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Inside:

- Letter from the Editor – Jennifer Cyr

Symposium: Comparative Areas Studies

- Contributors: Ryan Saylor, Amel Ahmed, Roselyn Hsueh, Nora Fisher-Onar, Marissa Brookes, Thomas Pepinsky

Symposium: When Locals Say You're Wrong: Member-Checking and Political Science

- Contributors: Allison Quatrini, Dvora Yanow, Peregrine Schwartz-Shea, Alyssa Maraj Grahame, Allison Quatrini, Nicholas Rush Smith

Original Article: Innovative Data Collection and Integration to Investigate Sorcery Accusation Related Violence in Papua New Guinea

Authors: Ibolya Losoncz, Miranda Forsyth, Judy Putt

In Memoriam: A Tribute to Kendra Koivu

- Contributors: Jennifer Cyr, Marissa Brookes, Anna Calasanti, Erin Kimball Damman, Christopher Day, James Mahoney, Jami Nelson-Nuñez, Sara Niedzwiecki, Fiorella Vera-Adrianzén

Longform APSA Awards (2019)

Table of Contents

Letter from the Editor

Jennifer Cyr - DOI: 10.5281/zenodo.3946782iii

Symposium: Comparative Area Studies**Comparative Area Studies: A Route to New Insights**

Ryan Saylor - DOI: 10.5281/zenodo.3946785 1

The Utility of Comparative Area Studies for Historical Analysis

Amel Ahmed - DOI: 10.5281/zenodo.3946792 7

Synergies of CAS: New Inquiries, Theory Development, and Community

Roselyn Hsueh - DOI: 10.5281/zenodo.3946799 10

**Making Sense of Multipolarity: Eurasia's Former Empires, Family Resemblances,
and Comparative Area Studies**

Nora Fisher-Onar - DOI: 10.5281/zenodo.3946803 15

**The Sweet Spot in Comparative Area Studies: Embracing Casual Complexity through the
Identification of Both Systematic and Unsystematic Variables and Mechanisms**

Marissa Brookes - DOI: 10.5281/zenodo.3946805 20

What's the "Area" in Comparative Area Studies?

Thomas Pepinsky - DOI: 10.5281/zenodo.3946807 22

**Symposium: When Locals Say You're Wrong:
Member-Checking and Political Science****Introduction**

Allison Quatrini - DOI: 10.5281/zenodo.3946811 27

My Participants Told Me I got it Wrong: Now What?

Dvora Yanow - DOI: 10.5281/zenodo.3946813 29

Member-Checking: Not a Panacea, Sometimes a Quagmire

Peregrine Schwartz-Shea - DOI: 10.5281/zenodo.3946815 39

"You're Asking the Wrong Question": Member-Checking during Fieldwork

Alyssa Maraj Grahame - DOI: 10.5281/zenodo.3946822 46

If My Participants say "You're Wrong," Does it Mean I really Am?

Allison Quatrini - DOI: 10.5281/zenodo.3946824 54

Member Checking: Lessons from the Dead

Nicholas Rush Smith - DOI: 10.5281/zenodo.3946827 60

Original Article: Innovative Data Collection and Integration to Investigate Sorcery Accusation Related Violence in Papua New Guinea

Ibolya Losoncz, Miranda Forsyth, Judy Putt - DOI: 10.5281/zenodo.3976055 66

In Memoriam: A Tribute to Kendra Koivu

Kendra was Everyone's Teacher

Jennifer Cyr - DOI: 10.5281/zenodo.3946830 75

Dance Lessons

Marissa Brookes - DOI: 10.5281/zenodo.3946834 76

No Causality without Correlation: On Learning from Kendra Koivu

Anna Calasanti - DOI: 10.5281/zenodo.3946840 77

On the Loss of a Dear Friend

Erin Kimball Damman - DOI: 10.5281/zenodo.3946842 78

Kendra Koivu: One of my Favorite People

Christopher Day - DOI: 10.5281/zenodo.3946845 79

Kendra Koivu: Remembering a Qualitative Methodologist

James Mahoney - DOI: 10.5281/zenodo.3946849 81

Sisu

Jami Nelson-Nuñez - DOI: 10.5281/zenodo.3946853 82

Kendra Koivu: A Brilliant Methodologist and a Dear Friend

Sara Niedzwiecki - DOI: 10.5281/zenodo.3946855 84

Kendrachaychik Ñuqanchikwan Tukuypuni (Our Dear Kendra, With Us Always)

Fiorella Vera-Adrianzén - DOI: 10.5281/zenodo.3946863 86

Longform APSA Awards (2019)

DOI: 10.5281/zenodo.3946899 88

Letter from the Editor

Qualitative and Multi-Method Research

2020, Vol. 17-18, No. 1

[https://DOI: 10.5281/zenodo.3946782](https://doi.org/10.5281/zenodo.3946782)

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The approaches we take to carrying out qualitative and mixed methods research are multiple. There is no one “right” way, at least not yet. Indeed, I am not sure there should be. The methods with which we engage—even those that have attained the status of “best practice”—deserve to be viewed critically from time to time.

Take, for example, the comparative method, or the “systematic analysis of a small number of cases” (Collier 1993, 105). Historically, this kind of small-n comparison has involved selecting from a set of cases located in the same geographical space (e.g., Latin America, Sub-Saharan Africa, or Western Europe). Nowadays, however, a non-trivial percentage of books compare a small set of cases *across* regions. Our first symposium below examines this kind of research, characterized as Comparative Area Studies (CAS). CAS differs from within-region, small-n research in several ways. For example, regional specialists typically leverage country similarities to control for context. CAS, by contrast, asks scholars to take context seriously, compelling them to think carefully about concepts, measures, and coding. The symposium considers what is novel and innovative about cross-area comparison, while also considering its implications for the comparative method in general.

What about qualitative data collection methods? These merit scrutiny and engagement as well. I rarely encounter a graduate student of my own who does not ask for guidance on how to undertake fieldwork and, in particular, interviews. Political science, as a discipline, often assumes that qualitative scholars will know how to carry out an interview when the time comes to do fieldwork. But this is not a skill we have innately. Asking the right questions is no easy task; understanding the answers is equally difficult. The second symposium deals with the latter question: How do we know if we have appropriately “heard” our interview subjects? How do we know if we’ve correctly interpreted their responses to our queries? One way to verify and validate the results of our research is to share them with our subjects directly. This practice of “member-checking” is increasingly important in the social sciences, but it is not without controversy. The second symposium below considers a range of issues associated with member-checking, considering, above all else, what happens when your research and your research subjects do not necessarily agree.

Often, the questions underlying the practice of mixed methods are less about how to carry out different kinds of methods (although this is certainly important) and more about how to bring these methods together to advance knowledge on a singular topic or question. The third contribution to this issue is an original article on how to integrate a series of methods used across a multi-site research project on Sorcery Accusation Related Violence (SARV) in Papua New Guinea. It is often the case that our research has many moving parts. This article offers an innovative approach to managing multiple types of data coming from several different places. It also provides a fascinating account of a project that delves into the shocking and sensitive topic of SARV.

Finally, our last set of essays remembers the inimitable Kendra Koivu, who passed away in early fall 2019. Kendra was a serious methodologist who had made her mark on the study of qualitative and mixed methods as a junior scholar. Our tribute to her examines the impact she had on her colleagues, her students, her friends, and the discipline as a whole. A bit of a spoiler: Her impact was great. Indeed, Kendra was well on her way to pushing the study of methods forward in her own right. See, for example, her help in coining the term, SUIN, a now common type of condition utilized in Qualitative Comparative Analysis.

It goes without saying—but I will say it anyway—that Kendra’s presence in the study of qualitative and mixed methods will be greatly missed. In this issue, QMMR celebrates her contributions and also mourns her passing.

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Symposium:

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Comparative Area Studies: A Route to New Insights

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Two trends stand out in contemporary political science. Some researchers are assembling ever-better global datasets (e.g., Coppedge et al. 2019), while others are conducting sophisticated experiments and other micro-level analyses within single countries (Pepinsky 2019). Alongside these trends, the 2018 volume *Comparative Area Studies: Methodological Rationales and Cross-Regional Applications* (Ahram, Köllner, and Sil) underscores the vitality of small- and medium-N case study research. Most notably, the volume advocates for cross-regional research. This symposium seeks to extend a burgeoning dialogue regarding the virtues, promises, and challenges associated with comparative area studies (Sellers 2019).

The symposium gathers six essays. Two, written by Amel Ahmed and me, are from contributors to the volume. Ahmed describes how comparative area studies can promote an ethnographic sensibility and enable researchers to better understand their historical subjects. I preview my essay in the next paragraph. The next two articles, written by Roselyn Hsueh and Nora Fisher-Onar, come from scholars whose research has affinities with comparative area studies. Hsueh documents a variety of examples of innovative research on China, which contrast the Chinese case in fresh and unusual ways. Fisher-Onar examines how comparative area studies might elucidate the emerging multipolarity in the world, by exploring how countries with imperial histories (China, Russia, Iran, and Turkey) are striving to expand their power. The final two essays, by Marissa Brookes and Thomas Pepinsky, critically appraise comparative area studies and suggest ways to sharpen it. Brookes thinks comparative area studies research could be strengthened if researchers better explicated their underlying logic of

causal inference, particularly by specifying if key variables constitute, for example, an “INUS” condition. Pepinsky presses practitioners to rethink what distinguishes an “area” as such and to consider whether our geographic conceptualizations should be replaced by alternative constructs.

In this first essay, I provide an overview of comparative area studies. I describe its distinctive features, identify its affinities with causal explanation, and provide a way that one can begin comparative area studies research. I first report some key characteristics of comparative area studies: a methodological imperative for cross-regional research, a practical desire to engage area specialists, and an embrace of epistemic diversity. In the second section, I describe how comparative area studies can help researchers explain outcomes in multiple cases, rather than using case studies as tests of a broad inferential pattern. Researchers can achieve causal explanation by comparing cases to an ideal type, which encapsulates general causal claims and can thereby help researchers explain why individual cases turned out as they did. This approach renders an alternative outlook on case selection that neutralizes common methodological concerns about cross-regional comparisons. The third section offers guidance to start doing comparative area studies, specifically by synthesizing the region-specific conventional wisdoms that surround one’s research question. Incidentally, for those readers who are unfamiliar with the edited volume, I want to mention that the first section is mainly a summary of comparative area studies. The second and third sections are more my personal take, and the volume’s editors or contributors do not necessarily share these views.

What Comparative Area Studies Is

There are methodological, practical, and epistemic dimensions to comparative area studies. The most obvious methodological aspect is that it is cross-regional. Such research designs are uncommon: Patrick Köllner, Rudra Sil, and Ariel Ahram (2018, 17) estimate that just 15 percent of the principally small-N comparative politics books that were reviewed in *Perspectives on Politics* between 2006 and 2013 had case studies from more than one region. So one reason that comparative area studies highlights cross-regional research is because it is relatively rare, which may diminish our awareness of its virtues.

Yet a more compelling reason to promote cross-regional research is substantive. Studying a phenomenon in different regional contexts may pose vexing challenges that yield novel insights, as one struggles to make sense of the commonalities and differences within and between world regions. In addition, cross-regional research can prompt us to reconsider conventional wisdoms that have taken hold within area studies communities, as well as among area-oriented political scientists. Later, in the third section of this article, I consider how engaging these region-specific conventional wisdoms can produce new conceptual and explanatory insights, and ultimately alter the analytic frameworks we use to understand the world around us.

A second methodological feature of comparative area studies is its requirement to pay close attention to context. This imperative is not the first plea regarding the importance of context. For example, Tullia Falleti and Julia Lynch (2009) consider how contextual factors influence the operation of causal mechanisms, and how contextual variation can induce mechanisms to behave differently and produce dissimilar outcomes. In this way, Falleti and Lynch regard context as something that exists independently of a theoretical hypothesis and its attendant causal mechanisms. By contrast, comparative area studies seeks to harness contextual nuance in a more thoroughgoing way. This process involves a “self-conscious effort to adjust the operationalization of concepts, the calibration of measures, and the coding of observations for each case in light of contextual attributes deemed significant by the relevant country or area specialists” (Sil 2018, 233). Catherine Boone’s (2003, 354-57) research on institutional frameworks in West Africa provides a region-specific illustration of how such considerations can produce rich concepts and complex measurement schemes. So although comparative area studies practitioners value general concepts and theoretical debates, sometimes including the desire

to find “portable mechanisms and causal processes” (Köllner, Sil, and Ahram 2018, 3, 14), contextual factors are not an afterthought. Instead, practitioners believe that “differences in context conditions need to be granted the same theoretical status as those recurrent mechanisms or linkages that are portable” (Sil 2018, 235).

The entreaty to take context seriously relates to one of comparative area studies’ practical imperatives. Adherents of comparative area studies strive to appreciate contextual nuance in part by engaging area specialists and their debates. Too often, political scientists remain sequestered from area studies communities. This distance may negatively affect the richness of our case studies. But beyond the potential improvement of a research product, there is a wider communal benefit that may come from engaging area specialists. In my experience, historians and area specialists have seemed genuinely interested to learn about my research topics and, through their probing, have helped reveal conceptual or other ambiguities that may not have occurred to interlocutors with my disciplinary background. Many of those reading this piece have undoubtedly had similar experiences. Thus one practical feature of comparative area studies is dialogical: a desire to make cross-disciplinary engagement commonplace (Sil 2018, 239).

Engagement with area studies communities has potential pitfalls, however. As Lustick (1996) emphasizes, secondary sources are products of how a historian or area specialist interprets the past. They use an implicit framework in their quest to identify the pertinent facts as such (cf. Trachtenberg 2009). Thus when social scientists use these materials, they are not harnessing a neutral and dispassionate record but are drawing on disputable materials. Similarly, area studies specialists often gravitate toward idiographic understandings of their research matter and may be skeptical of comparative research designs. The project of comparative area studies encourages researchers to be aware of and embrace these challenges, in order to enrich their understanding of a case’s context and the scholarly debates that surround it (Sil 2018, 235).

For example, Amel Ahmed (this issue) discusses how comparative area studies may help us understand historical actors as they understood themselves and their endeavors, rather than projecting our contemporary impressions of their predicaments onto them. Cross-regional research may assist our quest to empathize with and understand actors in seemingly disparate contexts. Yet as Thomas Pepinsky (this issue) makes plain, just what constitutes an area and how those conceptualizations

ought to frame our research are far from settled issues. An “area” may be less geographically bounded than one might think initially. In different ways, Pepinsky, Ahmed, and Nora Fisher-Onar (this issue) raise fundamental questions about how and why we identify world regions as such, and whether those constructs are the most fruitful way to organize unconventional comparisons.

A second practical imperative of comparative area studies is to examine substantively important phenomena, often with special attention to macro-level factors. My sense is that some practitioners of comparative area studies want to be the standard-bearers of macro-structural research on topics such as democratization, political order, and revolution. There is an intellectual heritage to books such as—to cite a few cross-regional examples—Barrington Moore’s *Social Origins of Dictatorship and Democracy*, Samuel Huntington’s *Political Order in Changing Societies*, and Theda Skocpol’s *States and Social Revolutions*. Indeed, Roselyn Hsueh (this issue) documents an affinity between comparative area studies and how innovative scholars have juxtaposed the Chinese case in new ways. Yet comparative area studies is not inherently disposed toward country-level, macro-structural research. For instance, Benjamin Smith’s (2018) contribution to the volume compares separatist conflicts in areas that straddle country borders: greater Kurdistan in the Middle East, the Baloch region in Southwest Asia, and the Tuareg region in North Africa. The research involves surveys and interviews, not macro-structural analysis, although the historical backdrop of the chapter is a macro-political process (post-imperial partitions). Overall, while the discipline has shifted toward case studies analyzing micro-level causal processes (Pepinsky 2019), comparative area studies helps preserve case-based research that is focused on macro-level factors and rich in historical detail.

In describing the features of comparative area studies, I think it is important to note two things that it is not. First, the demand to compare cases from multiple regions is not borne out of a desire to “increase the N” in order to see if the insights generated from the study of one region will “travel” to another. If it were, then one’s case studies would become tools that are used to test a nomothetic inference (see Köllner, Sil, and Ahram 2018, 11, 15; Sil 2018, 226–27, 232). And comparative areas studies would be epistemically indistinguishable from standard multi-method research; sure, the tools would differ (cross-regional cases studies rather than large-N analysis), but the two approaches would share the same neopositivist wellspring (Jackson 2011, 67–71).

Comparative area studies is not tethered to a particular epistemic project, because its advocates recognize “the epistemological heterogeneity of qualitative research” (Sil 2018, 227).

Instead, and second, comparative area studies embraces epistemic diversity. That means some people employing comparative area studies may very well conceive of their work in neopositivist terms, and some of the chapters in the edited volume could qualify as such. Marissa Brookes (this issue) offers methodological advice to enhance these types of comparative area studies. But the emphasis on contextual sensitivity also makes comparative area studies compatible with some forms of ethnographic research. For instance, Erica Simmons and Nicholas Rush Smith (2019) identify a variety of benefits to be had from comparative ethnography, including detecting commonalities across cases, preventing unwarranted extrapolations of findings from a single case, and sharpening theories and concepts. The spirit of comparative area studies shares much with comparative ethnography. Calvin Chen (2018) illustrates these affinities in his study of how Chinese businesspeople imported their Wenzhou model into Italy in recent years. A third approach to comparative area studies (from this non-exhaustive list) is research that focuses on explanation, rather than interpretation or inference. I describe this research avenue in greater detail in the next section. In sum, comparative area studies has epistemic, practical, and methodological features that help qualify it as a distinctive approach to social science.

