frustrations, insecurities, and, yes, even their grief (Matthew 2016). Years of observation and experience convinced Lee Ann that indifference, rather than embarrassment, is what keeps so many in the academy from uplifting, protecting, and championing many scholars of color and their work. She railed against the reigning diversity management paradigm of contemporary higher education, in which institutional gatekeepers ‘grant new access’ to individuals traditionally excluded from the power centers of academia (diversification) only to instantly assign competing values to their bodies and experiences; those values are wholly determined by the same gatekeepers’ ability to efficiently manage or trade them (commodification). Lee Ann knew intimately that the violence of institutionalized marginalization in our profession replicates not in the practice of *noticing* diversity, but in the business of *consuming* it.

“We can’t get through this BS without some sanity-making check-ins on a regular basis.”²

After publishing her exceptional analysis of racism in political science in the Duck of Minerva blog, Lee Ann told me: “It took me all these months to revise it . . . because everything about the issue enrages me.”³ To assert this in her written work was sanity-making in itself and, for her, a source of immense satisfaction. For Lee Ann, privilege was “simply having a choice;” the ultimate act of academic privilege amounted to “choosing not to think about whether to be bothered by inequalities in the field (or wherever).”⁴ In that blog piece, she located that privilege at the core of appraisals of quality in political science, which she called out as inextricably linked to “the kinds of bodies the faculty see as capable of *embodying* quality in the first place” (Fujii 2017). As

2 L.A. Fujii (personal communication, 2017).

3 L.A. Fujii (personal communication, 2017).

4 L.A. Fujii (personal communication, 2016).

she affirmed us, Lee Ann too took the racialized woman scholar's journey through the fraught territories of advocating for ourselves, and by extension, our work, as already so capable. Her scholarship and advocacy was as Kimberlé Crenshaw explicated intersectionality as praxis: challenging those parts of ourselves which are made "at home" in our environs, in the name of those parts of us that are not (Crenshaw 1991). Her commitment to requiring that political scientists at least *notice* the oppressive hierarchies in our discipline that "hide in plain sight" (Henderson 2013) and the scholars of color that traverse them, was often incredibly costly. It was also astounding.

Lee Ann is one of the precious few tenured, non-white, women mentors that I have known in international relations or comparative politics. She is also my trusted friend. As I experience acutely the loss of her presence on multiple dimensions, I remain grateful to have been strengthened by the warmth of her magnanimity and the force of her inspiration.

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Remembering Lee Ann in South Africa: Meta-Data and Reflexive Research Practice

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Lee Ann Fujii and I became fast friends, colleagues, and disciplinary comrades soon after we met at the 2004 Institute for Qualitative and Multi-Method Research (IQMR). IQMR presentations and workshops sparked fourteen years of conversation about the discipline, our positionality with respect to the discipline and research participants, methodologies, the “field,” and much more. Lee Ann made me laugh and encouraged me to think harder as we talked over coffee and chocolate at home in Oakland, New York, Washington, DC, Indianapolis, and Toronto; met up at APSA annual meetings; and practiced yoga together.

Deprived of her vital physical and phone presence, I still hear Lee Ann’s voice as I think through a recent experience in South Africa:

Sitting in a hotel room in a small North West province town on Tuesday evening, July 31, I receive an SMS from a tribal office assistant informing me that the *kgosi* (senior traditional leader or chief) and former regent are no longer available for interviews the next morning and will be away until Friday. This is the third time that this *kgosi* has postponed her interview, and I think she has deliberately run down the clock to avoid being interviewed before my Friday evening departure from South Africa. This *kgosi* has been welcoming in other ways, however, facilitating my entrée by introducing me to residents at a community-wide meeting and instructing the tribal office clerk to assist me. She also vouched for me with the woman traditional leader of a different community, speaking about me in a way which led that *kgosi* to agree to an interview.

Lee Ann encouraged and assisted scholars to analyze these sorts of research experiences without settling for simple explanations. She would have been disappointed if my interpretation focused solely on the hindrances to securing interviews that positivist researchers often label “access problems.” Revisiting her article “Shades of

Truth and Lies” reminds me that my interactions with the *kgosi* are meta-data, “spoken and unspoken expressions about people’s interior thoughts and feelings” (Fujii 2010, 232). “Meta-data,” Lee Ann wrote, “are integral to the research enterprise and constitute valuable data in their own right,” and are important indicators of “how the current social and political landscape is shaping what people might say to a researcher” (Fujii 2010, 232).

How might this meta-data inform this research on women’s involvement in traditional governance? Evasions, silences, rumors, and other meta-data can help attentive scholars to better understand and address the risks research participants may confront and to interpret other data generated through interviews and other formal research interactions (Fujii 2010, 232). For example, the meta-data that emerged from my interaction with the *kgosi* suggest that recent political uncertainty affects both the traditional leaders and their citizen-subjects whom I sought to interview, as well as those more directly involved in democratic politics and governance (Turner 2014; Williams 2010). Senior traditional leaders are selected by the “royal family” of their community and then appointed by the provincial premier. The July 2018 North West province research occurred in an environment marked by the December 2017 recognition and derecognition of numerous traditional leaders, the February 2018 resignation of then-President Jacob Zuma, the May suspension and subsequent resignation of Provincial Premier Supra Mahumapelo, the April and May national takeover of several provincial departments, and fierce struggles for political control of the province and the nation. The *kgosi* and other potential participants might have felt that an interview presents undue risk in this context, as being responsible for bringing local problems or concerns to a broader public might lead to reprisals.¹

Institutional Review Board (IRB) guidelines do not sufficiently address these potentially well-founded yet emergent concerns. This study was deemed to present “minimal risk” to participants, I abided by the IRB-

¹ I thank Mahlogonolo Rangata for raising this point.

approved informed consent protocol, and no reportable “unanticipated or serious adverse events” occurred.² My ethical obligations extend well beyond these procedures, however (Fujii 2010, 2018; MacLean et al. 2018). I share the participants’ worry about present and near future sociopolitical risk and their dissatisfaction with a “consent protocol [that] positions the researcher as someone who already knows more about the participant’s world than the participant” (Fujii 2012, 718).

My own research demonstrates how individuals negotiate perceived participation risks in different ways. I conducted individual interviews with two other women kgosi and several community residents who sometimes evaded or declined to answer especially sensitive questions. Other participants sought to mitigate risk by incorporating others into our interactions, in effect creating an accountability mechanism for both of us. Two participants chose to be interviewed in the presence of their friends or relatives. One large group chose to be interviewed collectively with a few people speaking on the entire group’s behalf; these spokespeople appeared to adhere to a preestablished script. Securing witnesses may reduce participants’ risk by ensuring other locals can attest the participant has not brought their kgosi or community into disrepute.

Another reading of my unsuccessful efforts to secure an interview with this kgosi would focus on her governance strategy. The kgosi has been involved in provincial and national traditional leadership structures since shortly after her appointment, and government officials often call meetings in other places at short notice. The repeated cancellations could be indicators of the extent to which the kgosi is physically present in her community. Her repeated cancellations may have had little or nothing to do with her willingness to be interviewed. These meta-data are open to multiple—perhaps concurrent—interpretations.

A Fujii-informed scholar also would consider these interactions from a relational perspective. Deeply critical of “the usual advice...to build good rapport,” Lee Ann contended that we should try to build productive “working relationships” in which researchers and participants “arrive, explicitly or implicitly, at mutually agreeable terms for interacting, conversing, listening, and talking with one another” (Fujii 2018, 12, 15). She insisted, “relational interviewers...treat everyone as ‘ends’ in themselves and not as a ‘means’ to some other end” (Fujii 2018, 6). In writing about these different

interpretations, I am engaging in the sort of reflective research that Lee Ann Fujii consistently advocated.

The ethical principles Lee Ann emphasized require researchers to acknowledge that power infuses every aspect of the research enterprise, recognize the “privilege that all researchers enjoy in gaining entrée into people’s worlds,” and attend to our positionality (Fujii 2018, 16). I bring to each research encounter both the substantial privileges associated with American citizenship and full-time tenured academic employment, as well as the complex signifiers of cis-gender femininity and embodied blackness, intersecting identities Lee Ann and I often discussed. These attributes shape how I am seen and how I see others, but do not determine the tenor of my individual interactions with women traditional leaders—themselves local elites with formal authority—their citizen subjects, and other participants (Fujii 2015; Turner 2016).

Describing research encounters and reflecting upon them in light of Lee Ann Fujii’s interventions makes my research process more transparent, albeit in a different sense than the DA-RT initiative she critiqued (Fujii 2016). But how might this “reflexive openness” affect the participants discussed in this text (MacLean et al. 2018)? As a practitioner of what Lee Ann termed “micro-level fieldwork,” I am pulled among dueling ethical impulses to protect participants’ identities, to make full use of the data generated through these research encounters, and to share my scholarship with participants (Fujii 2008). Lee Ann Fujii was a brilliant scholar whose methodological work raises a host of questions with no easy answers. The best I can do to honor her is to keep returning to these questions, and to the incisive, humorous, and supportive way she kept asking us to honestly confront ourselves, our scholarship, and our participants.

I miss her so much.

² Quotations are from the U.S. Office for Human Research Protections Common Rule procedures for expedited review (2009) and the Butler University IRB Notice of IRB Protocol Approval (2018).

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Taking “Professionalization in the Discipline” with Lee Ann Fujii

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The most important “class” I took during my time as a graduate student was not an official seminar. Rather, the one-on-one tutorials I received from Lee Ann Fujii covered much of what I most needed to know: about the ethics of fieldwork; professional norms in the discipline; how to stand up for myself as a young (ish) woman in this discipline. To say that Lee Ann Fujii changed the way I understand the ethics and practice of social science research would be an understatement. Over cups of tea in my living room or animated conversations on the phone or through email, Lee Ann challenged me to rethink my understanding of the field, especially by examining the ways power affects the discipline itself, the questions we ask, and how we go about answering them. Lee Ann loved a good discussion on research ethics and reflexivity, and I would like to continue this conversation by reflecting on a couple of the many questions I have wanted to ask her since her passing and questions I hope we continue to grapple with as a field.

Structural Power and Privilege

“The most fundamental privilege that all researchers enjoy is gaining entrée into people’s worlds. Choosing what to do with that privilege is an issue no researcher should take lightly. Some scholars will take the time to ponder such thorny questions as whose priorities—the researcher’s or the participant’s—should take precedence in a given moment or over time” (Fujii 2017, 16).

This spring, two months after Lee Ann’s passing, I entered my first refugee camp. The United States Agency for International Development’s (USAID) Office of Food for Peace sent me to run focus groups with women to discuss food security concerns. I was on an American Association for the Advancement of Science (AAAS) fellowship during the 2017–2018 year and by this point had been on several international trips with my USAID colleagues.

Refugee camps present serious ethical and practical challenges to researchers (Pittaway, Bartolomei, and Hugman 2010); my arrival with a US government team added to these challenges. I spent significant time leading up to my visit worrying about what voluntary

consent could even mean in this context, given the power differentials at play. I struggled with how to engage with protection concerns in the camps, reaching out to practitioners and academics to see if I might be overlooking risks to participants with the way my questions were worded. What I had put considerably less time into considering was how I might engage with disagreements during the focus groups on what topics *ought* to be discussed in the first place.

Repeatedly in the context of running focus groups, it became clear that what I believed was important to discuss was not always what the women in the group believed we needed to talk about. In one focus group, during a set of questions about power in the camps, two women interjected that we needed to talk about immigration and livelihoods. In another focus group during a set of questions about places in the camp that were safe and unsafe for women, women interjected that healthcare was the problem. There had not been a doctor in the camp for over a year. A woman disrobed to show a tumor on her abdomen that had not been treated. Another explained there was a woman whose uterus had prolapsed and she carried it wrapped in a piece of cloth attached to her hip, under her clothing so it would not get dusty. There was no doctor, they explained, to put it back in her body.

Because I had been sent as a part of a USAID team to investigate specific concerns, I was very explicit in the beginning of the focus group about why I was there, what work my office did, and how the information they provided would be used. I had explained that my office worked only with food assistance, and not health. Admittedly, this example is extreme—the disagreement about what *really* should be spoken about in these moments reflected serious and immediate needs that might not be as apparent in the course of other research.

But perhaps because of the extreme nature of this interaction, it has stayed with me as I have considered how we, as political scientists, decide what is really *worth* studying and how we decide, prior to entering our research sites, what topics and questions we will be investigating. On one level, this is necessary, as doing so

allows our interview questions and research protocol to be vetted through Institutional Review Boards (IRBs) and colleagues and allows us time to consider risks and sensitivities carefully before interacting with participants.

One thing Fujii's concept of "relational interviewing" does, however, is to challenge researchers to examine the power that allows them to feel entitled to decide what really *ought* to be addressed with research participants outside of dialogue with participants about what sorts of questions they think are important to address.¹ Fujii's practice of "relational interviewing" challenges us to more fully consider what it means to understand "interviewees [as] agents in their own right and hence partners in the interaction, rather than passive 'subjects.'" (Fujii 2017, 71). How then, might "relational interviewing" change not only the way we interview, but what we examine?

Critically and Collectively Evaluating Costs and Benefits

"None of these potential harms is a reason to avoid certain communities or research sites entirely for that would constitute another kind of harm—that of systematic neglect by researchers. Neither does it mean avoiding certain topics altogether" (Fujii 2017, 23).