Comparative Area Studies Produces Context-Sensitive Explanations

In this section, I describe how comparative area studies can be employed toward the goal of explaining cases. This section draws on my related article (Saylor, forthcoming). As I mention above, comparative area studies is not an approach that seeks to increase the N by adding case studies from one region to see if they corroborate a theory that was originally applied to cases from another region. (If we think of comparative area studies in this way, it ceases to have much distinctiveness.) When one uses case studies to see if they fit a broad cross-case pattern, the case studies serve as tests of an empirical regularity. One is trying to make a causal inference: the process of scrutinizing a theoretical premise with data (Waldner 2007, 150). The requisites for causal inference have long plagued unconventional comparisons. For example, Skocpol and Somers (1980, 191) criticize the “parallel demonstration of history”—in

which one juxtaposes cases to repeatedly show a theory's usefulness—because it does not establish controls and can therefore “only illustrate” but “not validate” a theory. Yet not all social science is oriented to making causal inferences.

Alternatively, one can fruitfully employ comparative area studies to explain cases. An explanation is distinct from an inference. An explanation describes what caused something to happen: it is a statement about how a cause manipulated something and produced its effect (Jackson 2017). One way to explain the outcomes of particular cases is to examine them in relation to an ideal type. Ideal types are deliberate oversimplifications of empirical reality. They can facilitate explanation by forcing researchers to determine, for “each individual case, the extent to which [an] ideal-construct approximates to or diverges from reality” (Weber 1949, 90). Ideal types are not hypotheses to be tested by individual case studies, but rather they are constructs that can help render particular cases intelligible (Jackson 2011, 112-15, 141-55).

Ideal types help researchers explain cases in a few ways. First, they direct our attention; ideal types are things against which the empirical facts of a case can be juxtaposed. Second, when applied to the actual facts of a case, ideal types can reveal the extent to which they account for the permutation of that case. Third, researchers can then identify the other factors that were not described by the ideal type, but which form part of the explanation of how and why a case turned out as it did. Ideal types facilitate explanations of individual cases.

This third aspect of what ideal types can do is where the affinity between explanation, ideal types, and comparative area studies becomes clearer. When one lists the factors that helped shape the outcome of a particular case, but which were not captured by the ideal type, one is adjusting for context. Indeed, Köllner, Sil, and Ahram (2018, 16) write that “what distinguishes (comparative area studies) is the idea that the context conditions across two or more regions—and of countries and locales within those regions—may encompass similarities and differences that affect the operation of more general causal processes and mechanisms.” Put differently, an ideal type may describe how some general causal process might operate in an overly simplified world, while contextual sensitivity can elucidate how and why that process played out as it did (or failed to do so) in an individual case.

Boone's (2003) research on state institutions in rural Africa displays these principles. She argues that variations in communal and class structures influenced

how rulers built state institutions in the countryside. Boone mentions that she wants to identify “a set of ‘ideal type’ variations in rural social organization” and their effects on institution building (323). When one case, the Korhogo region in Côte d'Ivoire, does not conform to her model's expectations, Boone forthrightly discusses the idiosyncratic reasons why it does not (244-45). She is adjusting for context. Her explanations persuade because they couple ideal-typical claims with contextual analysis.

Another way that ideal types can assist comparative area studies is with respect to case selection. When researchers use case studies as tests of a broader cross-case pattern, they usually justify case selection in terms of how their cases score on certain variables and whether a case is representative of a larger population of interest. Mill's method of difference, which pairs cases that are similar in many ways but differ on an explanatory variable, is the most common strategy of case selection (Koivu and Hinze 2017). Standard criteria for case selection often imperil cross-regional research. By contrast, because ideal types do not profess to represent actual empirical regularities, but rather ideal-typical causal claims, one can be freed from these case selection strictures. Instead, one can select cases that seem relatable—that is, pertinent and applicable—to an ideal type. Then, the case study itself will reveal whether the ideal type is useful for explaining the facts of the case. Basic contextual similarity can serve as an alternative basis for case selection.

Consequently, in ideal types, practitioners of comparative area studies can find a robust justification for making cross-regional comparisons, even when those comparisons contravene standard prescription on case selection. No longer would researchers succumb to the need to demonstrate “control” over a host of variables, a fundamental aspect of the conventional wisdom on case selection that inhibits comparative area studies (cf. Köllner, Sil, and Ahram 2018, 18). Not only does my approach to case selection facilitate comparative area studies, it also better aligns with the epistemic goals of those researchers who want to produce explanations.

Starting Comparative Area Studies by Appraising Region-Specific Conventional Wisdoms

This final section provides one way that scholars can begin to engage in comparative area studies. I encourage scholars to survey, compare, and synthesize the region-specific conventional wisdoms that surround their research topic. It is a first step to developing a conceptual and theoretical framework that may render intelligible

how your phenomenon of interest has unfolded in a cross-regional contrast space. I think this discussion is best presented through an applied example, so I reference my chapter in the edited volume, which draws on a larger book project (Saylor 2014).

My research analyzed how natural resource booms and different types of political coalitions affected state building in Latin America and Africa (three countries from each region: Argentina, Chile, and Colombia; Ghana, Mauritius, and Nigeria). The simplest summary of the argument and outcomes is that when commodity booms enriched social actors both within and outside of the ruling coalition (Argentina and Chile), more state building occurred than when booms enriched actors who were solely within or outside of the ruling coalition.

At an early point in the project, I surveyed the literatures on state building in each region. In Latin America, the formative state building era was during the period of “outward expansion” (ca. 1850-1900), when Latin American states were strengthening their connections to the world economy. Many studies, epitomized by dependency theory, framed scholarly thought by analyzing the extent to which export elites dictated policy and state building in a given country. Hence, state building was seen as something of a functional outgrowth of deepening economic links. By contrast, the crucial era for state building in Africa came after World War II (ca. 1945-65), when urban nationalist movements gained power. These leaders often installed policies of urban bias and elaborated “neopatrimonial” forms of rule. These respective paradigms do not comprise all accounts of state building in these regions, but in my estimation they are the archetypal themes.

At first blush these conventional wisdoms seem to have little in common. But a virtue of comparative area studies is that I was compelled to compare these conventional wisdoms to each other and to cases from each region. I juxtaposed not only the discrete arguments, but also the conceptual frames that implied how researchers ought to think about these phenomena. These comparisons were not methodologically novel—I am sure many readers have done similar things in their own work—but they are nonetheless worth highlighting.

The conventional wisdom on Latin America led me to learn that most African countries also experienced massive commodity booms during their formative state building eras. And the conventional wisdom on Africa helped me appreciate that the types of economic interests encapsulated within ruling coalitions (if any) mattered greatly. Whereas the literature on Latin America parsed differences in export elites at the helm of countries, the literature on Africa laid bare the consequences of having ruling coalitions that did not include actors with direct stakes in exporting. These region-specific conventional wisdoms helped me look at cases from another region from a different viewpoint.

I combined aspects of these conventional wisdoms together in order to relate these cases to each other, develop explanations of their individual trajectories, and pay attention to local context. The cross-regional nature of my comparisons enabled me to interpret cases that are often regarded as regional oddities (Colombia, Mauritius) as having features regularly observed in another region. By design, comparative area studies forces us to reappraise region-specific conventional wisdoms and create a dialogue between literatures. This process is not unique to comparative area studies—a researcher doing good work on one region is usually versed in the basic lessons from research on another region—but comparative area studies may impel researchers to go further than they otherwise might, and these endeavors may yield insights that are presently beyond our grasp.

Overall, the promise of comparative area studies comes not from its methodological novelty but rather from its pluralism. Comparative area studies allows researchers to embrace the fact that context does matter, and in ways that are often not reducible to the variable-oriented thinking prevalent in much contemporary political science. Yet practitioners of comparative area studies also seek to harness general theoretical insights and cutting-edge thinking on causal mechanisms. Thus comparative area studies aims to strike a delicate balance. This goal may be achieved not by conceiving of comparative area studies as a means for causal inference, but rather as something best suited to producing causal explanations.

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The Utility of Comparative Area Studies for Historical Analysis

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While focusing on particular countries or regions is indispensable for accumulating substantive knowledge, there are also costs to not stretching beyond a given geographic region when taking on “big” questions in the study of politics. The recent volume, *Comparative Area Studies: Methodological Rationales and Cross-Regional Applications* (Ahram, Köllner, and Sil 2018) identifies comparative area studies (CAS) as a distinct research strategy occupying unique intellectual spaces within the social sciences. As the contributions to the volume demonstrate, CAS has distinct advantages for developing mid-range theory, offering novel empirical findings and a different mode of triangulation. Such works can also serve an important disciplinary function by bringing into dialogue scholars that may be siloed off into various research communities. Moreover, they advance an important intellectual agenda in offering a mode of research that problematizes and denaturalizes our conceptions of geographical areas, and indeed, our understanding of what it means to compare.

In this essay I wish to develop further a dimension of the CAS framework that is acknowledged but not adequately treated within the volume: the utility of a comparative area studies sensibility for historical analysis. The basic intuition of the CAS framework, which is to question the notion of an “area” or the assemblage of cases that constitute a theoretically relevant unit of analysis, is critically important for historical research. This is because both our conventional understandings of areas and disciplinary conventions around area studies are situated in specific cultural and historical contexts that may not translate to the period under investigation. Thus looking across areas or bringing insight gleaned from one area to bear on the study of another opens important new avenues for the study of political development.

Social scientists have for some time been admonishing us to “read history forward,” emphasizing the need to take seriously actors’ subjective understandings of their situations and the context in which they are fighting their fights (Pierson 2004; Kreuzer 2010; Capoccia and Ziblatt, 2010; Ahmed 2010). An attentiveness to subjectivity has been central to efforts to revive historical

scholarship within the social sciences and much of the focus has been on time and temporalities. This has included work on sequencing, critical junctures, and also actors’ perception of the tempo of events (Mahoney and Reuschemeyer 2003; Pierson 2000; Cappocia and Keleman 2007; Grzymala-Busse 2011).

In addition to this emphasis on time, something that is critically important for historical analysis, but less thoroughly examined, is an appreciation of subjectivity with respect to actors’ understanding of the political space in which they are operating. Geography is surely one element of this, but, just as surely, geography is not determinative of what constitutes an area or region. Indeed, many scholars have questioned not only the construction of areas and regions as political entities (Holbig 2017; Fawn 2009), but also the physical geography on which these constructions are based (Wiggen and Lewis 1997; Schulten 2001).

Geographic demarcations themselves are politically informed at the same time that they inform our politics. It is for this reason, for instance, that Haiti can be imagined as part of Africa, while Turkey remains beyond the boundaries of Europe: In 2016, the African Union (AU) considered and voted on the inclusion of Haiti in the African Union. And while the bid was ultimately unsuccessful, it exemplifies the ways in which the geographical imagination need not correspond to accepted physical boundaries. It also led to several initiatives to deepen ties between the AU and the African Diaspora, defined as “the communities throughout the world that are descended from the historic movement of peoples from Africa” (quoted in Amao 2018, 50). In contrast, negotiations for Turkey to join the European Union, which began in 2005, continued for a decade and ultimately stalled out for failure to meet the political requirements for membership (Ugur 2010).

The challenge of developing a grounded conceptualization of regions is compounded in historical research because we often bring contemporary understandings of what constitutes a theoretically significant “area” to our research about historical phenomenon. We are often deceived by what Skinner

referred to as a sense of “familiarity” when reading historical texts. On this he wrote:

It is the very impression of familiarity, however, which constitutes the added barrier to understanding. The historians of our past still tend, perhaps in consequence, to be much less aware than the social anthropologists have become about the danger that an application of familiar concepts and conventions may actually be self-defeating if the project is the understanding of the past. (Skinner 1970, 136)

Overcoming this sense of familiarity requires something akin to an ethnographic sensibility (Schatz 2009; Simmons and Smith 2017). It is telling that Skinner compared the task of a historian to that of an anthropologist. Both need to develop modes of “seeing” that are different from those used to navigate familiar contexts. Building on this, Schaffer (2016) has offered the technique of “locating” concepts as a way to disrupt familiarity across both different ages and languages (55). Locating actors’ sense of political space historically is challenging for all the reasons noted above, but central to the effort is the need to problematize the familiar in terms of our understanding of areas.

The temptation to see the familiar in the past may vary depending on the place and time. For the historical context I am most familiar with, nineteenth-century Europe, this slippage is quite easy because the political geography remains more or less unchanged. So it may be possible to imagine that the idea of Germany today is what it was then, or that the physical geographical boundaries of Europe constituted the relevant political demarcations of space. These projections of the familiar onto the past would be very problematic given that German unification did not happen until 1871 and would remain contested for decades after. In addition, Europe of the nineteenth century was understood by many to extend to colonial spaces, especially with regard to the settler colonies. But there is a danger even with historical periods and places that may seem self-evidently different. As Schaffer demonstrates, Skinner himself has been guilty of this homogenizing tendency in his discussion of “originality” in the work of Milton (Schaffer 2016, 64-67).

With respect to our conceptions of space, the challenge is often a daunting one given that specific notions of geographic areas are built into our discipline. Even with the ebb and flow of area studies as separate fields, entrenched ideas about where a given politics

begins and ends are embedded in the organization of the academy. Organization such as the Latin American Studies Association, the Council for European Studies, the Middle East Studies Association and so on, provide opportunities to continually question the construction of regions, but also serve to maintain the prevalent practices of regional delineation. This is often reinforced by disciplinary conventions and training that starts very early on. A paper that comes out of a seminar on Western Europe becomes an article or a dissertation on Western Europe. And because the decision often happens at early stages of research, it can silo off important avenues for exploration.

Approaching questions with a sensitivity to actors’ subjectivity requires that we question contemporary understandings of political geography and investigate what, for the actors in question, is the relevant sense of political space. The CAS framework moves us helpfully in this direction. With such an approach, researchers can leverage deep contextual understandings of particular locales to creatively configure research strategies that stretch beyond specific area specialties. To be sure, CAS also requires notions of areas, and those too will be constructions. This is inescapable. But in breaking out of the typical regional delineations it invites greater reflexivity with regard to the way in which areas are deployed in our research. It reminds us that the answers to our question about Europe may not be found in Europe and that we may need to look elsewhere to even understand what Europe means in that context. That reminder in itself may help to disrupt our sense of familiarity.

This is a lesson that I have learned from my own efforts to understand the origins of electoral systems in nineteenth-century democracies (Ahmed 2013). Limiting the investigation to Europe left a fragmented picture of the dynamics of electoral system choice. Widening the scope to look at the settler colonies, and especially the United States, gave new purchase on the question. The move to incorporate the US in the study was not motivated initially by methodological considerations or a deductive logic of comparison, though the case did add great leverage in these respects as well. Rather, the idea to include the US came from contextual understanding of the European cases, and especially the high frequency of correspondence among elites across the Atlantic on the topic of electoral systems. Indeed, from their correspondence it was clear that across Europe and the settler colonies, elites saw themselves as part of a common project and readily shared strategies to advance that project. While not all CAS applications proceed in

such an inductive or exploratory manner, they are rooted in a commitment to using contextual understanding to specify the appropriate boundaries of inquiry.

Adding the US to the analysis changed both the periodization of the study and the theoretical framing. The key finding in the US case, that single-member plurality (SMP) was not the originary system as was previously assumed, led me to question whether it was the starting point for other cases (Ahmed 2010). Indeed it was not. Rather, most countries, like the US, started with mixed-member plurality and the shift to SMP, like that to proportional representation (PR), was a defensive strategy of pre-democratic parties seeking to retain power. The question then became *not* “why did some countries shift to PR and other retain SMP?” but rather, “why did countries choose to move to PR or SMP, understood as alternative strategies of competition?” This shift in the framing of the research question, though subtle, was critical and theoretically transformative. The compartmentalization of American and European Political Development in our field of study had obscured critical empirical findings and theoretical insights. Moreover, it is a demarcation that makes little sense for the nineteenth century, as the settler colonies were seen very much as an extension of Europe. Even if not

politically tied, they were intellectually and epistemically inextricable. Elites regularly exchanged ideas and political strategies to contain the incoming flow of democracy, and the settler colonies, far from being remote, ignored backwaters, were viewed as laboratories for democracy, a natural experiment unfolding for the benefit of Europe’s great powers.

Given that this particular cross-regional comparison provides so much fertile ground for investigation, it is surprising that more scholars have not made use of it. With some notable exceptions (Martin and Swank 2010; Steinmo 2010; Bateman 2018), the study of American and European political development remains fairly separate in our analysis. To be sure, there are also costs to doing CAS, especially to doing it historically, as it requires deep knowledge and a serious time commitment to developing the ethnographic sensibility necessary to do it well. But, as the CAS volume shows, there are also ways to make such comparisons manageable, through carefully constructed research designs and even creatively leveraged single case studies. While certainly not all will or should take up that call, if the paradigm of CAS encourages more scholars to look past disciplinary regional divides, we will be all the richer for it.

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Synergies of CAS: New Inquires, Theory Development, and Community

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The 2018 publication of Ariel Ahram, Patrick Köllner, and Rudra Sil’s edited volume *Comparative Areas Studies: Methodological Rationales & Cross-Regional Applications* (CAS) inspires enthusiasm from scholars of political science, such as myself, who are already engaged (with some trepidation in the age of mixed-methods and experimental research) in the enterprise of cross-regional contextualized comparisons. Reflecting on my own work, as well as other scholarship in the study of the political economy of development (PED), particularly comparative studies that engage the politics of China as a case, this essay considers how CAS encourages at least three synergies.

First, CAS identifies and motivates comparative investigations of regions and countries based on controlled empirical similarities and differences overlooked by traditional area studies research. Second, CAS facilitates the development of theories inspired by active engagement of theoretical and substantive advances in area studies. Third, CAS acknowledges existing scholarship and unites researchers engaged in cross-regional contextualized comparisons with

area studies scholars to create new inquiries and new communities.

New Inquiries: Nontraditional Assumptions of Similarities and Differences

The research agenda outlined in Ahram, Köllner, and Sil (2018) promotes the conduct of investigations unencumbered by traditional assumptions of similarities and differences between cases which may no longer hold (due to changing circumstances or timing, or both) or were based on outmoded stereotypes that burden rather than enlighten. Cheng Chen’s (2018) chapter, which investigates anti-corruption campaigns in China and Russia, joins other researchers engaged in work using China as a major case, crisscrossing the traditional boundaries of area studies. In traditional area studies research, on the one hand, China is often compared to its East Asian neighbors, regardless of China’s differing level of development, timing in global economic integration, and regime type, which contrast with East Asia’s newly industrialized countries (NICs).

A systematic comparison of China and the NICs that seriously considers contextual factors assumed to be similar shows profound differences which lead to different outcomes. My first book (Hsueh 2011) on China's regulatory state, which I contend is part and parcel of the country's globalization strategy, incorporates case studies of Taiwan, South Korea, and Japan. Shedding light on differences between China and the NICs, Hsueh (2011) questions traditional assumptions of similarities due to ethnocentric expectations and historical associations, and engages dominant perspectives in PED about modes of global economic integration and relationship to state control. China has historical and cultural ties to its East Asian neighbors; however, the country's post-1978 global economic integration in the context of neoliberalism and post-Cold War global politics, and Japanese colonialism and the Cold War during the NICs' similar stage of development, are important contextual factors, which profoundly shape variation in the global economic integration of China and the NICs.

On the other hand, Russia is often compared with countries in post-Soviet Eurasia. In her chapter, Chen (2018) persuasively argues for comparing the "two largest post-Communist giants" (134) in new inquiries, such as the ways in which the authoritarian party-state controls corruption, where the combination of capitalism and political authoritarianism serve as controls in the research design. Chen shows that a "well-matched and context-sensitive comparison could reveal significant divergence in the elite politics and institutional capacities of these regimes that would otherwise likely be obscured by single-case studies or studies restricted to one single geographical area" (134-135). All the same, Chen acknowledges that it may not always make sense to compare China and Russia, such as when research questions "assume scope conditions found primarily in one geographic area" (134), including studies on post-communist party systems, electoral institutions, and European integration.

Comparative Area Studies thus reconciles with Tulia Falletti and Julia Lynch's (2009) contention that "if causal mechanisms are portable but context-dependent, then to develop causal theories, we must be able to identify analytically equivalent contexts as well as specify where one context ends and another begins" (1154). By carefully delineating commonalities and similarities across cases, CAS contributes to the endeavor of generalizability in theory building. The precise combination of capitalism and post-Communist authoritarianism in China and its impacts might be overlooked by situating China

only in Asia. Likewise, understanding Japan only as an Asian country might overlook how its coordinated market economy function in patterns comparable to the advanced industrialized economies of Germany and France, as Steven K. Vogel (1996) has shown.

More nuanced comparative analysis grounded in deeper substantive understanding of regions and countries empowers the analyst to uncover the actual causal mechanisms at work. Pranab Bardhan (2010)'s comparative study of China and India shows that political institutions matter for development; however, it is not regime type per se but rather accountability institutions at different levels, which shape development outcomes. Without them, authoritarianism can distort development while severe accountability failures mar democratic governance. Likewise, the comparative studies brought together by Martin Dimitrov (2013) showcase the work of respected scholars of China and Russia, including Kellee Tsai and Thomas Remington, on understanding why in the post-1991 Soviet collapse, communism endured in five countries while it fell away in ten others. They argue and show substantively that differences in institutional adaptations shape the extent and scope of communist resilience.

Theory Development with Deep Engagement of Cases across and within Areas

"Contextualized comparisons steer a middle course between radical excisions of context-free large-n analysis and the thick, idiographic tendencies of area studies" (Ahram 2018, 156). The works in Ahram, Köllner, and Sil are in step with attempts to develop and evaluate theory armed with the willingness to engage in the deepening of knowledge of carefully selected country, intracountry, and cross-regional cases. Cross-regional contextualized comparisons offer the opportunity to "triangulate" data, just as mixed-methods research purports to do (Sil 2018). In his chapter, Sil contends that theories developed with within-case analysis (whether intra-country or intra-region) can be tested in another area, which triangulates as different types of data would. The merits of qualitative research and controlled comparisons are beyond the "close-up process-tracing analysis of a well-fitted case that usually confirms or illuminates a general proposition derived statistically or deductively" (227).

Cross-regional contextualized comparisons as advocated by CAS also synergize with the analytical leverage identified by Richard Locke and Kathleen Thelen (1996) in the comparison of similar political

developments in very different institutional contexts to understand their differences in extent and scope. Dan Slater and Daniel Ziblatt (2013) more recently underscore the indispensability of controlled case comparisons in generating internal and external validity in spite of political science's "multi-method turn" (3). Slater's 2005 study with Richard F. Doner and Bryan K. Ritchie, which challenges conventional wisdom about state autonomy in the developmental state, is developed with East Asian cases and further tested with their deep knowledge of cases from Southeast Asia.

The active engagement of scholarship across regional and country areas can inspire conceptual, theoretical, and substantive rigor, with methodological and theoretical implications (whether in triangulation of data, identification of causal mechanisms, or in the development of theory). CAS as a method of dynamic engagement of existing area studies scholarship can theoretically and substantively inform us about each individual case if findings are thoughtfully situated in existing debates and when scope conditions are clearly delineated, and claims are unambiguously defined.

In researching the country and sector cases of my next book, in addition to conducting in-depth fieldwork, I have delved into debates in area studies in ways that go beyond either accepting existing studies as never problematic or always biased and questionable. I have uncovered important divergences and similarities in how historical and primary records are understood. This discovery empowers me to tackle existing debates and new puzzles as a result of engaging them through the active triangulation of data, including pursuing primary documents and alternative secondary accounts. This is akin to what Ian Lustick (1996) describes as self-conscious use and Marc Trachtenberg (2009) refers to as the active approach toward encountering historiography as previously constructed narratives. I also avoid the "apolitical and ahistorical" reification of the market as a neutral and natural institution, as Kiren Chaudhry (1993, 246) has warned against. In this manner, CAS facilitates empirically grounded analysis and constructive extensions of and departures from prevailing knowledge without the unreasonable requirement to master deep knowledge of multiple countries from multiple regions.

Hsueh (2012) shows that in China and India's integration into the global economy, China and India have departed from neoliberalism, in addition to the diverging trajectories of the East Asian and Latin American NICs during a similar stage of development. Both countries have taken a "liberalization two-step," which

follows macro-liberalization with micro-level sectoral reregulation. Yet China and India have reregulated with political logics historically rooted in very different perceptions of strategic value and sectoral organization of institutions. In order to examine dominant patterns of market governance structures, I incorporate the same sectors in Russia into the comparative analysis (Hsueh, forthcoming), in addition to examining as shadow cases the same sectors in other countries of comparable size and timing in globalization.

Self-conscious engagement with existing debates in area studies has forced me to analytically clarify my independent and dependent variables, with the effects of specifying my research questions and carefully delineating my study's scope. It has helped me to elaborate on my controls, similarities experienced by my study's main countries (China, India, and Russia) and sectors (telecommunications and textiles). I am able to then negotiate agential and structural differences across and within the cases to refine and better articulate my theoretical framework. Showing that perceived strategic value operates across countries at the national level as well within country at the sectoral level maximizes the utility of analytical comparisons that Theda Skocpol and Margaret Somers (1980) identifies as "parallel demonstration of theory" and "the contrast of contexts" (175). It also reconciles with the CAS endeavor to identify and characterize generalizable political processes with regional and national variations.

Accumulation of Knowledge and Community Building

The CAS research agenda explicitly advocates bringing together scholars engaged in this type of scholarship, and for them to "engage with ongoing research and scholarly discourse within area studies communities" (Ahram, Köllner, and Sil 2018, 4) because "area studies can no longer be considered outmoded" (44). The community building effort is to be commended at a time when the discipline privileges certain methods and types of research, and scholars, such as myself, feel isolated in spite of a rich body of outstanding scholarship and a thriving, growing community of likeminded academics. Already I have benefited immensely from reading the works of and then meeting the excellent scholars behind the research published in the edited volume.

In addition to exposing scholars employing cross-regional contextualized comparisons, CAS recognizes the rich body of scholarship already engaged in this enterprise. Köllner, Sil, and Ahram's (2018) introduction

to the edited volume acknowledges that CAS's "use of the comparative method to surface causal linkages portable across world regions" and to engage academic "discourse in two or more area studies communities," in addition to balancing "deep sensitivity to context," (3) is not new. Indeed, in the study of PED, Atul Kohli (2004)'s systematic comparison of colonialism and the origins of patterns of state construction and intervention in South Korea, Brazil, India, and Nigeria exemplifies the best of controlled comparisons and portable causal mechanisms and regularities.

In addition to Kohli, an expert on India, China scholar Dorothy Solinger (2009) shows how representative countries from different regions (China, France, and Mexico), to alleviate crises of capital shortage in the neoliberal era, recalibrated their revolution-inspired political compacts between labor and the state to join supranational economic organizations. Mary Gallagher (2002)'s *World Politics* article compares China to Eastern Europe (Hungary) and East Asia (South Korea and Taiwan) to problematize the relationship between economic and political reforms. Yu-Shan Wu (1995)'s book, which systematically compares China, the Soviet Union, Hungary, and Taiwan, is an earlier endeavor of area studies meet generalizable inquiries. As is that of Chalmers Johnson's 1962 book, which contrasts the communist mobilizations of China and the Soviet Union.

More recent contextualized cross-regional research includes Mark W. Frazier (2019)'s comparative historical analysis on the impacts of urban land commodification

on variation in patterns of contentious politics in Shanghai and Mumbai. Frazier's work and my next book join the growing number of systematic comparisons of China to other globalizing countries of comparable circumstances and demographics, which transcend traditional boundaries of area studies.¹ These latest studies demonstrate that China can be a useful case to test and inform theories in comparative politics and comparative economic development. Whether emphasizing structural endowments, domestic and global actors and institutions, or the enduring salience of ideas, these works adopt the comparative method to examine national and subnational, micro-level variations. The cross-national analysis and subnational disaggregation enable systematic investigations that otherwise would not be possible with a focus only on macro or micro-level factors that make these countries seemingly difficult to track together.

Ahram, Köllner, and Sil's research agenda, showcased by Chen's chapter and past and present studies employing cross-regional contextualized comparisons with China as a major case in the last decade, amplifies Lily Tsai's (2017) call to China scholars "to build on previous scholarship on China while working actively with non-China colleagues to identify shared questions about political phenomena that exist beyond China" (26). Doing so extends beyond ensuring "hard-won findings about China fully contribute to knowledge" (26); it actively promotes new inquiries and new communities engaged in cross-regional and interregional contextualized comparisons.

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Making Sense of Multipolarity: Eurasia's Former Empires, Family Resemblances, and Comparative Area Studies

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As the West retrenches and new powers emerge, students of international relations are well positioned to address an outstanding question: *How to thrive in a multipolar world?* The question—and the answers which we bring to bear—resonate beyond geopolitics. This is because the task of living together in diversity is arguably the greatest analytical as well as normative challenge facing world politics more broadly (Fisher-Onar, Pearce, and Keyman 2018).

In this intervention, I address the question of living together in a multipolar world from an IR perspective. I suggest that dominant approaches like realism and liberalism, which favor Western-centric categories and large-N data, fail to capture important dynamics. I then make the case for family resemblances as a method of cross-regional comparison which enables the analyst to examine cases typically boxed into different area studies compartments. Finally, I operationalize the approach towards a baseline for comparison across Eurasia's revisionist former empires: China, Russia, Iran, and Turkey. I argue that by thus establishing a basis for comparison, we uncover patterns relevant to prospects for cooperation as well as conflict in a post-Western world.

Multipolarity: Views from the IR Tower

Attempts from within IR to make sense of multipolarity are often informed by positivist approaches like realism and liberal institutionalism.¹ Realist tools include concepts like revisionist versus status quo powers and their quest for status (Davidson 2006; Volgy et al. 2011), hegemonic stability, its eclipse and preventive war (Gilpin 1988; Levy 2011), the balance of power (Paul, Wirtz, and Fortmann 2004; Kaufman, Little, and Wohlforth 2007), and power transition (Tammen 2008). Such work offers a bird's-eye view and can help elucidate major mid-range questions like prospects for war between the retrenching United States and rising China.

Yet, there are limitations for the study of multipolarity. First, realism privileges substantive questions relevant to

great power—especially American—interests like nuclear proliferation (Kang 2003). This goes hand-in-hand with a tendency to ignore phenomena that appear pervasive to emerging powers—including nascent superpower China—like racialized hierarchies in world order.

Second, realists, like many others across the North American IR academy, tend to favor macro-quantitative methods which aggregate large numbers of randomized cases. By glossing over differences between cases, and ignoring outliers, the claim to universal purchase becomes possible (Berg-Scholsser 2018). The trade-off is that studies do not register nuance (Ahram 2013). As a result, the large-N analyst may overlook major motivational and behavioral patterns, including phenomena with causal force. A case in point is the game-changing role which counterintuitive alliances can play in and across national contexts (Fisher Onar and Evin 2010; Hart and Jones 2010).

An alternative approach is liberal institutionalism. Liberals are more likely to open the black box of domestic politics and thus to access non-Western readings of world order. However, liberals' concern is often less with non-Western perspectives than with the capacity of the Western-led liberal order and its institutions to co-opt challengers (Owen 2001; Ikenberry 2008). The primacy placed on Western concerns is evident in the intense but short-lived “hype” (Zarakol 2019) around the BRICS, which dissipated when these emerging economies wobbled by the mid-2010s (Hurrell 2019). Nevertheless, the relative share of economic and normative power enjoyed by the United States and Europe continues to diminish. As anger at relative decline finds expression in phenomena like Brexit and the Trump presidency, the capacity of the Western-led liberal order to absorb challenges under multipolarity remains in question, a concern brought into dramatic focus by the COVID-19 pandemic.

¹ For a discussion of how other, critical approaches within IR address the question, see Fisher Onar 2013; 2018.

Multipolarity: Views from—and across—Area Studies

If realist and liberal frames for reading multipolarity tell only part of the story, how to better access rising powers' perspectives? Given that the challenge is *how to thrive in a world of many poles*, the ability to triangulate across poles is valuable. Engagement of other perspectives can foster epistemological and pragmatic openings for more pluralistic research and foreign policy practices (Saylor, this issue; Acharya 2011; Fisher Onar and Nicolaidis 2013). That said, cross-regional triangulation is useful even if the analyst rejects the critical project of decentering international relations. Strategic reconnaissance of other cultures for defensive or offensive purposes is a well-established tradition. Examples include the adventures of British and Russian imperial agents in the nineteenth-century “great game” over Eurasia, and the foundation of area studies within the US academy during the Cold War to inform policy makers about non-Western regions (King 2015).

These (neo-)colonial origins notwithstanding, area studies today offers interdisciplinary insights into the cultures, economies, political systems, and foreign policies of non-Western powers. It leverages the nuanced knowledge of historians, linguists, geographers, anthropologists, sociologists, and diplomats, among others. Area studies attends, moreover, to issues of geopolitical significance from migration and social movements to political economy and the sociology of religion. In each of these arenas, field experts are likely to draw conclusions that are both more accurate and more contingent than those of counterparts in the IR tower. Such sensitivities can be useful in the management of multipolar complexity.

Yet area studies are no panacea. Respect for complexity is a normative and a methodological commitment; it can yield rich, often counter-intuitive insights, but also insistence on the *sui generis* nature of each case. This tendency is reinforced by the structural division of labor between area compartments within the academy. Thus, experts on one world region (like the Middle East) rarely converse with specialists on or from other regions (like East Asia), nor develop cross-regional expertise. The upshot is that important insights may be difficult to translate across regional specializations, much less to disciplinary IR or political science.

The challenge, then, is to mediate between problem-driven respect for case or cross-case specificity on one hand, and broader relevance on the other. Enter

Comparative Area Studies (CAS), defined by Ahram, Köllner, and Sil (2018, 3) as any “self-conscious effort” to simultaneously: (i) “balance deep sensitivity to context... us[ing] some variant of the comparative method to surface causal linkages that are portable across world regions; and, (ii) engage ongoing research and scholarly discourse in two or more area studies communities against the backdrop of more general concepts and theoretical debates within a social science discipline.”