Knowing under what conditions a researcher should avoid a topic of conversation and when she should engage it is a challenge Fujii grappled with throughout her scholarship (on avoiding the discussion of sexual violence, see Fujii 2010, 238). In conducting "relational

interviewing" it is important to be reflexive about potential risks to participants in research (and react accordingly) but yet not to close the door on entire topics or communities resulting in "systematic neglect by researchers" (Fujii 2017, 23). Working hard to find ways to mitigate risk instead of simply avoiding research areas that are "too challenging" is important.

And yet, it is generally the researcher (and their academic community and IRB) who has the power to make the decision of how much risk is too much risk, of what questions are safe, and whether benefits of the research outweigh the risk. When you are interviewing middle class aid workers sitting in Washington, D.C., this trade off does not seem particularly problematic. When dealing with situations of extreme vulnerability, such as life in refugee camps, however, the calculus is more fraught. When is it appropriate to ask people to trade their emotional energy and time to provide no benefit to themselves but perhaps to others in a distant future down the line? And are there more meaningful ways to give back to communities than those that have been normalized in the discipline? If we think of interviewees as "partners in the interaction" (Fujii 2017, 71), does this change how we understand what we "owe" our partners? Fujii grapples with this question when she interacts with the "man with the black notebook," (Fujii 2017, 94–95) but I am not sure she left with a clear answer. My hope is that her important work pushes the discipline to continue this conversation.

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¹ See also MacLean (2013).

A Posture of Active Reflexivity: Learning from Lee Ann Fujii's Approach to Research

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Although both qualitative and quantitative scholars have begun to recognize that researcher positionality is consequential for social research, many have yet to theorize fully about the practical realities of *doing* reflexivity in the field. Positionality refers to the demographic characteristics and personal backgrounds of a researcher's identity and their impact on interactions with research participants (Fujii 2017, 17). How, then, should scholars go about reflecting on the complexities of positionality during field research? As junior scholars, Lee Ann Fujii's research and mentorship prompted us to confront this question often. Her work on political violence and interpretive methods make clear that positionality is context-specific and fluid. Because positionality is particular to every context, the effects of a researcher's social location are never straightforward or static.

While we attempted to employ these insights during our research, we still confronted moments of surprise resulting from unanticipated effects of positionality in the field, and had challenges processing these occurrences. Lengthy discussions with Lee Ann after our return from the field pushed us to further reflect upon the inherent assumptions we make regarding others' interpretations of our own position, and challenged us to do so continually throughout the research process. We call this particular posture towards research "active reflexivity."

Positionality in Context

Data emerges from the interactions between researcher and participants (see Fujii 2017; Schaffer 2016; Yanow 2014). To generate knowledge, the researcher must interrogate each interaction. However, researchers often assume that the impact of one's gender, racial, or sexual identity on research is both self-evident and straightforward. For example, Berger (2015) assumes that migrant women were more at ease with her because of her accent, and Davis (1997) assumes that African Americans are less forthcoming with white interviewers than black interviewers. While these assumptions are

certainly plausible, both scholars claim that a single dimension of their identity has a similar effect on the responses of a class of interviewees.

Lee Ann's work rejects the imposition of rigid identity categories on both researcher and subject. Her research shows that individuals simultaneously hold multiple identities. These interact in myriad and contextualized ways, and they shape research interactions. Whether it is race, gender, sexuality, age, class, ability, or profession, the dimensions of identity can and do shape interactions between researcher and participant.

The meanings of identities and the privileges they bestow are not fixed. What it means to be black in America, for example, is different from what it means to be black in Canada or in Rwanda. As Lee Ann insightfully observes (2017, 17): "What combination of traits matter depends on what people in the research site find salient." In Lee Ann's (2009) *Killing Neighbors*, for example, she outlines how different elements of her identity—and how people (mis)read her—shaped her interaction with participants. For example, while Rwandans perceived her as a *muzungu* (foreigner), many also mistyped her as half-Rwandan. The impacts of these intersectional identities, however, shifted depending on the identity of the participant or how they typed her. Ignoring the dynamic specificity of positionality and the uncertainty of its recognition may lead to a process of reflexivity that imposes rigid categories on both the researcher and subject, and the relationships between them.

Beyond affecting the generation of knowledge, rigid reflexivity also risks undermining the building of ethical relationships. By contrast, recognizing that reflexivity is active and dynamic allows the researcher to build what Lee Ann (2017, 15) calls a "working relationship," where interviewer and interviewee "arrive...at mutually agreeable terms for interacting, conversing, listening, and talking with one another." Awareness of power dynamics arising from positionality can help researchers be more ethical, shaping strategies for obtaining informed consent

(i.e., taking extra time to go through the consent form), or guide decisions around where an interview should be located (Fujii 2017, 23).

However, the multi-dimensionality of identity makes the appraisal of positionality and its effects a complex endeavor. This was made clear through conversations with Lee Ann, who often asked “How do you know that?” in response to our assertions about positionality and its effects in the field. In an attempt to answer her question, we suggest scholars should adopt a posture of *active reflexivity*.

Active Reflexivity

Active reflexivity is a posture that embraces the sociality and dynamism of positionality. It denotes an attempted awareness of our own positionality, a confrontation of the inherent assumptions we make of the effects regarding our identity, and a concerted effort to do so continually throughout the research process.

The foundation of active reflexivity is humility, a trait Lee Ann stressed in her research and impressed in her mentoring. The particularities of positionality mean that one should avoid uncritically imposing her own understandings of how identity operates in each research interaction. For example, when Jessica was carrying out research on religious intolerance in Indonesia, she assumed that she would have greater rapport with those who shared a religious identity with her than those who did not. Yet this was not always the case, as she experienced when she interviewed Pastor John¹ about religious intolerance in a city in West Java province. Because Pastor John had occupied a similar milieu as her own family, Jessica assumed that the interview would be candid and informative. She even dismissed her Muslim research assistant for the day to get more “insider” information on interreligious tensions. However, despite perceived similarities, Jessica had trouble establishing a working relationship with him. He avoided discussing questions about religious tensions in Bandung city and redirected the conversation to the successes of the religious tolerance movement there. Upon later reflection, Jessica recognized that she had reduced Pastor John to a singular Christian identity, failing to recognize the complexities of his character. As an activist of interreligious harmony, perhaps speaking ill of other religious groups did not align with his professional identity. It was only the obvious misalignment between expectations and reality that forced Jessica to think about the fluidity and complexity of positionality in practice.