As Sil (2018) suggests, CAS often entails cross-regional, contextualized small-N comparisons. With regard to emerging powers, this intermediate level of analysis helps to capture variance within and across actors in different regions, teasing out cross-cutting patterns. For example, the ability to recognize that a power struggle is unfolding in X state where moderates are outmaneuvering hardliners, and to compare and contrast such struggles across X, Y, and Z states affords very different insights—and policy prescriptions—than reading states as monolithic blocks (Fisher Onar 2021).

Family Resemblances and Eurasia's Former Empires: China, Russia, Iran, Turkey

There are many ways to operationalize cross-regional comparison as showcased in this symposium and the edited volume by which it was inspired. As a contribution to the toolkit, I invoke the notion of “family resemblances,” defined as cases that share significant overlapping elements even though they may not uniformly display one common feature. As Goertz (1994) suggests, family resemblances offer a handle on concepts which are “intuitively understandable,” such as electoral authoritarianism, but difficult to formulate in terms of “exact specification or definition” due to the presence of overlapping features across cases rather than identical “hard cores” (25).

The notion of family resemblances serves comparative area studies because it enables the analyst to escape the straitjacket of Cold War regional categories which tend to emphasize the role of geography over history, sociology, or economics in shaping outcomes (Pepinsky, this issue). By thus assessing resemblances across regional foci one can identify similarities and differences for fresh insights into actors that are otherwise lumped together (in large-N studies) or kept separate (in single- or area-bound small-N studies). Such patterns, in turn, can be probed towards refining the operative concept, hypothesis generation, identification of necessary and sufficient causal mechanisms, and

inductive theory-building (Goertz and Mahoney 2012).²

To demonstrate, I turn to a cross-regional, contextualized small-N set of cases which demand a medium level of expertise in return for a medium level of portability. The four cases—China, Russia, Iran, and Turkey—are geostrategic but rarely compared. Spanning the Eurasian landmass from the eastern Mediterranean to the Pacific, they have figured prominently in Western grand strategy since at least the great game between Britain and Russia. From the “Heartland” thesis of Anglo-American strategists in the early twentieth century through to Robert Kaplan’s 2018 book *The Return of Marco Polo’s World*, these states have long served as the “other” of European and American geopolitical imaginaries (Morozov and Rumelili 2012; Fettweis 2017). At the dawn of multipolarity, such anxieties are exacerbated by these countries’ revisionist behavior across the vast Eurasian geography (Mayer 2018).

However, operationalizing comparison is challenging. This is due to cross-case discrepancy when assessed via conventional IR or area studies criteria like material capacity or cultural attributes. Thus, for the IR scholar, Turkey and Iran are, at most, multi-regional middle powers with spoiler potential, while Russia is arguably a declining great power, and China a rising superpower. One can draw on the flourishing regional powers literature to address these differences (Nolte 2010; Parlar Dal 2016), but the fact remains that these four states present an “apples, oranges, and cherries” problem, as it were, regarding their comparative magnitude. Meanwhile, for the area studies analyst, historical, linguistic and sundry other specificities make comparisons between even Turkey and Iran problematic, much less with Russia and China.

Nevertheless, there is meaningful overlap, I argue, in China, Russia, Iran, and Turkey’s trajectories. The family resemblance emanates from their common experience as “revisionist former empires.” This feature matters because imperial legacies, both real and imagined, shape national projects and foreign policies (Fisher Onar 2013; 2015; 2018).

Consider that all four are: (i) successor states to large and long-lived, geographically contiguous Eurasian

empires which, (ii) since the seventeenth and eighteenth centuries, and especially during the “long nineteenth century,” were overshadowed by European colonial powers (and by a Japan reinvented along European lines). European expansion was due to military primacy and emergent forms of political and economic organization, namely, the nation-state and capitalist industrialization. But if these features helped Europeans achieve global conquest, (iii) the four Eurasian empires commanded sufficient state capacity to retain formal sovereignty. This overlapping experience distinguishes China, Russia, Iran, and Turkey from the vast majority of non-Western actors who were thoroughly subjugated.³ (iv) In response, moreover, reformists in each empire outmaneuvered traditionalists to pursue military, political, and economic modernization along Western lines for the paradoxical purpose of defense against the West.

(v) The four empires finally collapsed within roughly the same decade in the Chinese revolutions of 1911 and 1913, the Russian revolutions of 1905 and 1917, the Young Turk and Kemalist revolutions of 1908 and 1923; and the establishment of constitutional monarchy in Iran in 1925. (vi) In each case, moreover, it was internal rather than external agents that instituted modernizing authoritarian regimes. And while these regimes displayed great ideological variation as the states evolved over ensuing decades, from the foundational moment to today they have shared one common feature: deep ambivalence towards Western hegemony. (vii) Resentment of the West references the humiliating experience of eclipse,⁴ and is inculcated through school curricula, national media, and commemorative practices, among other nation-building tools. (viii) Today, anti-Western sentiments—and the promise to restore once-and-future glory—are mobilized, in turn, for domestic or foreign policy. (ix) Such agendas are distinctive from post-colonial projects, which tend to eschew expansive claims. For China, Russia, Iran, and Turkey, however, the frame is of manifest destiny regarding their ability—realistic or otherwise—to play order-setting roles in former imperial geographies. (x) Finally, overlapping resentment of the West and aspirations to power projection inform policy coordination (Kavalski 2010). This is evident in

2 Family resemblances are especially useful for analysts committed to causal inference. Soss (2018) develops the logic for interpretivist scholars, arguing that an exploratory commitment to “casing a study” rather than “studying a case” can better capture dynamics on the ground. Both approaches hold promise for problem-driven, cross-regional comparisons in a multipolar world.

3 This is a feature I elsewhere theorize in juxtaposition to the colonial and post-colonial condition as the “concessionary condition” in reference to the imposition of Capitulations regimes by European powers rather than full-fledged colonial control (Fisher Onar 2021).

4 The long-nineteenth century eclipse of these states’ *ancien regimes* by European powers is one source of what Zarakol (2010) characterizes as “stigmatization” within international society, as is the Cold War experience of domination by the United States (notably in Russia, where imperial nostalgia is arguably strongest for the Soviet rather than the Czarist period).

endeavors like the Shanghai Cooperation Organization, or the Astana group, via which Russia, Iran, and Turkey have sought to shape outcomes in Syria. Such initiatives hardly augur a unified block, but they provide discursive and institutional frameworks (Schmidt 2008) for both cooperation and rivalry, informed by an overlapping sense that the time for Western power projection across Eurasia is over.⁵

Thus, despite obvious differences, recognizing the family resemblance between China, Russia, Iran, and Turkey as “revisionist imperial successor states” enables exploration of compelling mid-range questions as the West retrenches: What commonalities and differences drive revisionist projects? How do national narratives, steeped in resentment of ebbing Western hegemony, shape policies? How, for example, do such frames intertwine with status-seeking behavior? And can they authorize action that defies rational choice expectations? If so, how do patterns at the sub- or trans-national levels compare with—and potentially mitigate—revisionism at the interstate level? What, ultimately, do our answers suggest for the propensity of Eurasia’s resurgent powers to clash or cooperate with each other, and with Western counterparts?

The toolkit of CAS can help to at least begin addressing such questions in ways that do not exclude (re-)emerging powers’ perspectives.

Conclusion

In sum, at the dawn of multipolarity, students of world politics—including but not limited to IR scholars—must make sense of non-Western diversity. To supplement an analytical apparatus forged in the West for stronger cross-regional comparisons, I have proposed a comparative area studies (CAS) framework with which to examine similarities and differences in the revisionist behavior of four major actors rarely studied in concert. Proposing “family resemblances” as a tool for comparison, I show that China, Russia, Iran and Turkey are “revisionist former empires” (Fisher Onar 2013; 2018) which can be assessed vis-a-vis their imperial pasts, and the ways such legacies shape domestic and foreign policy today. By thus establishing a baseline for comparison, individual or collaborative research can explore mid-range questions regarding cooperation and conflict between resurgent Eurasian powers, and in their relations with Western counterparts. The study of family resemblances across other traditionally-segmented area studies foci can likewise elucidate outstanding real-world problems.

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⁵ An interesting question beyond the scope of the present essay but bearing further exploration regards how many resemblances must be present to constitute a legitimate basis for comparison. Soss’s (2018) work on how to reflexively “case studies” as the analyst interpolates between empirics, theories, and research question rather than “studying cases” as pre-existing phenomena may offer some answers.

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The Sweet Spot in Comparative Area Studies: Embracing Causal Complexity through the Identification of Both Systematic and Unsystematic Variables and Mechanisms

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The tremendous value of Comparative Area Studies (CAS) is difficult to overstate, as CAS scholars appear to accomplish the impossible: reaching broad-ranging conclusions from cross-case comparisons spanning two or more geographic regions, while still incorporating the sort of deep and detailed knowledge of people and places that is the hallmark of classic area studies. CAS researchers not only showcase the approach's great strengths; they also encourage more work along these lines, since CAS contributions comprise only around 15 percent of recent works in comparative politics (Ahram, Köllner, and Sil 2018, 17). With this encouragement comes some welcome advice, including a push for more precisely conceptualized variables so that they are portable across contexts, admonitions against the assumption that geographic proximity defines the full population of cases to which one's theory applies, and a reminder that idiosyncratic factors are no less important than systematic conditions when it comes to causal explanation.

This essay offers additional advice to enhance the CAS approach, starting from the premise that Comparative Area Studies' greatest strength is also its main challenge: striking a balance between fully context-sensitive case studies, and the development of generalizable causal theories. I argue that CAS scholars can better balance these idiographic and nomothetic goals through more careful consideration of the logic of causal inference guiding one's research. In particular, CAS scholarship would benefit not only from more explicit attention to whether explanatory variables found to travel across regions are necessary, sufficient, INUS, or SUIN, but also from a more conscious effort to determine whether or not the causal mechanisms linking explanatory variables to outcomes also travel across regions. In other words, does X_1 cause Y_1 *in the same way* in one region or area as it does in another?

Good qualitative hypothesis testing typically entails two things: establishing the causal importance of

variables that cases have in common through cross-case analysis and identifying the mechanisms that link those variables to the outcome of interest through within-case process tracing. For the first task, CAS scholars seek to test whether a causal theory that explains cases in one region or area also explains cases in regions or areas other than the one in which that theory was initially developed. Yet it is not always clear what it means for a theory to "travel" across areas. For instance, if X_1 , X_2 , and X_3 are found to cause Y_1 in cases in Southeast Asia, should CAS researchers reject the cross-regional generalizability of the causal theory if they find that X_1 and X_2 , but not X_3 , are causally significant for Y_1 in cases in Latin America? Part of the problem is that assessing a theory's generalizability is not as simple as determining whether X_1 , X_2 , or X_3 is present or absent across all cases with the outcome Y_1 . Here is where more careful attention to the nature of explanatory variables in relation to each other and to the outcome can help.

In particular, CAS scholars should first specify whether the explanatory variables under consideration are necessary, sufficient, INUS (an insufficient but necessary part of a larger cause that is itself sufficient but unnecessary), or SUIN (a sufficient but unnecessary part of a larger cause that is itself insufficient but necessary) (Mahoney, Koivu, and Kimball 2009). Doing so would allow the researcher to then consider whether his or her causal theory is cross-regionally generalizable—meaning applicable to cases in more than one world region—despite cases examined in the second region not having the exact same combination of explanatory variables as the cases examined in the first region. For instance, in the example above, failing to find X_3 in any of the Latin American cases would not render the causal theory inapplicable to Latin America if X_3 is only a sufficient, but not necessary, cause of Y_1 in the Southeast Asian cases. Likewise, consider the possibility of X_3 being an INUS variable, as in the following causal equation:

Figure 1. $(X_1 * X_2) + (X_3 * X_4) \rightarrow Y_1^1$

Again, finding X_1 , X_2 , and X_3 in the Southeast Asian cases, but only X_1 and X_2 in the Latin American cases, would still confirm that one's theory travels across regions since X_3 is part of a causal combination that is not necessary to produce the outcome Y_1 . Finally, consider what would happen if X_3 were a SUIN variable, as in each of the following possibilities:

Figure 2. $(X_1 + X_2) * (X_3 + X_4) \rightarrow Y_1$

Figure 3. $X_1 * (X_2 + X_3) \rightarrow Y_1$

Figure 4. $X_2 * (X_1 + X_3) \rightarrow Y_1$

Figure 5. $X_1 * X_2 * (X_3 + X_4) \rightarrow Y_1$

Once more, finding that X_1 , X_2 , and X_3 cause Y_1 in the Southeast Asian cases, while only X_1 and X_2 cause Y_1 in the Latin American cases, would not necessarily render one's causal theory ungeneralizable across regions, unless one of the Latin American cases were missing not only X_3 but also X_4 in the scenario represented in either Figure 2 or Figure 5.

Note that X_3 —whether sufficient, INUS, or SUIN—can still be considered a systematic variable, even if it does not appear in any of the Latin American cases, because X_3 is still part of a larger causal model that explains cases in both regions. It is important to keep in mind, however, that a complete causal explanation for any one case often also includes unsystematic variables, meaning factors that are truly unique to a single case, which CAS scholars are right to recognize as no less important for causal explanation than systematic variables, which contribute to causal explanation in at least two cases. Cross-case analyses help scholars separate systematic from unsystematic variables so we can identify the generalizable parts of the causal story *even if* the full causal explanation for any one case also includes idiosyncratic factors that cannot be generalized beyond a single case.

That said, it is possible that what appears at first to be an unsystematic variable in the initial analysis of cases in one region is later revealed to be a systematic variable once additional cases are analyzed in a different region. For instance, X_1 , X_2 , and X_3 might be found to cause Y_1 in every Southeast Asian case except one, which instead features X_1 , X_2 , and X_4 . At first, X_4 would appear to be

idiosyncratic to that single Southeast Asian case. Adding Latin American cases to the analysis, however, could reveal that most Y_1 cases in Latin America are also caused by X_1 , X_2 , and X_4 , meaning X_4 is a systematic variable after all. Such a scenario would suggest the causal model represented in Figure 5.

In sum, the first way for CAS scholars to test whether their causal theories travel across regions is through cross-case analysis. Crucially, testing for the generalizability of a causal theory is not the same thing as expecting every positive (Y_1) case within one's scope conditions to feature the exact same combination of explanatory variables as every other Y_1 case. Rather, what matters is whether each explanatory variable is necessary, sufficient, INUS, or SUIN since the role each variable plays in the full causal model tells the researcher how to interpret that variable's presence or absence in each case. Only fully necessary variables should be expected to appear in every Y_1 case.

The second way for CAS scholars to test whether a causal theory is generalizable beyond a single geographic region is through a cross-regional analysis of causal mechanisms. Qualitative researchers rarely rely on cross-case analyses alone to test their causal hypotheses. Instead, they combine cross-case methods with process tracing, a within-case method of causal inference that provides evidence of the specific processes through which explanatory variables actually cause the outcome in question. Arguably, causal mechanisms are at the core of theory development, which requires the researcher not only to identify a non-spurious correlation between explanatory variables (X_1 , etc.) and the dependent variable (Y_1) but also to explicate *how* and *why* those explanatory variables actually cause the dependent variable. Therefore, if scholars strive to develop truly generalizable causal theories, they should test not only whether the variables in their causal models travel across regions but also whether, holding variables constant, the same causal *mechanisms* connect those explanatory variables to outcomes in different cases. This advice applies to qualitative comparisons in general, but should prove especially valuable for CAS scholarship, which can evaluate the generalizability of causal theories by searching for recurring causal mechanisms across cases in different regions.

The distinction between variables and mechanisms is an important one. If a researcher finds that X_1 and X_2 are causally significant for Y_1 in all cases examined across both Southeast Asia and Latin America, it is still

1 Following the norms of Boolean algebra, the + denotes the logical OR, and the * denotes the logical AND.

possible that the specific processes through which X_1 and X_2 cause Y_1 actually differ across the two regions. That is, X_1 and X_2 might cause Y_1 through one mechanism in Southeast Asia, and through an entirely different mechanism in Latin America. Such equifinality in causal mechanisms, again, holding variables constant, would call into question the cross-regional generalizability of the causal theory. Yet this is exactly where CAS scholars' deep area knowledge can bring balance to the analysis. By conducting fully context-sensitive case studies that "get the story right" as best as possible for each case through consideration of case-specific background details and vital idiosyncrasies, CAS scholars are well positioned to assess whether equifinality in causal mechanisms is

caused by something systematic within or across regions or by factors that are unique to individual cases.

Political scientists will increasingly view Comparative Area Studies not just as a welcome addition to the qualitative methods toolkit, but as outright indispensable for moving comparative politics and related subfields forward. The two main goals of CAS scholarship— theoretical breadth and case-specific depth—are not at odds and actually enhance each other in several ways. Getting the most out of CAS, however, will require greater consideration of the specific causal role each explanatory variable plays within a causal theory as well as closer attention to whether or not causal mechanisms, not just variables, travel across regions.

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What's the "Area" in Comparative Area Studies?

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Comparative Area Studies (CAS) promises to bring together the method of focused qualitative comparison and a sensitivity to area context in multiple world regions. Ariel Ahram, Patrick Köllner, and Rudra Sil's *Comparative Area Studies* (2018), for example, provides a wonderful overview of how comparativists can learn from what might seem to be audacious cross-regional comparative projects. What could be more interesting than insisting that we read more European political history to make better sense of the case of the United States (Ahmed 2018) or identifying the "Arab" Spring in Israel and Mali (Ahram 2018)? I suspect that for many comparative social scientists, the very idea of learning about something familiar by comparing it with something very different is what attracted us to our field in the first place.

And yet the broader enterprise of CAS rests on what I consider to be a profoundly conservative orientation towards the world's regions. The starting point for this short essay is the observation that the literature on CAS almost universally conceptualizes "areas" or "world regions" in traditional Cold War terms (see e.g., Ahram, Köllner, and Sil 2018; Basedau and

Köllner 2007). Although areas such as "Latin America" and "the former Soviet Union and Eastern Europe" do reflect geographical features and some world-historical processes, as categories they primarily reflect Western, and in particular American Cold War, *political* categories. An alternative model for CAS would be to reject these traditional conceptualizations of area and embrace more historically grounded or socially meaningful understandings of the world: former Spanish colonies, former Ottoman territories, Zomia, the Indian Ocean and Mediterranean worlds, communist single-party states, and others. Some comparative area specialists have suggested how to do this; for example, Cheng Chen (2018) remarks that the post-communist world encompasses both the former Soviet Union and parts of Asia and Latin America. One future for CAS is to reconfigure "areas" and "regions" around these alternative ways of organizing cross-regional comparisons, thereby joining critics of "area studies" as commonly understood from across the humanities and social sciences.

The remainder of this essay develops this argument. In the next section I use the discussions in Ahram, Köllner, and Sil (2018) to identify what I consider to be

a relatively thin substantive understanding of regions or areas, and their contribution to the enterprise of CAS. I then turn to the case of Southeast Asia—a particularly diverse and rather problematic world region—to illustrate the limits of regional knowledge and the necessity of cross-regional comparisons for most useful comparative social science. Based on these examples, I then conclude by discussing a future for CAS that rejects traditional definitions of world regions in service of a more substantive understanding of how nation-states might be classified or categorized.

Area Knowledge in Comparative Area Studies

Area studies insights and regional expertise have always shaped the development of comparative politics; periodic worries about the demise of area studies notwithstanding, this is unlikely to change. Writing about the third wave of democratization twenty years ago, Valerie Bunce (2000, 716) explained both the pragmatic and substantive reasons why research has been organized by world regions:

Intellectual capital, the temporally clustered character of these regional transitions, and the undeniable appeal of carrying out controlled, multiple case comparisons are all compelling and convenient reasons to compare Latin American countries with each other, post-Socialist countries with each other, and the like.

CAS looks beyond what Bunce called the “bounded generalizations” that come from within one region in search of the possibilities of (and limits to) further generalization—while remaining faithful to the insights that only area knowledge can provide.

In addition to seeing whether findings generalize, cross-area comparisons are particularly valuable for demonstrating whether *concepts* developed within one region travel or not. The chapter by Von Soest and Stroh (2018), for example, discusses *neopatrimonialism* in sub-Saharan Africa, and the roughly comparable concepts of *bossism* from Southeast Asia and *caudillismo* from Latin America. If neopatrimonialism only makes sense in its application to sub-Saharan Africa, then the concept is useful, but narrow; if it is roughly synonymous with bossism and caudillismo, then all three might be replaced with a more general concept that encompasses them all. Comparing only across regions while maintaining careful attention to the intention of each concept—which depends on the area studies context in which the concept emerged—makes this possible.

Examples such as this, unfortunately, are rare among scholars working explicitly in the CAS tradition. Most invocations of CAS focus on what can be learned by comparing what might seem to be very different cases, and Mill-style defenses of the utility of comparing in this way. Actual *conceptual* insights drawn from comparing across areas are almost entirely absent.

It could be that as CAS continues to mature as an intellectual agenda, it will focus more on concepts and findings that have emerged from rich area studies debates, and that productively travel across regions. But what if such conceptual contributions are rare because “areas” are not analytically meaningful? Quoting Bunce (2000) further,

At the most general level, region is a summary term for spatially distinctive but generalizable historical experiences that shape economic structures and development and the character and continuity of political, social, and cultural institutions... Region, therefore, lacks the specificity we value as social scientists. Among other things, it tends to be too variable in what it means—over time and across research endeavors. It is also easily misunderstood and all too often underspecified. (722-3)

In this view, comparative social scientists ought to be skeptical of world regions as conceptual categories. It is the “historical experiences” and “institutions” that are of real interest, and our attention should be focused on these rather than on the geographic “summary term” used to classify particular countries.

I do not wish to make too much of this critique. Plainly, sub-Saharan Africa just *is* different than East Asia. But for the “area” in CAS to be meaningful, it must do real analytical work. I see little evidence that the areas or world regions in CAS are doing anything more than representing a handy shorthand for “this country is different and far away from this other country.”

What’s in an Area?

My view is that areas are doing little analytical work in CAS because world regions rarely do much analytical work even under the best circumstances. To see why, I will invoke the case of Southeast Asia. Of all world regions or areas, it is perhaps the most obviously a social construction. It is not united by language, colonial history, climate, biogeography, race, religion, or anything else. Southeast Asia is nothing more than the stuff between South Asia, East Asia, Australia, and the Pacific.

Few Southeast Asianists really take the region seriously as a world region or area with an inherent or objective internal logic.¹ “Southeast Asia” exists because of what I have elsewhere termed the “historical accident” (Pepinsky 2015) of World War II, and it persists because of the convenience of perpetuating the academic division of labor. This is not to dismiss Southeast Asian studies as a field of study, but rather simply to note, as Ashley Thompson (2012) writes, that “the existential question—[what] is Southeast Asia?—has been constitutive of and essentially coterminous with the field of Southeast Asian Studies” (3).

A Southeast Asianist like me² will approach the very premise of CAS with some inherent skepticism. Sure, we should compare across areas or world regions, using the insights from other regions to enrich what we know about our own while endeavoring to remain sensitive to the regional or national context of each case. But that is what most Southeast Asianists already do, because we have to. Communist single-party regimes are rare, so comparing Vietnam with another case requires looking outside of the region, to East Asia (Malesky, Abrami, and Zheng 2011). Cases of regime collapse in Muslim-majority authoritarian regimes are also rare, so comparing the fall of Indonesia’s New Order to another case of Muslim-majority regime change requires looking to the Middle East (Pepinsky 2014). My understanding of CAS in Southeast Asia differs rather starkly from Huotari and Rüländ (2018), who focus on concepts such as Anderson’s (1983) “imagined communities” or Slater’s (2012) “strong state democratization” that might usefully travel to other world regions. In my view, Southeast Asia as a *region* has not done much analytical work in these or any other contributions. *Country* knowledge is essential; *regional* knowledge is not. Generalizing beyond the countries that inspired them is not Comparative Area Studies, it is just regular Comparative Politics. The same is equivalently true for many old and new classics in comparative politics that compare cases across world regions: Theda Skocpol (1979) on social revolutions in France, Russia, and China; Anthony Marx (1998) on race in South Africa, Brazil, and the United States; and Susan Stokes et al. (2013) on brokers in Argentina, India, and Venezuela.

And outside of the more positivist social sciences, the notion that one would look beyond the traditional

world region is part and parcel of what most people who study the countries that comprise Southeast Asia actually do. Themes of movement, border-crossing, and reconfiguration of Western conceptual categories to reflect more socially meaningful geographies can be found across the humanities and interpretive social sciences. Such research is not really CAS in the sense that authorities in the methodology such as Ahrām, Köllner, and Sil (2018) mean it, because it is not really about comparing units. But it does mean that the study of Theravada Buddhism in Thailand requires some understanding of a “southern Asian Buddhist world characterized by a long and continuous history of integration across the Bay of Bengal region” (Blackburn 2015), and that studying Southeast Asian *hajjis* means studying the Indian Ocean networks that they follow (Tagliacozzo 2013). And in fact, one of the most influential conclusions from the past twenty years of Southeast Asian studies is that vertical geography is often more consequential than spatial geography. The highland area termed “Zomia” (van Schendel 2002) that spans East, South, and mainland Southeast Asia comprises a more socially meaningful “region” for most of history than does the WWII-era concept of “Southeast Asia.”

“Areas” as Substantive Themes

One response from a defender of CAS might be to hold that Southeast Asia is a misfit area, not representative of the other areas. Perhaps this is true. But I wish to offer a more constructive response, in which the Southeast Asian experience generalizes. One future for CAS would be to redefine “areas” or “regions” as traditionally understood. Rather than reifying world regions as substantive entities or even as analytical categories, CAS might reconfigure world regions or areas along substantive themes: colonial, religious, linguistic, geographic, or political. In what follows I offer examples of each, drawing from prominent themes in Southeast Asian politics.

That different colonial regimes endowed postcolonial societies with different social and institutional legacies is an old theme in the social sciences. Rather than imagining Southeast Asia as a region, one might instead look at the former British or Spanish empires as providing the natural regions within which to compare what are otherwise very different countries like Myanmar and the Philippines.

1 It is interesting to note that international relations theorists take the region-ness of Southeast Asia much more seriously than comparativists or area specialists, whose job it is to know the politics of the countries in it (see e.g., Acharya 2013).

2 I recognize that there is an irony in identifying as a Southeast Asianist but then criticizing the usefulness of this concept of Southeast Asia. In my own case—which is common among regional experts—I became a “Southeast Asianist” only upon applying for academic jobs and being expected to teach courses on Southeast Asia.

This might suggest comparing direct and indirect rule in British India and British Malaya, or “cacique democracy” in the Philippines (Anderson 1988) with its counterparts in Latin America. These comparisons are only surprising “inter-regional” comparisons relative to a narrowly geographical understanding of regions.

World religions also provide a substantively meaningful way to conceptualize world regions. The Muslim world and the Theravada Buddhist world, as noted above, both would group some Southeast Asian countries with other countries from South Asia (the Theravada Buddhist world) and further afield (the Muslim world). Catholic majority countries would lump the Philippines with southern and central Europe and Latin America; Vietnam and Singapore would join China, Japan, and Korea in their combination of Mahayana Buddhism with Confucian principles. For questions of identity, religious mobilization, or state-religious authority relations, these might prove to be much more useful conceptual categories than would any geographic area.

Southeast Asia’s linguistic diversity is particularly striking. Also striking is how *some* countries find themselves part of a broader community defined by colonial language. Timor-Leste, a former Portuguese colony occupied for a quarter century by Indonesia, immediately joined the Lusosphere upon independence in 2002. Although this group of countries also shares a history of Portuguese colonialism, so colonial and linguistic heritage overlap perfectly, the phenomenon of a European language spoken primarily by a *mestiço* elite serving as a tool to build national identity in plural societies travels well across the Lusosphere (and travels poorly elsewhere in Southeast Asia).

Geography does serve as a convenient tool for classifying world regions, and “horizontal” or “flat map” geography does capture important spatial variation around the world. But as discussed above in the discussion of Zomia, “vertical” geography provides an alternative conception of space that can unite upland peoples across world regions—and, as a result, lowland peoples as well. Other geographies might focus on water rather than land as the unifying characteristic: the Indian Ocean world,

for example, or the littoral states of East and Southeast Asia around the East Vietnam/West Philippine/South China Sea.

The final substantive theme through which to reconfigure world regions is political. The postcommunist world includes Vietnam and Laos alongside the former Soviet Union, China, Cuba, and so forth. Petroleum-rich hereditary sultanates include Brunei Darussalam alongside the United Arab Emirates and Qatar. Other regime types unite the competitive authoritarian regimes of Singapore and (formerly) Malaysia with counterparts in Tanzania and (formerly) Mexico, and the junta in Thailand under Prayut Chan-o-cha with Egypt under Abdel Fattah el-Sisi (Geddes, Wright, and Frantz 2014).

Each of these examples follows a common logic: rather than seeing whether concepts or findings travel from one regional context to another, they start with the assumption of comparability based on a substantively or theoretically relevant characteristic and use this to define the scope conditions of a particular analytical or empirical claim. There are naturally risks to this exercise, as the assumption that communism or colonial heritage forms a natural comparison set itself warrants further investigation. And insofar as world regions serve as the primary organizational units for comparative politics more broadly, this argument also implies that the broader interdisciplinary practice of conceptualizing the world into regions warrants further scrutiny.³ But refiguring “areas” around substantive rather than geographic variables may prove to be a useful way to develop the logic of CAS further, with implications that travel to comparative politics as a discipline more broadly.

The argument I make here is not to imply that CAS ought to discard “Latin America” or “the Middle East and North Africa”⁴ as categories. Rather, CAS researchers ought to strive to “replac[e] proper names of social systems by the relevant variables” (Przeworski and Teune 1970, 30); here, this means focusing less on regions and more on the substantive features that a collection of countries shares. If this is not possible—and I believe that it sometimes is not (Pepinsky 2017)—then we need substantive engagement with regions *qua* regions.

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³ And indeed, one interpretation of the “area studies wars” of the 1990s was an argument that regional knowledge was subservient to comparative social science (see e.g., Bates 1996).

⁴ Or “Southeast Asia,” I dutifully insist.

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Symposium:

When Locals Say You're Wrong: Member Checking and Political Science

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[https://DOI: 10.5281/zenodo.3946811](https://doi.org/10.5281/zenodo.3946811)**Introduction**

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The practice of member-checking has become increasingly important in field-based political science research. We define member-checking as the process of discussing or sharing a part of research with the project's participants. The purpose is ostensibly to ensure the accuracy of what participants said and whether the researcher's inferences and arguments seem plausible to them. Implied in this definition is validity. Part of member-checking's importance stems from allegations of falsified data in some published research (Lubet 2018). In addition, the idea of replicability is also implied here. Rather than others attempting to replicate the study, however, researchers do so themselves to demonstrate that they acted in good faith. In short, if members confirm that we as researchers "got it right," it lends additional credibility to our work. There are other reasons to engage in the practice of member-checking. Doing so can act as a response to those who argue that fieldwork is a haphazard process (Kapiszewski, Maclean, and Read 2015). In this sense, member-checking can make our research appear more systematic. Furthermore, an invitation into the lives of research participants can create obligations and inequalities that researchers may feel responsible for addressing (Kapiszewski, Maclean, and Read 2015). Member-checking is one way in which researchers can give back to the communities that so generously share their experiences with scholars.

Given the benefits of member-checking, the idea that it is always the appropriate course of action and a necessary step in field-based research may seem self-evident. The purpose of this symposium is to demonstrate that it is in fact anything but straightforward. Indeed,

member-checking can pose several hazards. It can have unintended adverse consequences, including but not limited to, disruptions in an organization, interpersonal disagreements, and psychological or emotional distress (see Yanow, this issue). Researchers may also experience pressure from review boards to engage in member-checking as a stronger form of consent, which could lead researchers to portray respondents in a positive light even when the data show otherwise (see Schwartz-Shea, this issue). Finally, there is a marked difference between ensuring that facts are correct and ensuring that the meaning is correct. Verifying that the meaning is correct is a far more complex issue than confirming facts and thus must be treated in a critical manner (see Schwartz-Shea, this issue). These hazards demonstrate the importance of engaging member-checking critically, rather than assuming that it is always necessary or even desirable.

In addition, there are other reasons for a discussion of member-checking in qualitative methods. Member-checking is not a new practice—a read of the preface of James Scott's *Weapons of the Weak*, in which he describes performing his analysis, writing his study, and then returning to his field site to collect reaction and opinions clearly indicates member-checking, although he does not use the term (Scott 1985, xix). Recently, however, there has been an increase in participatory action research, community-based research, and community-based participatory research, resulting in a desire to view participants as more than "subjects" and "objects of research" (Orsini 2014). Next, there has been increased criticism of ethnography. Lubet's 2018

book *Interrogating Ethnography* largely stemmed from both concerns regarding the veracity of Alice Goffman's 2014 ethnography *On the Run* and praise for researchers like Matthew Desmond, who hired a fact checker for his 2016 study *Evicted*. In addition, some institutional review boards are beginning to require member-checking (Locke and Velamuri 2009). Finally, with ever-rising concerns regarding fake news, member-checking can act as an additional protection from spurious accusations that researchers have fabricated their data.

The essays in this symposium address the ontological, practical, and ethical issues that arise regarding member-checking. They represent a progression through the research process, first considering the definition of member-checking and what is at stake, and then how the process of member-checking might play out at various stages of field research, including asking the research question, first attempts at writing, and returning to the field multiple times. While the specific issues the researcher is facing at each stage may differ, each essay is motivated by a single guiding question: "What do I do?"

Dvora Yanow answers this question by providing a comprehensive breakdown of what is meant by the term "member-checking," and how researchers might engage in the process. It is not enough to simply share a portion of the research with participants. Rather, it is necessary to think critically about what researchers should share with participants, when they should share it, and with whom in the community they should share. Such consideration is necessary, she points out, due to methodological and ethical implications. Member-checking can result in making certain portions of research public, can embroil researchers in conflicts amongst participants, and can also raise unforeseen expectations. In this sense, researchers should take care to consider these matters before undertaking member-checking.