Reflection should occur not only at points of perceived disjuncture from expectations, or “revelatory moments” (Trigger et al. 2012). Being actively reflexive requires interrogating assumptions in moments when expectations and practice appear to align. It is, of course, impossible to navigate any situation without preconceived expectations. Recognizing this, active reflexivity demands continual reflection. Many of Jessica’s subsequent reflections on her assumptions about positionality were prompted through conversations with Lee Ann, months after the interviews took place. These discussions not only highlighted the need to interrogate assumptions about positionality, but to do so throughout the research enterprise. Interrogating and learning from the mundane requires continued and conscious effort.

Beyond humility and a dedication to continual reflection, we suggest three strategies for being actively reflexive. First, Lee Ann taught us to interrogate and record our assumptions about positionality and how and why we suspect it will play out at the research design stage. She pushed us to think about how our “working relationships” with interlocutors would unfold and why (Fujii 2017). One’s assumptions will almost certainly be challenged during the course of fieldwork and during the data interpretation stage. However, being explicit with them provides a benchmark for reflexivity. This will aid in the ability to perceive both incongruences *and* congruencies between expectations and outcomes.

Second, and relatedly, we suggest incorporating a pre-interview protocol that explicitly outlines expectations going into the interview and a post-interview protocol that outlines whether or not the interview unfolded in accordance with expectations. Even if the interview unfolded as anticipated, it is useful to consider the reasons why assumptions and expectations were met. Such a protocol compels reflection on the process of generating data and assists with reflection at later stages of interpretation.

A third strategy in the pursuit of an actively reflexive posture is to rely on others. Research assistants, in particular, offer one potential avenue for further reflexivity. As Lee Ann notes, they can “help make sense of what people said during interviews” from their own unique social location (2017, 29). Similarly, asking interlocutors themselves, at a later date, may provide valuable insights. Talking through one’s assumptions and reflections is a means of interrogating those assumptions and the conclusions we draw across all stages of research. These

¹ The name has been changed to protect the anonymity of the participant.

three strategies are not exhaustive, but they may assist researchers in adopting a posture of active reflexivity.

Conclusion

Lee Ann's research and mentorship teaches us to be attentive to positionality in our research and beyond. These lessons are not limited to interpretive research projects alone. All researchers must recognize the

intersectional and context-specific nature of positionality and *actively* confront their assumptions and expectations of its dynamics and effect. Being attuned to dynamics of power by engaging in reflexivity throughout the research process enables us to accord the dignity and respect that all research participants are entitled to (Fujii 2017, 1).

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Editing LAF: Educate, Don't Defend!

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I first encountered Lee Ann Fujii at a 2006 APSA panel on ethnographic research. Seated on the dais and listening to Timothy Pachirat present his slaughterhouse fieldwork, I noticed a woman opposite me in the front row, doubled over so she couldn't see him, shaking her head back and forth at the gory details. That was Lee Ann. I found her reaction highly ironic in retrospect, hearing about her own fieldwork with human perpetrators and victims of violence. Introduced at the panel's end, we adjourned to the market hall across the street, with Peri Schwartz-Shea. It was the first of many conversations with the person I, like others, came to refer to as LAF, her email signature. It was also the beginning of an intellectual engagement that entwined the three of us for over a decade, largely, though not exclusively, around methodological and ethical issues.¹ Over time, LAF's and my encounters broadened beyond academic activities. We shopped together; hosted each other in DC and Amsterdam; commiserated when our cats died; and planned a trip to Sarajevo, where she wanted to show me the synagogue and other favorite places.

In engaging reviewers of her early, fieldwork-centered, interpretive work, now broadly admired in political science, LAF often struggled with how to respond. That was the focus of our initial interactions. Like many of us, LAF initially wrote up her work as if its research rationale were self-evidently logical and discernible to any reader. In those years, at least one reviewer per round challenged that premise, questioning the rigor, the validity, the generalizability, the sample size, etc., of her carefully crafted, empirically-supported analyses and theorizing. LAF allowed Peri and me to use an example from a typical exchange for teaching purposes in the 2008 Short Course "Writing (Up) Interpretive Research." After lengthy consideration, I believe that her permission given then would extend to similar purposes here.

Interpretive methods were still not widely used—or understood—in US political science at that time. Misreading the manuscript's interview-based field research as an ethnography, the editor called on LAF to develop the causal argument, specify the dependent variable, link

the causal logic of the theoretical argument to the data, and clarify the research design. One reviewer asked for discussion of the potential selection bias arising from the sampling procedure. The other said that the interviews, however fascinating, valuable, and hard to collect, did not provide adequate, let alone systematic, evidence based on a research design and sound theorization. Faced with such assessments, a researcher—especially one just starting out—might decide to “tuck tail,” bite the bullet, and conform her non-realist/non-objectivist research to methodological expectations that do not fit what she had done. From our 2007 emails (redactions done for the Short Course):

Sent: Tue 9/18/2007 17:57

To: Dvora Yanow; Peregrine Schwartz-Shea
[hereafter DY/PSS]

...I know I should be excited about a very tentative R&R. ...But the comments about “haphazard” data and lack of systematic evidence are where I run into problems b/c I did not do a survey of a large N random sample.... And people don't like building theoretical arguments on what they are calling “ethnographic” data. I doubt that the reviewer is going to say, “oh, well, she interviewed 12 of that group” when to them 12 of anything cannot be the basis for any theoretical argument, particularly about something as complex as [event].

To understand the comments better, I would ask to read the manuscript. And then the conversation would begin:

From: DY

Date: Tuesday, September 18, 2007 2:26 pm

Cc: PSS

Hey, congratulations!!!

...Have you actually presented this as variables-based research??? If so, then ignore what I'm about to write. If not, you might consider pushing back a bit.

1 Among them, several APSA Short Courses, including the 2007 “Interpret This!” that she co-organized; the 2009 NSF Workshop on Interpretive Methods, where she joined the teaching staff; a 2014 workshop on institutional review boards; her 2018 interviewing book in the Routledge Series that Peri and I co-edit; and 12 years of Methods Cafés at the Western and APSA. Lee Ann also was active in the Interpretive Methodologies and Methods Conference Group: Executive Committee member since 2013; 2016 and 2017 Program Chair; and Chair of the Executive Committee from September 2017.

a. Variables: [A then-Ph.D. student doing ethnographic research] got this question on his first conference appearance...: “It’s not clear to me what your dependent and independent variables are.”...

b. “Selection biases from sampling procedure”: Did you really do sampling? If not, you might engage this as well—perhaps in terms of snowball or other exposure... [citations].

...[I]f you’re not arguing that it’s a classic sample, you don’t have selection in the classic sense, which means you don’t have selection biases—but you do have something else entirely that you can discuss on its own terms. That is, purposive selection...; ...you can discuss the purposes, the rationale for the selection, and...engage silent voices—and perhaps even reflect on silenced voices, by others and perhaps even by you.