Peregrine Schwartz-Shea offers a different perspective by discussing whether member-checking is always the necessary, appropriate, or helpful course of action. Indeed, she warns researchers to avoid jumping to the conclusion that member-checking is always the best course of action. Her essay illustrates the complications at stake, pointing out that while "fact-checking" and ensuring the veracity of basic details is expected of all careful researchers, checking how participants make sense of what they have reported is another matter. When dealing with participant interpretations and statements that are not ontologically stable, there may be alternatives available to demonstrate the quality of

interpretive work. Member-checking, ultimately, should not be considered a default.

The next three essays examine member-checking quandaries that may arise during various aspects of the research process. Alyssa Maraj Grahame considers what happens when participants make objections known during the process of fieldwork. In other words, what should the researcher do when participants believe that the research *questions* are wrong? Her essay demonstrates that it is more fruitful to approach this issue as a new puzzle rather than as a validity problem. Probing the claim "you're asking the wrong question" presents an opportunity. In doing so, the researcher can better understand the broader context in which participants operate, delve more deeply into their political agendas, and delineate disagreements among participants. Rather than creating obstacles, examining the thoughts behind "you're asking the wrong question" can generate additional insights, making it a valuable part of the research process.

Allison Quatrini asks what the researcher should do when participants state that the research findings are wrong. Her essay considers member-checking while fieldwork is still in progress, although it also examines participants' reactions to the writing of initial findings. She argues that participant claims that the researcher "got it wrong" do not mean that the project is invalid. Instead, thinking critically about why participants might be making such claims is instructive. She provides four solutions that researchers should keep in mind when facing these issues. Researchers should actively consider the nature of politics, pointing out that the definition differs from context to context. Next, she suggests that participants who understand the discipline of political science in a way that is fundamentally different from the American context may also color participant views. Considering the differences among members is also important, as different backgrounds may account for the reasons why participants disagree with one another. Finally, researchers may encounter situations in which their observations on the ground do not conform to their original expectations. In this sense, participants may have a valid point when they state that researchers are "wrong," and some revisions may be necessary.

Finally, Nicholas Rush Smith's essay presents an additional conundrum that may emerge at a more advanced stage of articulating research findings. His is a special case of the question "what do I do?" as his contribution addresses what one does when members have passed on, and there is no one with whom to "check

back.” He writes that in cases such as these, verifying facts is less important than learning more about local politics. In addition, he points out the ethical issues that are raised, suggesting that since the dead cannot speak for themselves, representing them with empathy is important for researchers. Doing so provides rich context to allow a better understanding of participants’ worlds and the choices they made, despite the discomfort they cause.

Taken as a whole, the contributions to this symposium offer distinctive critical reflections on the possibilities and limitations of employing member-checking as a standard practice in political science. Moreover, while member-checking usually implies that the researcher engages in the procedure after the conclusion of field research and articulation of the project’s findings, the

symposium’s essays examine how member-checking might work in surprising ways at different stages of the research process. They also provide practical insight into how field researchers can navigate tensions between locals’ and researchers’ understandings of the political phenomena under investigation. Finally, member-checking has been gaining ground among interpretive and even some qualitative researchers, and Schwartz-Shea (2014) demonstrates in an analysis of methods textbooks that member-checking is an appropriate way to assess the quality of a study. In this sense, both positivists and interpretivists alike will find something of value here. It is our hope that this symposium will generate debate and discussion regarding this key methodological topic.

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“My Participants Told Me I Got It Wrong. Now What Do I Do?”

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The question which the title poses was asked by Allison Quatrini at the 2016 “Textual Analysis and Critical Semiotics” APSA Short Course. In methods terms, one answer to it might be “member-checking,” discussed here in its contemporary understanding as an activity carried out at some point after a research encounter (an interview, an interaction, an observation) is completed in which the researcher “checks” with the situational member about the former’s understanding of the latter’s words, experiences, or both. It is understood as a strategy to optimize the descriptive,

interpretive, or theoretical validity of qualitative research findings (Sandelowski 2008). Given the qualitative or interpretivist methodological goal of understanding the lived experiences and lifeworlds of research participants, the idea has intuitive appeal. Why not “check back” with those studied to assess one’s understanding of what they’ve said or done? The method has increasingly been adopted among interpretive and some qualitative researchers conducting interviews and participant-observer/ethnographic field research. Indeed, Schwartz-Shea’s (2014) analysis of methods textbooks shows that

member-checking has become an accepted indicator of the quality of a research project—that it follows expected standards for particular research methods. Researchers can use it to demonstrate that their manuscript meets methods criteria; reviewers can use it to assess whether, indeed, it does. Having and knowing such “standardized technical procedures,” as Elliot Eisner (1979, 73) pointed out, provides “[o]ne of the sources of intellectual security for doctoral students as well as for professors.”

Yet the “why not?” question hides much. Member-checking is not an unmitigated good: its conceptualization warrants critical assessment, rather than blanket endorsement. Following a brief history of the concept’s use and a working definition, this essay discusses what needs to be problematized in its treatment, including things often overlooked in methods texts’ discussions. Focusing on what “checking” means, these include:

- What, precisely, is to be sent back: A quotation? An interview transcript? A portion of a manuscript? The entire manuscript?
- When in the course of a research project should that be sent—immediately after an interview? When a draft manuscript is finished? On publication?
- To whom should materials be sent—all participants? Some? Which ones?

Answers to these questions raise potential ethical questions, including concerning handling feedback. Exploring these matters reveals the methodological presuppositions underlying the language of “member-checking” and some of its practices: the presumption of a single correct truth and of who possesses it. Whereas these presuppositions may not be problematic for positivist-informed qualitative research, they do raise challenges for interpretive research. Gaining clarity on the practices is important for both methodological approaches.

What is “Member-Checking”?

Member-checking refers to the practice of communicating some aspect of one’s research to one or more of the persons among whom that research was conducted. Which aspect and which persons are discussed below.

The practice seems to have been enacted some three decades (at least) before it was named. In his widely read methods appendix to the second edition of *Street Corner*

Society, a three-and-a-half-year-long participant-observer study of a neighborhood in “Cornerville” (Boston’s North End), William Foote Whyte writes about sharing his thinking with “Doc,” one of the “corner boys”: “Much of our time was spent in this discussion of ideas and observations, so that Doc became, in a very real sense, a collaborator in the research” (Whyte 1955, 301). But Doc’s involvement also included reading the manuscript: “...we had long conversations over his suggestions and criticism.” And Whyte “also had innumerable feedback discussions with Sam Franco”—the settlement house director who read his study of the Nortons, the gang of boys Doc led who hung out on Norton Street—“before hiring Doc to direct a storefront recreation center” (Whyte 1993, 289).

The concept was apparently formalized, however, only toward the late 1970s. Writing in 1981, Egon Guba noted its epistemological focus: “In establishing truth value, then, naturalistic inquirers are most concerned with testing the credibility of their findings and interpretations with the various sources (audiences or groups) from which data were drawn. The testing of credibility is often referred to as doing ‘member checks,’ that is, testing the data with members of the relevant human data source groups” (Guba 1981, 80). He elaborated: In member checks,

data and interpretations are *continuously tested as they are derived* with members of the various audiences and groups from which data are solicited. The process of member checks is the single most important action inquirers can take, for it goes to the heart of the credibility criterion. Inquirers ought to be able to document both having made such checks as well as the ways in which the inquiry was altered (emerged or unfolded) as a result of member feedback. (Guba 1981, 83; italics added)

Note Guba’s original conceptualization of “member checks” and “member feedback” as the *ongoing, fieldwork-based* testing of the researcher’s understanding.

Following its discussion by Lincoln and Guba (1985), the concept was taken up more widely by qualitative researchers in sociology and other fields beyond the educational evaluation community in which it originated. By the time of that publication, however, Guba’s initial conceptualization had shifted to the act of sending the researcher’s draft case study back to the “respondents at the case site(s)” in order to “test its credibility” (Lincoln and Guba 1985, 373).¹ Still, in both treatments, member-

¹ Schwartz-Shea (this issue) explicitly distinguishes between these two understandings of member-checking.

checking was explicitly intended as a way of addressing the question: Did I get “it” right? Guba and Lincoln (1985), Erlandson et al. (1993), and Miles and Huberman (1994) all positioned member-checking as a test on credibility, the latter still using the language of getting “feedback from informants.”

These treatments were formulated in the face of encroaching 1970s behavior(al)ism and its realist-objectivist bases. In positioning member-checking as lying at “the heart” of research and researcher credibility, these methodologists were seeking to explicate the scientific character of qualitative methods, especially vis à vis a researcher’s possible “bias,” a key topic of debate at the time. Attacks focused on qualitative researchers’ seeming lack of objectivity and, hence, the questionable trustworthiness of their “findings.” Defenses were mounted by some of the leading qualitative methodologists of the day, including Donald Campbell, Elliot Eisner, and Egon Guba. In his 1978 monograph we can see Guba working out the arguments that appeared in subsequent publications, including the initial framing of what became member-checking. For example, concerning “establishing credibility of findings,” he wrote: “Since so much of naturalistic inquiry depends upon the perceptions of informants, it is essential that they find the data and inference of a naturalistic study credible and persuasive” (Guba 1978, 65). Note that his assumed audience is neither manuscript reviewers nor other researchers, but instead the participants in the educational programs he is evaluating—the educators and other professionals who were potentially his readers as well as his “informants.”

Guba (1978, 65) quotes Eisner, in a work then in press, calling the approach “multiplicative corroboration—the use of...peers to pass judgment on what has been structurally corroborated.” And he quotes Campbell, in an article also at the time in press, proposing a similar method, called “participant evaluation,” using participants to provide credibility checks:

‘Participants...will usually have a better observational position than will [...] outside observers of a new program. They usually have experienced the preprogram conditions [...such that] [t]heir experience of the program will have been more relevant, direct and valid, less vicarious [than the researcher’s]. Collectively, their greater numerosity will average out observer idiosyncrasies that might dominate the report of any one [researcher].’ (Campbell, quoted in Guba 1978, 66)

Guba continues: “Assurance of credibility of the final result of a naturalistic inquiry is probably best obtained through frequent and thorough interaction with informants as the information develops. In this fashion information with limited credibility can be identified early and either eliminated or buttressed” (1978, 66). He adds that this might be thought to expose the researcher “to untoward influences,” but that such exposure might be safeguarded through the use of other methods listed previously in the monograph. And he concludes, in words echoed in his later writings,

It is likely that the criterion of respondent credibility is the single most important judgment that can be brought to bear on a naturalistic inquiry. Without it one can have no sense that the findings and inferences have any reality, particularly since so much depends upon the perceptions of people. With it, except in the case of a general conspiracy to mislead the investigator, one can be reasonably sure that the findings do reflect the insights and judgments of a large group of people coming from different perspectives. (Guba 1978, 66)

Problematizing the Concept

Guba in 1978 is concerned with the credibility of respondents. By the mid-1980s, concern about the trustworthiness of the researcher’s “findings” had shifted to researchers’ presentations of individuals’ views in the written manuscript. Member-checking was now treated as a control on that. Each of its two components—“member” and “checking”—calls for critical examination, as does the researcher’s response to feedback received. These are often not engaged in treatments of the concept in the methods literature (Locke and Velamuri, 2009, excepted). As Nicholas Rush Smith (this issue) explores various aspects of what it means to be a “member,” I will focus on what checking entails, taking up the question of member identity in that context. My own ethical baseline for this discussion—a concern missing in the methods literature—is that *one should not even engage the prospect of sending something to a situational member to “check” unless one is prepared to take the response seriously*. This means dealing with it in some fashion, at a minimum thanking the individual who makes the time to read the item and comment on it, whatever the tone of the response. Beyond that, the response might be discussed in one’s research manuscript, which I take up below.

What Should a Researcher Send Back?

Guba's initial treatments of member-checking in the context of educational evaluation reports make clear that what is to be sent back is a draft case study. As the concept came into wider use, however, this delimited object shifted in scope. Taken up widely in interview research, it came to mean the transcript of a single interview, whether of a recording or of the researcher's notes, to be sent back to the person interviewed who would be asked to corroborate the written text. Beyond that, including in participant-observation and ethnography, the boundedness of the object became even fuzzier. Should one send a portion of a manuscript in which an individual had been quoted—or send the single quote only? What about passages reporting paraphrased conversations, rather than direct quotes? Should one send entire paper or article drafts? Book-length manuscripts? What about descriptions—for example, of events, acts or interactions? Rather than providing definitive answers, I intend these questions to provoke critical reflection.

To Whom Should Material be Sent?

Determining what to send interacts with the identity or role of the intended recipient. Again, matters are clearest when it comes to interview research: a transcript or summarizing notes could be sent to the person interviewed. The requested action is also contained: the “member” is asked to respond to the text's accuracy. Indeed, it could be unethical to send it to anyone else, something to which I return.

But as the written material expands in scope beyond spoken words, the range of intended recipients also grows. Consider a paper, article or book chapter draft, which includes not only direct quotes but also paraphrased material. Even if the work focuses on a single actor (e.g., Mintzberg 1970; Wolcott 1973; Behar 1993), the researcher is likely to have spoken with others in the field setting, at times at length. Should the manuscript be sent to all of them? And field research manuscripts of whatever length are also likely to include observational data—of settings, events, acts, interactions, and so on. Should these also be included in member-checking?

In her critical assessment of *Street Corner Society*, for instance, Boelen (1992, 33-34) asked whether Whyte had “commit[ted] an ethical cardinal sin by not taking his manuscript back to the field and checking the data and contents with the subjects.” Whyte—who reported having discussed his observations extensively with “Doc”—replied, “At the time of my study, I had never heard of such an obligation” (Whyte 1993, 289). In

much of his subsequent work, he noted, he did discuss findings and interpretations with participants. He also shifted to doing participatory action research, which he found useful for “getting the facts straight,” among other things (Whyte 1989, 381). Then he took up the matter of implementing Boelen's idea:

How does one feed back the data and contents of such a study to a community of 20,000 people—or even to the parts of the community I focused on? Should I have fed back my findings on the social ranking and leadership pattern to the Nortons, as a group? When I once asked them who their leader was, they stated they were all equal. To reveal to them that behaviorally they were not equal would have embarrassed Doc and upset his followers. (Whyte 1993, 289)

Such upset did, in fact, take place, but years later, at someone else's hand.

When in the Course of the Research Should Something be Sent?

The scope of the material can determine the timing of its transmission. An interview transcript may be sent back immediately after transcribing a recording or notes. With more material, however, the timing is less clear-cut. Draft papers, articles, chapters, and book-length manuscripts are usually completed after many conversations and interviews have been conducted. Often, then, more time elapses between the interaction and the sending. Here is where problems of two sorts arise.

One concerns memory. Researchers have tapes or contemporaneous notes; participants rarely do. Aside from lapses of memory, social, political, organizational, or personal circumstances may have changed such that what had been said months earlier seems no longer tenable and individuals “cannot believe” they actually said what they are quoted or paraphrased as saying (especially in light of intervening events) or regret or do not recollect their previously-held views.

A second arises from presenting spoken material (whether from formal interviews or less formal conversations, depending on research design) drawn from more than one source. Longer manuscripts may also include descriptions of the researcher's observations of research settings, events (such as meetings), acts, interactions, and so on. The further along one is in deskwork and textwork processes (Yanow 2000), the more the writing has likely incorporated ideas informed

by academic concepts and theoretical literatures. The resulting juxtapositions may render individuals' words in an entirely different light than they had imagined—including analytic arguments that do not comport with their own sense of settings, persons, or events.

Related to both of these, there is the phenomenon of seeing one's words in print, something people interviewed by journalists also experience: even when the quote or paraphrase is accurate, seeing one's words in print may frame them in a new light. If they are excerpted from a longer statement and juxtaposed with others' words or with the researcher's analytic comments, they may appear to the speaker as having been "taken out of context"—a

phrase commonly used to signal the speaker's sense that the words are being used (twisted?) to make the writer's point, rather than the speaker's.

With all manuscripts, researchers might choose to wait until publication to share them with members. This is the ultimate way of controlling speakers' responses to seeing their words in print, as it leaves the researcher with a diminished ability to engage those responses. In fact, in this symposium, Schwartz-Shea rules out such "sharing" as a legitimate form of member-checking.

Table 1 summarizes the discussion, moving from lesser to greater researcher control over the scope of possible responses.

Table 1. Implementing Member-Checking: What, to Whom, When?

What to share?	With whom?	When?
Transcript or notes from a single interview, with direct quotes	Person spoken with or interviewed	During deskwork/textwork <ul style="list-style-type: none"> • after transcription • prior to or during analysis • while analyzing or writing
Interview summary notes, with paraphrased material	Person spoken with or interviewed	During deskwork/textwork <ul style="list-style-type: none"> • after transcription • prior to or during analysis • while analyzing or writing
Excerpt from paper, article or chapter draft including direct quotes and paraphrased material, plus descriptions of settings, events, and analysis	Person(s) whose words are presented Person(s) involved in settings, events, acts, interactions described	During deskwork/textwork <ul style="list-style-type: none"> • after transcription • after drafting analysis • after drafting excerpt
Full paper, article, chapter or book manuscript draft including direct quotes, paraphrased material, and observations	Person(s) whose words are presented Person(s) involved in settings, events, acts, interactions described "Gatekeeper(s)"	During deskwork/textwork when draft is completed
Published manuscript	Person(s) whose words are presented Person(s) involved in settings, events, acts, interactions described "Gatekeeper(s)"	Part of textwork dissemination

Reacting to Responses

When member checking is carried out... [and] written about,[the encounters] are often portrayed as conflict-free, and the “benevolent” image of the researcher who shares the final work with participants is reinforced. (Caretta and Pérez 2019, 361)²

If they read the item sent to them for comment, readers’ reactions can range from indifference to outrage. How should the researcher react to comments that say, “You got it wrong!”? What if different participants give divergent responses? I know of no definitive set of answers to that situation. Here are some possible ways to think about it.

First, as we are, after all, as human as anyone else, researchers should double-check their text and notes. If one discovers an error, corrections are in order—along with thanks to the person who caught it, perhaps adding an acknowledgement in the manuscript.

But what if the researcher is convinced he didn’t “get it wrong”? Having an audio- or videotape or stills of an interview or event makes handling this situation easier, as the researcher can then send “proof” of the quoted text. Detailed interview or field notes might also be persuasive. Having no notes but only one’s memory becomes problematic for arguing for one’s view of what transpired. I know of no easy solution for this interpersonal uneasiness. However, this formulation of the situation suggests a world in which the point of the exchange is to verify spoken language (or observed acts), rather than to assess the broader gestalt of the situation—including what the researcher learned from other parts of the conversation or observed event(s), other conversations or acts at other times, and words or deeds articulated or committed by other situational actors. Here, one is on somewhat firmer, though not necessarily easier, ground, especially if it is not the “facts” of the situation that are in question but, instead, the analysis. What individual members often do not take into account is that researchers commonly have access to other interlocutors and that the written material—if it is more than an interview transcript—may also reflect the views learned from those persons. That may explain why what the member is reading does not comport with that member’s views. This can be pointed out—which might lead to a prolonged back-and-forth over what constitutes “the truth” of the situation. This exchange may generate

additional data and new insights into the research topic, which may become part of a revised manuscript. Here is also where having promised confidentiality to all respondents can be brought to bear (against pressure to answer the question, “But who said that?!” and, if need be, pointing out that the same promise extends to this member vis à vis others). Explaining that the analysis also reflects debates in the researcher’s theoretical community, leading to other views than those of the situational member, may or may not be persuasive, depending on the interests of the protesting or complaining member.

And then there is the matter of handling comments “logistically” in a revised draft. I have seen these treated in three ways. One buries the dispute in an endnote or footnote—as if hoping the problem will disappear. Another draws the contested view into the text, engaging the differences substantively. This move may treat the disagreement as new “evidence,” serving potentially as the basis for additional analysis, as mentioned above. A third ignores the dispute altogether.³ If one claims in one’s methods section, however, to have done member-checking, a reader might reasonably expect to know how it was conducted, with what results, and if a dispute ensued, how it was engaged, and where in the manuscript. Ignoring the response is, then, not a practical action, quite aside from the ethics of inviting people to respond and then ignoring their replies or of using contested information without discussing the dispute. Table 2 summarizes possible member reactions and researcher responses.

My intention is to note these moves in the hope of sparking reflection and discussion, not to endorse one over another; as others may be possible. Here, for example, James C. Scott (1985, xix) brings other dimensions to bear, writing in the Preface:

This book is...more the product of its subjects than most village studies. When I began research, my idea was to develop my analysis, write the study, and then return to the village to collect the reactions, opinions, and criticisms of villagers to a short oral version of my findings. These reactions would then comprise the final chapter—a kind of “villagers talk back” section or, if you like, “reviews” of the book by those who should know. I did in fact spend the better part of the last two months in Sedaka collecting such opinions from most

² Caretta and Pérez are concerned with using member-checking to increase participants’ involvement in research, which adds other dimensions not engaged here.

³ This third move has come to light in conference corridor chats and seminar discussions with students. After all, as editor Jennifer Cyr asks, how would a reader know if it were done?

Table 2. Responding to the “Member’s” Reaction

Member reaction (Note: These are not direct quotes.)	Researcher response
Any	Thank you (for your time, your effort, ...) Possible acknowledgement in the final manuscript
“You took my words out of context; what I really said/meant was...”	Check notes, recording: Did I get it “wrong”? <ul style="list-style-type: none"> • Yes? Revise (and send acknowledgement) • No? <ul style="list-style-type: none"> ○ Write back, including evidence from notes or tape ○ Ask for follow-up visit for further discussion; bring evidence ○ Include “dispute” in text and discuss ○ Include “dispute” in a footnote or endnote ○ Ignore
“I’m going to prevent you from publishing...” “If you publish that, you will never do research here again!”	See Mosse (2005, 2006) ³ Modify one’s text (see Schwartz-Shea, this issue; Caretta and Pérez 2019, 367-68)

villagers. Amidst a variety of comments—often reflecting the speaker’s class—were a host of insightful criticisms, corrections, and suggestions of issues I had missed. All of this changed the analysis but presented a problem. Should I subject the reader to the earlier and stupider version of my analysis and only at the end spring the insights the villagers had brought forward? This was my first thought, but as I wrote I found it impossible to write as if I did not know what I now knew, so I gradually smuggled all those insights into my own analysis. The result is to understate the extent to which the villagers of Sedaka were responsible for the analysis as well as raw material of the study and to make what was a complex conversation seem more like a soliloquy.

To clarify the issues raised by researcher-initiated member-checking, whatever its form, consider the circumstance in which someone other than the researcher brings the published findings back to the persons among whom the research was conducted. Ellis’ 1986 study provides one example. The sociology professor who had introduced him as an undergrad to her research setting

brought her book—of whose publication members were unaware—with him on a visit to one community. He read them key passages; several residents were infuriated by the descriptions of themselves and their family members (Ellis 1995). In another example, Boelen (1992) relates revisiting Whyte’s “Cornerville” twenty-five times over nineteen years, thirty to forty-five years after he concluded the research (Whyte 1993, 285). In stays of up to three months, she tracked down “Doc’s” sons and members of other “gangs” and told them about the book. Many claimed not to have known about it, contradicting Whyte’s own narrative. Boelen discusses various aspects of his account with them; they confirm some, refuting others.⁴ Neither of these examples would be considered “member-checking” as that has been defined and enacted historically.⁵ Including them in a critical discussion of the method, however, might help make explicit not only the potential of such checking and its limitations, but also its ethical ramifications.

Ethical and Methodological Issues in Checking with Members

Sending things back may have unanticipated and unintended consequences for others. Consider the

4 Because so many have picked up on Boelen’s critique of Whyte, it is worth noting that her reconstruction of events thirty to forty-five years later is not unproblematic, as are her assumptions concerning life in Italy, on which she based on her own lived experience there, seemingly as an adolescent. Boelen’s critique appears in a symposium “*Street Corner Society, Revisited*” in the *Journal of Contemporary Ethnography*, including responses from Angelo Ralph Orlandella (“Sam Franco” in the book) and Whyte. Arthur Vidich’s essay there (1992) is especially useful in thinking about member-checking.

5 Buroway (2009, 99-100) treats Boelen’s dissection of Whyte’s research as a type of “ethnographic revisit,” which might be considered a different form of checking on one’s interpretations when it is the researcher doing the revisiting.

case in which permission to access the setting—say, an organization—was granted by the Executive Director. It would be logical, then, to send material back to that individual. Supposing the research had been conducted among that person’s subordinates and that the manuscript includes analyses of their acts alongside critical assessments (theirs or the researcher’s) of the Executive Director. What pressures might that individual bring to bear on the researcher to reveal subordinates’ identities? What might be the consequences for those subordinates of having their views made public—or for the Director?⁶ In other words, the power dimensions at play in research settings may enfold the researcher, unfurling in unanticipated ways. Considering field research as an intervention in the setting under study may bring these relationships and consequences to the fore. This view of research is an alternate to the earlier concerns out of which member-checking grew, which sought to minimize the impact of the researcher on the setting. It is more in keeping with interpretive approaches in particular, which increasingly emphasize the relational character of research (see, e.g., Fujii 2018 in the context of interview research).

Some researchers, irrespective of methodological bent, consider member-checking a way to give something back to the people among whom they conducted their research. Several raised this point, for example, at a day-long mini-conference on political ethnography at the 2017 French Association of Political Science meeting, and it surfaces in discussions with US and other researchers.⁷ Field researchers’ feeling that they “need” to give something back derives from the sense of having benefited from participants’ metaphoric gifts (of their time, hospitality, and so on), leaving an “imbalance” in the relationship which needs to be righted (much like the potlatch of the Northwest Pacific Indigenous peoples [see, e.g., Kan 1989] or US Christmas card list-keeping are intended to achieve). Some fields of inquiry—sociology, for instance (see Walby 2010, 643)—and some research designs—notably experiments—compensate participants financially or otherwise. For some researchers who do not pay participants, sending back a transcript or a draft manuscript feels like it rights the imbalance of indebtedness.

A version of this feeling of wanting to “pay” participants back emerges in organizational studies field research. Organizational members often approach researchers after the latter have been hanging around for some weeks, asking when they are going to share their “findings”—and researchers feel the pressure to comply as a way of repaying a social or informational debt. (Indeed, such requests often catch PhD students unawares.) Aside from the power dimensions, this scenario raises ethical concerns regarding a researcher’s making information public in the absence of either a literal or a social contract supporting such revelations.⁸ Additionally, as Schein (1999) notes, most researchers—lacking training as consultants—do not have the professional wherewithal to deal with the unintended emotional or psychological consequences for situational members of these sorts of interventions, not to mention for themselves. What might appear as “simple” member-checking, then, may have serious consequences, including internal organizational disruptions, demotions, or firings, as well as individual distress and interpersonal strife.

These sorts of reactions take place, too, in other than organizational settings and in different forms. Mosse’s experience is a key example (see n. 4). Whereas his may be an extreme case of readers’ responses, it suggests a caution: some forms of member-checking might raise expectations (as inappropriate as these might be) that the researcher will refrain from critically assessing the social practices and institutions in which participants are embedded. (For further discussion of this point, see Schwartz-Shea, in this symposium.)

Concluding Reflections

The more we poke at the character of member-checking as presented in methods textbooks, the clearer the underlying methodological presuppositions become. The concept presumes that social realities are singular: there is one “truth” of events, acts, and so forth, which the researcher is working to unearth. In this singularity, the member’s “truth” trumps that of the researcher. Member-checking privileges the member’s account of what was said or of what transpired, ignoring other dimensions of social scientific research. Central among these is the fact that the researcher may know things that the member in question does not, having cast a wide research net in

6 As I wrote this, news and other media were filled with stories of pressures being brought on White House staffers to reveal the identities of Anonymous (2018), the author of a critical op-ed in the *New York Times*, and those quoted in Bob Woodward’s just-published *Fear*. As I revise it over a year later, Anonymous is back in the news, now with a book (2019), and those pressures have resumed.

7 For a wide range of thinking on the topic, see Brettell (1993) and Gupta and Kelly (2014).

8 That is, researchers may have formal contracts with “gatekeepers” who granted them access, but that does not necessarily accord them ethical or other permission to reveal what they learned from those individuals’ subordinates.

order to inquire at various levels or arenas of the setting being researched, so that various viewpoints at play in the field setting are brought to bear on the research question (a political science and organizational studies research practice that anthropologist George Marcus, 1995, captured in the phrase “multi-sited ethnography”). These multiple points of view—multiple “truths”—are then reflected in the research writing, such that any single member’s view(s) would be considered alongside others’. Additionally, researchers are engaged in conversation with particular literatures and their theories and ideas, which may contribute additional theoretical insights to the study and analysis and which may challenge local “truths.” On this point, too, Whyte’s comment is instructive:

Note that Boelen [in her critique of his Cornerville study] deals with field relations only in terms of the researcher’s presumed obligations to those studied. She does not consider the right of the researcher to publish conclusions and interpretations as he or she sees them. How to balance our obligations to those we study against our rights as authors to publish our findings is a complex question that cannot be answered by dealing only with our obligations to informants. (1993, 289)

In the end, the concept of member-checking is too slender a reed on which to hang the complexities of studying and interpreting the multiple truths that characterize social realities, which may emerge in the

course of field research. As a hoped-for magic potion to eliminate researcher “bias,” member-checking has failed. Today, not only is the relational character of research on the table, but so are the ways in which writing constructs readers’ knowledge of the settings, persons, and events or interactions being presented (see, e.g., Marcus and Fischer 1986) and, hence, the researcher’s responsibility for the form and character of that writing (Ellis 1995). The challenge, then, is to develop more robust ways to engage the scientific character of field research encounters and their interpretation, in an ethical fashion.

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“Member-Checking”: Not a Panacea, Sometimes a Quagmire

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I first encountered “member-checking” circa 2004 while writing a chapter about what sorts of criteria might be appropriate for assessing the quality of interpretive empirical studies. In the 2014 revision I wrote:

“Informant feedback” and “member checks”...answer the questions “How do you know that your study’s ‘representations’ are recognizable by the people you studied?” and “How does the reader know that ‘these words,’ ‘these views,’ are theirs, rather than yours?” Informant feedback/member checks are specific ways that researchers test their own meaning making by going back to, and asking for feedback from, those studied for an assessment of whether the researcher has “got it right”—that is, has understood the experiences of those studied on, and in, their own terms (Schwartz-Shea 2014, 135).

In the three paragraphs that followed, I analyzed the impulse behind the technique and the reasons why its use might be problematic. I concluded that its use by interpretive researchers “should not be mistakenly understood as implying an objective, external truth about actors’ experiences—for that would be inconsistent with interpretive presuppositions that posit the need to understand meaning as situated, historical, and constructed” (136).

That introductory overview still rings true to me although, of course, there is much more to say about the practices of member-checking in interpretive work—hence this symposium. As well, much has changed in the discipline of political science in the nearly two decades since I first encountered the term, and particularly since 2014 with the rise of the data sharing and archiving, “transparency” movements, and the recent, high-profile challenges to Alice Goffman’s (2014) ethnography *On the*

Rim. Nor are our research practices entirely walled off from the explosive accusations and rumor mongering of the internet age in which debates about truth and facts are now the stuff of everyday discussion.

What is worrisome about this new disciplinary and political milieu is the possibility that the many reasons *not* to use member-checking may get lost because of the rhetorical allure of the phrase: “member-checking” sounds simple and common sensical, drawing much of its power from the analogous phrase “fact-checking,” practiced in particular these days by journalists who assess politicians’ formal speeches and everyday social media “talk.” It also resonates with everyday practices of “double-checking,” such as the time and location of an appointment or, for scholars, whether a quotation has been accurately transcribed. The “checking” appeals as something that careful workers and thinkers would do; checking with “members” may also seem ethically desirable because it implies respect for their knowledge of their own lives. Yet the apparent simplicity and attractiveness of the phrase are belied by the numerous complications that ensue when implemented—from differentiating its use from other practices to the bigger question of its all-too-often unexamined philosophical underpinnings. As I wrote in 2014, the term can be understood from within an interpretive orientation, but it also has realist-objectivist origins (see Yanow, this issue).

In this essay, I examine this array of complications for the purpose of educating readers, comparing it to other practices that might be preferable. As important, I warn against adopting any universal mandates for member-checking. Those who choose not to member-check (like those who refuse to archive data) are not ethically suspect or deficient scholars and should not be deemed to be such. They are quite likely to have reasons *particular to their chosen projects* for their choice not to engage in this practice.

¹ My thanks to all the members of the 2017 APSA roundtable, “Member-Checking: ‘My Participants Told Me I Got It Wrong. Now What Do I Do?’” for their stimulating remarks. My thanks, also, to Dvora Yanow for incisive feedback on previous drafts. I appreciate her “checking” of my arguments. Any errors remain mine.

Differentiating Member-Checking from Other Practices

What constitutes “member-checking?” Most generally, it means sharing some portion of a written text, after the initial data generation stage but before publication, with some of those studied and inviting various forms of feedback. The practice gives the author the opportunity to modify the manuscript based on members’ feedback *prior to its publication*.² For the researcher, this practice entails a number of decisions: what to send, to whom, when, and how to handle the feedback, as Yanow’s symposium essay examines in detail. On the first of these, Locke and Velamuri (2009) have categorized the extent of sharing along a continuum, from “restricted” (showing individuals only their own data) to “selective” (showing individuals descriptive accounts or characterizations pertaining to them) to “comprehensive” (sharing most, if not all, of the manuscript) (493).

Because of potential overlap with other research practices, it is necessary to identify what member-checking is *not*. First, the definition does not include activities at the *initial* data generation stage. For example, while interviewing, researchers may say to interviewees, “If I’m understanding what you’re saying...” or, “Let me recap my understanding of what you’ve said and let me know what I’m missing or misunderstanding.” Both of these phrases invite immediate feedback in the moment (as compared to the later sharing of, say, the researcher’s write up of an interview, or characterization of an event witnessed by the individual). Similarly, during a participant observation study, the researcher might compare one member’s ideas against another’s (as when one “maps” the field, expecting variation in how people understand what they are doing or what is going on). Or, the researcher might compare her own experience of an activity with that of those beings studied. These are typical ways of generating evidence in the field that can be used for immediate follow-up with site members. In contrast, the sharing of the researcher’s written text, particularly the selective or comprehensive versions of member-checking, potentially impinges on the researcher’s epistemic authority as the one responsible for the text’s representations, analyses, and findings.