...[I]f you want to revise [o]n the reviewers’ terms, in order to get the work out, do so. But if you are troubled by their framing of the issues, try doing it your way, with full explanation in your letter [to the editor]. The more transparent you can be in the article about not just what you did, but why you did what you did...—perhaps think about leading the reviewer/s by the hand so they can try to get inside your head....

My thinking at the time was to encourage LAF to revise in keeping with her own presuppositions, rather than try to defend her choices on the methodological grounds more familiar to the reviewers. She replied:

Sent: Tue 9/18/2007 20:52
To: DY
Cc: PSS

...I don’t present my work as variables-based at all b/c it’s not. I do talk a bit about my methodology, to wit:

The data come from intensive interviews conducted in.... I chose the two sites [reason].... I used a purposive sampling strategy [reason].... Across these different perspectives, I also tried to ensure a balance of men and women and a wide range of ages. The goal of the sampling strategy was....

Perhaps I need to get away from the language of sampling altogether.... It also makes me realize that people don’t know what purposive sampling is and what it’s for. Perhaps I need to say that I based the research design on [source] (which is true) and tried to get people who represented (oh, that’s a loaded term) a wide

spectrum of [characteristics]. ...I realize now that I do have to do a whole exegesis about how my methods *do* constitute rigor and systematicity....

...I like your advice...about doing it my way. After all, I can’t change how I collected the data, which they all seem to like for its richness—but it’s funny that richness never substitutes for what [is] deem[ed]...‘real’ or ‘rigorous’ evidence/data. It leaves me defending not just my argument and analysis but my research design....

And it was “interesting,” she added, that journals known as outlets for “alternative approaches” would still judge interpretive research by non-interpretive standards.

But how to engage someone who does not speak your methods/methodological language? Peri had tackled this in opening her chapter on “standards” (Schwartz-Shea 2014/2006). What I suggested to LAF was a strategy based on assuming the reviewer and she didn’t share the same frame of reference, so she needed to educate more than to defend:

From: DY
Date: Tuesday, September 18, 2007 3:13 pm
Cc: PSS

...[W]e still need to be...‘bicultural’...—to speak both [our] own ‘home’ language and that of the dominant group.

...What you could do is elaborate on the ‘range’—why age, etc.? why not other demographic characteristics? i.e., why are some important, in this context, whereas others less so? what might you have missed through such focus?

...I’d love to see you take this on on its/your own terms, rather than try to defend your choices on quantitative grounds/terms. The heart of the matter is...—you are making ‘truth claims,’ and a reader wants to know (be able to evaluate) if those claims are trustworthy. Can you think like this reader and extract from your brain what your thinking was on this?

...I would say, try to educate, rather than defend (subtle shift). DO say you based on [citation], if that’s the case—her work is recognized, and part of the politics of science is signaling that you are building/standing/relying on others’ work that has long been accepted as scientific....

...What's going on now, I think—why you have so many R&Rs—is that, having learned how to write a book (through the dissertation), you're now learning how to write article-length mss., which are their own 'art form....'

“Educate rather than defend”—that resonated with her.

Sent: Tue 9/18/2007 21:30

To: DY

Cc: PSS

...I soooo agree with what you say here and it gives me hope (strangely enough)—not to cede ground, to educate rather than defend.... I...didn't realize I could also couch things in my response to the editor and not try to jam everything into the “revised” version....

...I think I included age and gender just b/c of my own...training that samples need to be in some way “representative” of the population....

I also didn't realize that articles are a separate art form—soo true....

A couple years later, Peri and I asked Lee Ann to write a book on interviewing for the Routledge Series on Interpretive Methods. The manuscript went through several revisions. As editors, we found the voice of the first two versions different from the one we knew, and loved, in the papers we had helped her revise. The third revision nailed it. A key moment in that shift partially captures our mutual learning throughout these processes:

I'm struck by how much I am learning about my own take on interviewing by reading your comments. ...[W]e often do not realize how much our assumptions permeate our [research] designs. ...So learning through doing is precisely what allows us to see the extent of those prior assumptions and the extent that they may have been wrong-headed. Indeed, unmasking prior assumptions that turn out to be “wrong” is not a by-product of relational interviewing—it is central to the process! (LAF email, March 2015)

The manuscript reveals another, very special trait. Lee Ann started her career with a full complement of realist-objectivist ideas, theories, methods standards/

criteria. As she embraced a more interpretivist position—sparked largely, it occurs to me, by the mismatch between her field experiences and those ideas—she retained the “bicultural” ability to speak to both sides of a politicized methodological divide. Having learned to respond to reviewers' challenges, she could translate between the two ways of thinking. Such translating was, after all, the font of her “lies” and “accidental ethnographer” papers, at least.

In one particular arena, however, LAF would not be edited: “race.” Its manifestations in The Netherlands became part of our conversations, especially each November–December when black-faced characters appeared in Dutch shop windows, which she knew from Belgium (think: Rwanda!).² In August 2017 she e-introduced me to a colleague of hers in Amsterdam: “...I was telling him about your research on race in NL but I couldn't do it justice. ...[Y]ou both have similar critiques about the whole Black Pete bullshit....”

But I think she felt her adopted “educate, don't defend” was inadequate for dealing with “race” in political science practices. In September, she wrote about her “other big project, which I'm now calling ‘White Supremacy in the Discipline.’ I'm no longer going to use the more socially palatable term ‘diversity’....” She had already sounded these themes in print (Fujii 2007, 540). In November she emailed Ido Oren, Peri, and me:

I want to organize 2 theme-related roundtables [at APSA]...on white supremacy in the discipline. I am wedded to that language bc I am sick of framing the problem as “how do we recruit more PoCs” (which always gets bogged down in racist discussions about qualifications...). I see the real problem as...a system of privilege that actively sustains the status quo and benefits white scholars/faculty, even if they deny it.

She attached this draft abstract:

Why is Political Science so white? It has been six years since APSA released its “Report of the Task Force in the 21st Century,” which found increases in the number of women and scholars of color since 1980 “has been at a very slow pace, especially among faculty of color.” A more recent *New York Times* article showed that trends in enrollment in top schools is similarly bleak.... There can be no more pressing time to discuss this troubling state of affairs than the present political

2 December-time Christian gift-giving takes place on the 5th, St. Nicholas Day. The white-bearded Sinterklaas arrives three weeks beforehand, accompanied by “Black Pete's”—the Saint's “helpers,” dreadlocked, thick-lipped, and in blackface, wearing Renaissance-era “Moorish” costumes and the slave's gold hoop earring.

moment, when...white nationalists have been rallying to protect white privilege. Rather than view white supremacy and the pervasive problem of whiteness in the discipline as separate issues, these roundtables explore the extent to which the two are intertwined. Scholars from various subfields will focus not on the problem of how to recruit more “qualified” candidates of color, but rather on the problem of white privilege and white supremacy that works to maintain the status quo in the discipline.

The roundtables, “Democratic Implications of a Mostly White Discipline”—I: Recruitment and Hiring,

II: Teaching and Research, took place on August 30, 2018. Sadly, she wasn’t there in body, though her spirit suffused the room.

I closed one of those 2007 emails with this: “Keep in mind: some yrs from now, insh’allah, you won’t have to be making these basic arguments, because your name will carry its own weight; and others will rely on your stuff to build their arguments.” What more she might have accomplished.... I miss her voice; and I miss her.

Acknowledgements

Heartfelt thanks to Fred Schaffer for critical readings, Peri Schwartz-Shea for emergency editing help, and Sarah Parkinson for asking me to write something and for patient, masterful editing.

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Crossings: A Tribute to My Best Academic Friend

Michelle Moyd

Indiana University—Bloomington

On February 28th, Lee Ann Fujii texted me happy birthday wishes, which arrived as I prepared to deliver a keynote lecture at the University of Hamburg the following evening. She knew from previous conversations that I was anxious about the talk. She reminded me of my expertise, reassuring me that I would be fine. But she also did not let me off the hook. If I felt underprepared, she gently admonished, then I needed to have a chat with myself about why, and then strive to not let it happen again.

Lee Ann's advice and support in that moment typified our long friendship. She listened carefully, sifted out the key thoughts in what I told her, and returned them to me with a new sheen. Sometimes they sparkled, reflecting unqualified enthusiasm and excitement about my accomplishments, personal or professional. Other times—perhaps most times—they had something more like the patina of coins long in circulation. Familiar, valuable, useful, but not shiny. She gave me the tough criticisms I needed to hear to sharpen my skills, to be a person who took up the space necessary to do original research, to teach passionately, to be a good friend, to advocate for myself and others. Our friendship encompassed a set of characteristics I have found nowhere else. Although she was my confidante, she was also my peer mentor; my first-round reviewer; my intellectual partner in interdisciplinary thought and practice about political violence and its perpetrators; my career coach; my reality checker; my friend.

Academic Friendships: An Appreciation

Many friendships exemplify these qualities. Checking in with each other, caring about each other's important moments, catching up on life—these are all elements of close friendships. My friendship with Lee Ann encompassed much more, because our research interests were so similar, because we were both women of color, and because we occupied similar spaces in academic hierarchies.

We had talked on the phone the day before her text. This call had been distinctly different from the many others we'd had over the years. In that moment, Lee Ann was experiencing profound shock and grief. Her mother had passed away just hours before we connected

on the phone. With both of us sitting in hotel rooms separated by nine time zones, she expressed her sadness, her relief that she'd gotten to see her mother before her passing, her gratitude to her brothers in attending to their mother's needs in her last few years of declining health. I let her steer the conversation, offering paltry words of condolence, wishing I could do more. I listened.

Our friendship, built on mutual trust and admiration, operated on multiple levels. Even in the midst of her grief, in the shock of her mother's passing, Lee Ann insisted on hearing about my upcoming keynote. She helped me process my insecurities, showing excitement for my work as she always had. Having heard each other talk about our academic challenges, frustrations, and triumphs, Lee Ann had the right words to encourage me to *be* the expert in the room.

I fully expected that we would pick up where we left off in our next conversation. But this was our last. Lee Ann died suddenly just two days later.

Lee Ann's death left a void. How could this woman—so vibrant, so health-conscious and careful, with still so much work to do—be gone so suddenly? We first met in 2003 at an orientation conference for Fulbright Fellowship winners. I was on my way to Tanzania for dissertation research, and she was headed to Rwanda for hers. She introduced herself to me at an opening session, and we immediately became friends. We shared an irreverent sense of humor, and our interpersonal qualities complemented each other—she was outgoing and effervescent, I am reserved and hesitant to initiate conversations with strangers. Our friendship was multilayered and rich. She was the first person I called when I faced a professional or personal challenge. Our friendship, which had weathered so much time and distance, is irreplaceable.

We never wrote anything together, but we should have. Our shared research interests connected us across disciplinary differences. We both studied the incomprehensible—how people took active steps to participation in genocide and mass killing, the social histories of perpetrators, and the performative and gendered aspects of what she termed extra-lethal violence. I mined archival sources for evidence to access a more distant past. Lee Ann's research drew primarily

on oral interviews. But despite our different approaches, we were both fascinated with the relationship between violence and power in different contexts and registers, past and present. Neither of us saw disciplinary boundaries as obstacles. Rather, we valued different forms of knowledge production for the distinctive qualities they could bring to the study of violence in different times and places. We talked about collaborating all the time, mostly in jest. But we both knew that our methodological and interpretive perspectives would be sharpened by simply learning from each other—how to read archival sources against and with the grain, how to hear an interviewee’s points of emphasis and hesitation, and how to convey these complexities in clear prose. I wish we had written something together.

Disciplined Crossings

Lee Ann often joked with me about being a “wannabe” historian. In fact, she *was* one. Her work was infused with historical texture that complemented her formidable skills as a social scientist. At the time of her death, she was on the cusp of completing a complex new book that sought to merge the disciplinary sensibilities of political science, anthropology, and history to reveal the common threads between occurrences of political violence across time and place. She chose three distinct sites—Rwanda, Bosnia, and 1930s Eastern Shore, Maryland—stretching well beyond the scope of her path-breaking and influential first book, *Killing Neighbors: Webs of Violence in Rwanda* (2009).

Her credentials as a historian, and her appreciation of historical thinking as a fruitful method, manifested most clearly in her work on Maryland’s Eastern Shore. She conducted original historical research using archives, old newspapers, and oral testimonies to explore several early-1930s lynchings in the region, not simply as part of US history, but through the wider lens of political violence and its performativity in different contexts across time and space. To guide her historical journey, she sought out a local historian as a consultant, incorporating her into the project, and into her network. By studying a US-based case alongside Bosnia and Rwanda, Lee Ann’s comparative work took aim at American exceptionalism, refusing to let us point to other places as examples of racialized terror and violence. It was—it is—right here in our own backyard.

Lee Ann was tireless in her dedication to understanding how such horrors could occur, and committed to expanding her discipline’s tools for investigating and interpreting them. She placed her

Rwanda research within a global context to help her better discern how patterns of violence shared certain characteristics, while also diverging from each other. She viewed historical thinking as essential to building this context, and to identifying these patterns. She had spent the better part of a decade conducting interviews, visiting archives, and analyzing secondary literatures well outside political science. She did so in order to understand these seemingly disparate cases on their own terms, as well as within larger frameworks.

Lee Ann’s background in theater mixed with her political and historical approaches, sensitizing her to how people spoke, what they said, what they didn’t or couldn’t say with words. Lee Ann’s interpretive skills consequently made the most of different kinds of sources. She recognized not only the words spoken or written, but the gestures accompanying the words, the silences between—or in place of—the words. Her analyses of how perpetrators of political violence come to commit such acts always foregrounded the situational, the contextual, and the capacity for people to be many things at once. She sought out historians as interlocutors, and she questioned the boundaries of her discipline. She did so in order to emphasize what was human about the exercise of political power through violence, whether face-to-face or from a distance.

Through her many research trips, her method remained rooted in a firm ethical position: she would not violate her informants’ trust, she would not prod them to speak beyond their limits, and she would give as much interpretive weight to the silences in the exchanges as she did to the words she recorded. She studied the worst manifestations of human violence, but she always wrote about them with deep care and sensitivity. She felt so strongly about the ethics of fieldwork methodologies that she wrote a book on interviewing, offering a relational approach for others to emulate as they embarked on the daunting task of oral research. The fact that this slim volume has already become a staple for scholars undertaking oral research across the disciplines speaks to its practical message—an interview should be a conversation, not an interrogation. It belongs to both the interviewer and the interviewee. It must rest on an ethical foundation. Her uncompromising stance on this is one of her lasting gifts to the profession.

The Magic of Empathy

Upon learning of her death, jarred into recognition of all I had lost, I turned to Facebook to express my grief and dismay:

“I am heartbroken. It is incomprehensible to me that my dear friend Lee Ann Fujii is not here anymore. I will miss her so very much. She had so many plans. And because of her, I know that I must carry on with mine.”

And this is what she would wish for me. That I keep researching and writing, that I listen to my sources with care and discernment, that I be bold in the conclusions I draw after doing so. And that I bring those same skills to my teaching and everyday interactions with friends, family, and colleagues. Lee Ann was a fearless scholar, a brilliant mentor, and the best kind of friend one could want—supportive, reliable, and honest.

I gave an early draft of this piece to a friend, an anthropologist with five decades of fieldwork experience.

She recognized immediately, without knowing Lee Ann or her work, all that we have lost with her passing. She noted that Lee Ann’s singularity as a scholar, mentor, and friend, was rooted in a finely tuned sense of empathy. This informed her methods, her mentorship, and her scholarly writing. Of all that I learned from Lee Ann in our nearly fifteen-year friendship, this was the abiding lesson she taught me: without empathy, we will miss untold insights, opportunities to be intellectually fearless, and chances to enrich ourselves through friendships. I am a better historian for having learned these lessons from Lee Ann Fujii. I will always be grateful for having had the privilege of knowing her as a friend and colleague.

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Remembering Lee Ann Fujii's Creativity and Courage

Peregrine Schwartz-Shea
University of Utah

I first met Lee Ann in 2006 when she was, or was about to become, a newly minted Ph.D. I did not initially realize she was so early in her career because Lee Ann was comfortable in her skin, a peer who engaged with my co-author Dvora Yanow and me in ways that made us remember her. (As I recall, Dvora and I had lunch with her in the food court at Reading Terminal Market in Philadelphia.) Later I learned that Lee Ann was not your typical graduate student, having had a previous career in theater. Given that background, at APSA 2007, Lee Ann led the first Interpretation and Method (IMM) Conference Group workshop—Interpret This!—in an improv exercise, “Yes, *and*.” Participants were arrayed in a line and we had to pass a phrase through the line, building on it in this positive way—rather than a reactively-defensive, “Yes, *but*.” The point of the exercise was to learn affirmative engagement and flexibility for field research interactions. There was much laughter, as with Lee Ann leading, a workshop never felt stodgy!

Lee Ann understood context as crucial evidence for social science research. This emphasis was partially due to her professional trajectory. Lee Ann's career path can be contrasted to my own entry into academia. I progressed straight through from high school to college to graduate school, a pattern I now think appropriate to only some disciplines, say chemistry or perhaps art. On reflection, I think this trajectory can be disadvantageous to social scientists who should be bringing life experiences to our research agendas as a way of building strong, on-the-ground connections to the worlds we seek to understand. From that perspective, it now seems obvious that Lee Ann's theatre background was the foundation of her incredible creativity. That creativity was evident in the ways in which she conceptualized her research projects. It also formed the scaffolding for her work in the field: both literally in her interviewing and, also, in her deeply methodological understandings of what goes on “out there.” Lee Ann repeatedly demonstrated that research is about so much more than formal data generation; it includes, as well, the many contextual clues we pick up by being in the field (meta data, accidental data). One of her legacies is that Lee Ann taught us to *recognize* those

contextual clues as evidence that can be valuable for our research projects in their own right.

2010. “Shades of Truth and Lies: Interpreting Testimonies of War and Violence.” *Journal of Peace Research* 47 (2): 231–41. <https://doi.org/10.1177/0022343309353097>.
2015. “Five Stories of Accidental Ethnography: Turning Unplanned Moments in the Field into Data.” *Qualitative Research* 15 (4): 525–39. <https://doi.org/10.1177/1468794114548945>.
2018. *Interviewing for Social Science Research: A Relational Approach*. New York: Routledge.

My memory of Lee Ann leading improv, especially the group's laughter and learning, contrasts starkly with Lee Ann's research focus. She conducted fieldwork in challenging locations tackling topics that some prefer not to think about—genocide, lynching. She pursued these topics with persistence and passion. As she recounted in her interviewing book, she also made mistakes along the way. Indeed, her willingness to admit her mistakes *in print* speaks to her courage; it is one of her qualities that made her a model for graduate students in particular but, also, more senior scholars.

Lee Ann took that human flaw and turned it upside down by declaring, “mistakes are gifts.” Such a reconceptualization is a strong rejoinder to the contemporary political science preoccupation with *a priori* research-design perfection that implies researchers can work it all out ahead of time. Lee Ann knew from her field work experiences that mistakes will be made. Rather than ignoring, concealing, or denying them, she emphasized the research journey and the ways we could use mistakes to reflect on ourselves and our work. Lee Ann reminded us that if we can see our mistakes in a new light, we will gain the courage to learn and to persist in our work. Rather than “Yes, *but*” as an excuse, responding to mistakes with “Yes, *and*” can even open avenues for the new theorizing as her work impressively demonstrated.

2009. *Killing Neighbors: Webs of Violence in Rwanda*. Ithaca: Cornell University Press.

Forthcoming. *Show Time: The Logic and Power of Violent Display*. Ithaca: Cornell University Press.

I mourn Lee Ann's premature loss because she worked so hard to make academia a better place. That sounds like a cliché, but I mean it in a very specific way; Lee Ann was not a Pollyanna, but someone who was clear-eyed in the ways that she challenged the status quo. She wanted political science scholarship to matter and, in her last years, as the discipline obsessed about replicability, she demanded that we see the world around us, not only by studying the victims and perpetrators of violence, but also by recognizing the ossified hierarchies of race and gender in the discipline that have stifled our capacities to *see* the social world. Dismantling these hierarchies has become an ever more urgent task—one that I can hear Lee Ann pushing us all to continue in her name and memory.

2016. "The Dark Side of DA-RT." *Comparative Politics Newsletter* 26 (1): 25–27.

2018. APSA Roundtable I, Democratic Implications of a Mostly White Discipline I: Recruitment and Hiring; organized before her death.

2018. APSA Roundtable II, Democratic Implications of a Mostly White Discipline II: Teaching and Research; organized before her death

Despite the topics that Lee Ann studied, and her frustrations with academia, she never lost her sense of humor and her smile. Along with her scholarship and activism, Lee Ann's laugh and her smile is what I will miss most.

In Memoriam:

Lee Ann Fujii

In order to honor the memory of Lee Ann Fujii, a respected scholar of comparative politics and qualitative methods, friends and colleagues of Lee Ann's are raising funds in order to rename the American Political Science Association's Minority Fellows Program annual travel grants program in her honor. After discussing Lee Ann's incredible work as a mentor and her commitment to diversifying the discipline of political science, her colleagues decided that taking steps to foster the careers of junior scholars from diverse backgrounds would be one of the best means by which to honor Lee Ann's legacy.

The MFP travel award program is one of the association's most important means for fostering early career opportunities for scholars from diverse backgrounds. The MFP supports individuals from underrepresented groups applying to doctoral programs in political science or in the early stages of their doctoral education. Currently, the travel grant program provides small grants to approximately 15 MFP Fellows to help them attend the APSA Annual Meeting—a key professionalization activity. The goal is to raise \$35,000 in order to greatly expand the program's granting capacity for five years and rename it in Lee Ann's honor for the duration of that period.

For those interested in donating to the effort, donations can be made at the following website under the entry for Lee Ann Fujii Minority Fellow Travel Grants: <https://www.apsanet.org/Donate-Now>.

Detailed information about the program can be found here: <https://www.apsanet.org/mfp/travelgrant>.

Anyone interested in supporting this effort can contact the APSA development team at 202-483-9357 or development@apsanet.org. Gifts of any amount in the form of cash, appreciated stock, or direct transfers from retirement accounts are accepted from both APSA members and non-members alike.

Articles

Qualitative and Multi-Method Research

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<https://doi.org/10.5281/zenodo.2563131>

Using Digital Object Identifiers in Qualitative and Multi-Method Research and Beyond

Sebastian Karcher

Qualitative Data Repository

Syracuse University

Citation is indispensable to social science. A citation points us to a location. As the places where we keep publications have changed, so should the way that we cite them. In years past, the primary way scholars consumed articles was in bound volumes organized chronologically on shelves. We found them (and helped others find them) by referencing the journal title, year, volume, issue, page numbers, author, and article title. Increasingly, even when a particular journal is also available in hard copy, scholars access articles as pdf or html files. This shift to online location has necessitated a change in how publications are made available to others, and how they are cited. We typically still include the old elements, but we add a crucial innovation: a persistent identifier. For articles, the persistent identifier used is a digital object identifier or DOI. DOIs have become crucial components of academic referencing and virtually all articles published in scholarly journals now have a DOI.

Qualitative and Multi-Method Research (QMMR) has been in production since 2003. During that time, it has regularly published substantive essays on a wide range of topics and questions, many of which have been well used across the section’s diverse research traditions.¹ With this issue, *QMMR* has shifted to an all-digital format, and every article now includes a DOI, a string of characters starting with 10.5281.

With DOIs, we ensure that articles published in *QMMR* remain *accessible* in the long run, we make the

articles more *findable*, and their authors more likely to receive *credit* when their articles are viewed, downloaded, and cited. This short memo seeks to answer some questions about digital object identifiers and their implementation in *QMMR*.

What are DOIs good for?

The primary function of a DOI is to provide a stable link to a resource. Imagine you are a publisher of digital material; you know that many people use URLs to link to these materials. Now, suppose you need to change the URL for technical reasons (e.g., you rename your company and your homepage moves to a different domain). How will you let everyone know the new URL and make sure they change their links? This is a virtually impossible task and the reason that link rot—links that are no longer working—is such a problem, including in political science journals (Gertler and Bullock 2017).

DOIs are a *permanent* identifier for resources (i.e. the DOI of an article, as opposed to its URL, never changes) and work together with a set of “clearinghouses” for links, referred to as DOI registration agencies. Instead of linking directly to the publisher, you link to the DOI, e.g. <https://doi.org/10.5281/zenodo.938958>, which then resolves to the correct URL (<https://zenodo.org/record/938958> in this case). If the URL of the article changes, the publisher only needs to notify the registration agency, which redirects all DOI links to the new URL.

¹ Google scholar, e.g., finds 20 formal citations to Ahmed and Sil’s (2009) piece on multi-method research and 18 for Dunning’s (2008) article on natural and field experiments. These numbers almost certainly understate the actual number of citations since *QMMR* (in part due to the lack of DOIs) is not well captured by Google Scholar and other citation indexes.

This central clearinghouse offers another, less well-known benefit of DOIs: together with URLs, registration agencies also store citation metadata (author, title, data, etc.). Having this available in a central location allows you to quickly import this information into databases (such as reference managers) or even to automatically generate citations in a wide range of styles on the spot: <https://crosscite.org/>.

Finally, DOIs make it easier to recognize when a work is being referenced (whether it is as a formal citation in an academic publication or as a link in a tweet or blogpost). Ongoing efforts² are therefore capitalizing on DOIs to provide citation metrics as well as alternative metrics or “altmetrics” like social media mentions (see Piwowar 2013). References with DOIs, therefore, help their authors receive credit for their work.

Where are QMMR articles archived?

QMMR articles are archived at ZENODO (<https://zenodo.org>), an EU funded, non-profit repository for scholarly work run out of the CERN particle accelerator.

Who creates the DOIs? Why do they say “zenodo”?

The DOIs are created by our archiving partner, Zenodo, based on metadata provided by QMMR. They are registered with the Datacite (<https://datacite.org>) registration agency.

Should I include DOIs in citations? If so, how?

Yes, you should absolutely include DOIs in citation. In fact, most major citation styles such as the *APA Style Manual* and the *Chicago Manual of Style* require or strongly encourage their use. Always use the DOI in its link form, i.e. <https://doi.org/<DOI>>. That way, readers will be able to quickly and reliably get to the resources you cite.

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² See e.g. <https://www.crossref.org/services/event-data/>