Second, the definition of member-checking does not include what members might say *after* the study is published in book reviews, blogs or op-eds. Clearly, what members say may not always be positive. As one example, Scheper-Hughes’ 1979 *Saints, Scholars and Schizophrenics: Mental Illness in Rural Ireland* was pilloried in the Irish press as a violation of members’ privacy. After a return visit, Scheper-Hughes wrote explicitly about her attempt to reconcile an honest ethnographic account with respect for her study participants. Yet one told her: “You wrote a book to please yourself at our expense. *You ran us down, girl, you ran us down*” (Scheper-Hughes 2000, 119; original emphasis). Member-checking *before* the initial publication would have given Scheper-Hughes the opportunity to consider member perspectives and make changes in her manuscript, thereby possibly changing the character of that post-publication debate. As the internet increases the likelihood that members will read what is written about them, clarifying that member-checking occurs before publication becomes even more important.

Finally, member-checking is narrower than what Cooper (2008) calls “data sharing,” anthropologists’ practices of and experiences with sharing data with research participants both during and after publication. Some of the practices he describes might be understood as member-checking, but he does not clearly distinguish between generating data in the field, writing-up data prior to publication (the definitional arena of member-checking), and sharing publications with participants. As well, he omits the overlap between “data sharing” and “data archiving.”

Voluntary or Mandated?

Member-checking can be a voluntarily chosen activity, negotiated as part of access to a research site, or mandated by a university’s “human subjects protection” board³ during the prior review of a proposed project. (I take up voluntary member-checking below.) When member-checking is a condition of access to a site or organization, gatekeepers can demand that they see a portion of or the entire manuscript, thereby retaining their power to request or require particular changes or redactions before publication. Such negotiations involve complex questions of legality and ethics that researchers should seriously consider because of the potentially

2 Some extend the concept of member-checking to include reactions after publication. My thanks to Dvora Yanow for this point. As I argue below, I see that extension as inconsistent with a primary purpose of member-checking—receiving member feedback to challenge the author’s draft so that, subsequently, she can incorporate changes into the manuscript before publication.

3 These include Institutional Review Boards (IRBs) in the US, Ethics Review Boards in Canada, Human Research Ethics Committees in Australia, and others by different names elsewhere.

significant impact on the published representations of their knowledge claims. (On negotiating access, see Bondy 2013.)

The extent to which US ethics review boards are requiring member-checking is not clear because of the decentralized implementation of the ethics review system (Yanow and Schwartz-Shea 2016). Locke and Velamuri (2009), however, report that for Velamuri's dissertation, his university's ethics review board mandated member-checking as a way to enact a robust form of consent. Board members' thinking was that by "securing consent at the end point as well as the beginning of the research, *participants* would understand exactly what they were consenting to" (Locke and Velamuri, 2009, 500; emphasis added). This additional layer of consent—required not by research-setting gatekeepers but by a university entity—is troublesome. It implies not only knowledge of, and agreement to, research purposes and procedures, but also a favorable "outcome," (i.e., that researchers will represent participants or their community or organization favorably). It adds additional pressure, beyond that of gatekeepers, on researchers to slant their findings—to pull their punches, so to speak, as Scheper-Hughes might have been motivated to do had she engaged in member-checking. But requiring informed consent as to purpose and procedure should not mean that researchers' knowledge claims must be *liked* by participants.⁴ It is as if, by analogy, medical research participants would have to see the outcome of an experimental medical procedure in order for their initial consent to be considered genuine.⁵

Motivations for Member-Checking

Member-checking may be motivated by distinct purposes, which should shape researchers' decisions about what to share and with whom. First, member-checking can be a means of improving anonymization by giving selected portions of a manuscript to a member to check (or assess) whether, for example, a particular snippet, if published as is, might reveal her identity

to others, particularly those within the research site. Using Locke and Velamuri's (2009) terminology, this purpose could be accomplished through "restricted" member-checking, in which the researcher checks on specific quotations (or other text) with the member who produced them. Dealing with the feedback would be, comparatively, straightforward—deleting the phrase or changing it in ways that would better disguise the member's identity. However, the effectiveness of this strategy is uncertain, as members may not be aware that the colloquial expressions they commonly use could reveal their identities to others. As important, there are other means of identity protection (such as masking identities⁶ or using composite characters), the effectiveness of which should be compared to member-checking before choosing that option.

Second, some have argued that member-checking will improve the quality of a research manuscript by enabling members' critical assessment, as Velamuri argues (Locke and Velamuri 2009, 499). Newton (1997) agrees, quoting Shokeid: "to the extent that the anthropologist's labor carries authority in representing another reality, it must also stand the test of its subjects" (641). For this purpose, member-checking would likely need to be on the extensive end of Locke and Velamuri's continuum, involving either "selective" or "comprehensive" sharing. Although this rationale may seem compelling, it glosses over significant issues, most prominently the problem that members may, and likely will, differ in their assessments of whether the author's characterization of their realities is "truthful."⁷ The possibility of contending assessments complicates how the researcher deals with them in a revision. Specifically, on what grounds is one member's feedback to be preferred over another's (and, also, over the author's own initial views)? As important, there are many ways to demonstrate the trustworthiness of a research manuscript (Schwartz-Shea and Yanow 2009). These alternatives, such as thick description, should be compared to the actualities of member-checking, rather

4 Access and consent can be bound up with the extent to which researchers promise that the identity of research participants, organizations, and/or communities will not be revealed during the research process and upon publication. For discussion of the complexity of anonymity in ethnography, see van den Hoonaard, 2003. For purposes of space, I do not take up that complication here, but if research participants' identities can be protected, *mandates* that researchers give them an opportunity to comment on a researcher's written draft are particularly problematic for reasons ranging from the integrity of the research contribution to the academic freedom of researchers.

5 Mandated data sharing has been termed an ethical imperative by medical ethicists. For a critique of this position, see Cooper (2007). The extent to which "data sharing" and "member-checking" overlap is not clear.

6 Masking refers to changing details in a published study that are not essential to analysis in order to disguise the research site or the actors therein (Jerolmack and Murphy 2019).

7 As an extreme case of disagreement among members, technical consultants and program managers of an aid organization studied by Mosse (2006) objected that his representations were biased and wanted his book draft rewritten; field staff, in contrast, agreed with his analysis. Mosse succeeded in publishing his account but only after protracted interactions with the aid organization.

than to the seductively simple rhetoric of the phrase.

A third purpose, overlapping to a large extent with the second, is an ethical checking of the “fairness” of an author’s representations of individuals, events, or collectivities. Velamuri argues that in his case, “given my research concern with business ethics, the idea that member review provided a means of ensuring that I would not place *my research interests* above the interests of the organization or those of its members resonated with me” (Locke and Velamuri, 2009, 499; emphasis added). His concern is bound up with possible reputational harms to members after the research is published. Will an industry or a community or set of people identified by a common factor (e.g., rural Irish) be represented in ways that *they* feel might defame them in the eyes of various audiences? Will “unfair” representations, if published, harm those industries, communities, or individuals? These are significant, complex ethical issues. But it is important to ask whether member-checking should be the primary way of addressing them compared to, say, ethical reflection by the author, such as asking whether a potentially hurtful characterization is central to the findings or only adds “color” for the reader. Moreover, it is even conceivable that the practice itself might raise inappropriate expectations among members, specifically, that the published study will be something of which they approve without reservation.

Also, there is the status of the author’s own truth claims. Are her representations necessarily suspect? If, as a result of member feedback, she re-characterizes what she has understood from her research, does that action further knowledge or hinder it? At a minimum, Locke and Velamuri (2009) recognize that one possible consequence of member-checking is that it “may narrow and sanitize the variety of represented human action” (494) if members seek flattering portrayals of themselves. This is precisely the conundrum on which Scheper-Hughes (2000) reflects after her return visit to her field site some twenty-five years later. Many people there remained very angry about her portrayal of the community and would brook no explanations and, indeed, demanded that she leave, and criticized others who housed her or appeared with her. Yet as she explains in the essay, while she agrees now that her book left out the many positive characteristics of the community (notably, almost no rape or sexual assault), her research purpose was to explain why Ireland had, at that time, the highest rate of hospitalization for mental illness in the world. Addressing that question meant a focus on the less privileged members of that community, producing

representations of patterned social interactions to which others took umbrage. To which members does Scheper-Hughes owe the most ethical concern—community leaders, community members writ-large, or the most vulnerable members?

Some researchers find such ethical entanglements so vexing that they turn to participatory action research (PAR)—an approach that engages field members in every step of the research process including design, analysis, writing up, and publication, constituting a kind of ongoing member-checking. PAR may be appropriate to some research topics (e.g., community health is one area where it has been used extensively). Yet what if the activities of individuals, organizations or communities are ethically abhorrent, as with the Ku Klux Klan (Blee 2003) or some of the practices within an industrial animal slaughterhouse (Pachirat 2011)? An admonition that member-checking be done for “fairness” obscures ethical dilemmas in which choices must be made by the researcher that, per force, cannot please everyone. Even PAR researchers Caretta and Pérez (2019), who are committed to a “more open process of knowledge production and a decentering of power,” (372) felt their epistemic authority challenged: “We felt we had to revise and at times even censor our analyses that were or would not be well received by the research communities” (370).

Finally, a fourth motivation, which may be one of the most common given the similarity between “member-checking” and “fact-checking,” is the desire for an “accurate” rendering of members’ experiences. After the very public criticism of Alice Goffman’s (2014) award-winning book, *On the Run*, by journalists (e.g., Singal 2015) and legal scholars (Campos 2015; Lubet 2015), “accuracy” as a standard may be something that will motivate more scholars to consider and use some of the practices subsumed under the label of member-checking. But this may be the motivation tied most closely to the presuppositions of positivist philosophy of social science, in which a singular truth and/or a neutral, “unbiased” stance by the researcher is considered possible. It is to these philosophical issues that I now turn.

Implicit Ontological Assumptions

I was surprised by how taxing it would be to navigate the positive and negative influences of the challenges associated with implied character, descriptive details, and interpretive perspective raised as I worked to realize my research project, balancing my responsibilities to my professional community [academics] with those to the

community of research participants [who provided member feedback].

—Velamuri in Locke and Velamuri 2009, 500-01

In its similarity to fact-checking, member-checking presumes that what is to be checked is something that is ontologically stable over some period of time. For some things in a research project, that is a reasonable assumption. Things that are easily checked include, for example, the year an organization was founded, the location of an event, or the professional identity of someone at a meeting. Yet as this last example hints, there are a multitude of details that a researcher might get wrong for a variety of reasons—from interviewees themselves not remembering to inconsistencies in available documents about the timeline and other particulars of events.

The extent to which this sort of accuracy matters is bound up with the varied purposes of research. Despite that variety, and contra Lubet (2015), most researchers are not focused on providing evidence of a character sufficient for legal proceedings but, instead, on contributing to knowledge. To be clear, getting basic details—commonly shared knowledge of both members and knowledgeable readers—correct is part and parcel of being a careful researcher. Related to this, to make their manuscripts more persuasive, to attest to having “been there,” researchers sometimes produce maps of their sites, such as Pachirat’s (2011, 44-45) rendering of the slaughterhouse layout and the production line moving a living animal from killing and dismemberment to packaged meat. Pachirat’s map was intended as an accurate portrayal of the production line, requiring careful attention to detail both during observation in fieldwork and during note-making and textwork. In short, some things, the outcomes of widespread consensus over time, which have relatively more ontological stability, can be checked for researcher error.⁸

That point admitted, much of what scholars are most interested in is more complex—involving what *sense* participants make of particular, temporally stable facts, such as a published speech, the estimated number of people attending a rally, or the layout of a particular building and its implications for action or practice. The

meanings that people make of such facts are not so ontologically stable, but rather contingent in a number of ways and for various reasons. Specific to member-checking, how an interviewee responds *in the moment* to an interview question (or discussion of a topic) may be quite different from his experience on seeing his answer on the written page in an interview transcript. The interviewee may second-guess himself, thinking, “Did I say that?” or “Oh dear, that doesn’t sound so good.” Another possibility is that with the passage of time and interim events (such as the closing of a business, the firing of a boss, or a political assassination), remarks made at an earlier time may, on reflection, seem to the member as, for example, naïve, unkind, or risky in some way.

Whatever the reason, suppose the member asks for substantive changes to the interview transcript beyond, say, a misremembered date. Should those revised views be understood by the researcher as preferable to the views first expressed? Which of these is “correct,” “true,” or “accurate”—the initial statements or the subsequent revisions? For those assuming ontological stability of expressed views, versus a contingent perspective on meaning making, the revised, “corrected” reactions are to be preferred. For those with a contingent view of individuals’ and collectivities’ meaning making, member-checking can never shift researchers toward a single truth because each inquiry occurs in a new moment as individuals and collectivities move through time. In this contingent view of meaning, history matters, whether it is the life cycle of an individual who understands her experiences in different ways over time or emerging collective discourses about, say, statues of some US states’ Civil War heroes. From this perspective, and in tension with my earlier definition, member-checking might be better understood as *a new round of data generation* that may, or may not, deepen a researcher’s understandings of those studied but still potentially challenges her epistemic authority.

Whichever philosophical position one takes, repeated member-checking leads to the possibility of infinite regress. Must the researcher’s revised text be checked *again*? And if more changes are requested, is another round needed? What does any ultimate agreement indicate, truth (for positivists) or deeper understanding

⁸ It is important to say these days that subscribing to an interpretive philosophy of social science does not mean accepting falsehoods such as Holocaust denial. All researchers should aspire to accuracy of this sort even if certain details may need to be deleted or changed to protect the identity of research participants. Of course, how intersubjectively “settled facts” came to be is itself an interpretive research question.

(for interpretivists)? Or is it simply the researcher's and members' exhaustion?⁹ (This brings up a question that is often not asked about member-checking: Will it be appreciated by members or might they consider it an imposition since they have already, at a minimum, given of their time?)

Several rounds of review appear to be a potential outcome of member-checking. When he was a doctoral student, Velamuri (2009) went through three rounds of drafts, reviews, written requests for modifications, and discussion. In total, the managers of the organization he studied “sent some 70 pages of requests and arguments, including additional company documents that they had not previously shared. The fourth draft...was incorporated into the dissertation” (Locke and Velmauri, 500). That the company sent additional documents clearly demonstrates the way in which member-checking can serve as a data generation technique. Such an extended process, however, may have unintended consequences if the checking transmogrifies into attempted censorship, as in the case of Mosse (2006; see note 7). Scholars should ask themselves, then, will member-checking be worth it? Velamuri found it taxing (see the epigraph that begins this section), yet it may well have been worth it *to him*, if it enabled continued access to the field. What is important for interpretive scholars to remember at the design stage of research is that the typical usage of member-checking often conflates checking ontologically stable facts with thornier purposes of checking members' contingent, and often varied, views on the quality and fairness of author representations.

Ever since the so-called “crisis of representation” in the mid-1980s, anthropologists and others have reflected on the ethics of representing others (e.g., Alcoff 1991). Member-checking may seem a tempting way to address this ethical problem, and for some of the narrower purposes considered above, it may be useful to a research project. But it should not be considered (or promised) casually in a research design, and it is highly problematic, both ontologically and ethically, if mandated by ethics review boards or others such as funders. Commenting on Shokeid's (1997) reported exchanges with interlocutors that today we would consider member-checking (although that specific term is *not* used), Plotnicov (1997)

reflects on the tension between research participants' meanings and the integrity (i.e., epistemic authority) of the researcher as author:

Is it possible to improve on the current means of giving subjects their own voice while maintaining the ethnographer's integrity? It seems unlikely. The only apparent solution is to invite the subjects to read the final draft of the manuscript or its page proofs and have them respond, refute, amend, qualify, etc., Published together as a unit, the different perspectives ideally should reflect and illuminate each other. (643)

However, he goes on to call this solution “impractical”; wisdom, he says, “comes with accepting and adjusting to limitations” (643). Such impracticality might be debated, but it is clear that researchers should very carefully consider what to check, why it should be checked and, especially, whether member-checking is appropriate to their projects. For those committed to PAR or studying marginalized communities in the global south (e.g., Jazeel and MacFarlane 2010), the answer may be a resounding yes, given the potential gulf between their privileged backgrounds and the communities they study. For researchers in other epistemic communities or with different projects, such as “studying up” (Nader 1969), the answer is much less clear because of the complexities analyzed here.

Concluding Thoughts

I have sought to clarify the definition and practice of member-checking, differentiating it from other practices that appear similar. Moreover, it is also possible to recognize its practice even when the term is not used. Continued reflection by political scientists on member-checking is warranted as there appears to be much more to learn about scholars' experiences with it, their purposes in using it, and the extent to which it was helpful or produced unexpected consequences—the dreaded quagmire of my subtitle.

The motivating impulse behind member-checking—wanting to understand the perspective of those studied—can be met through incorporating other criteria for research quality into research practices and

⁹ Caretta and Pérez (2019) take up the matter of accuracy in what appears to be a positivist conception of research. Specifically, in her research project, Caretta engaged in a member-checking exercise in which community men appeared to agree with her representations whereas community women contested it, arguing that she had favored the men's perspective. Agreeing with the women, she incorporated those perspectives into her write up only to later publicly concede that “a man [who] asserted that what was written in the booklet was not accurate” was right, concluding in *this* 2019 analysis that he had “invalidated” the booklet claims (365-66). This accuracy conundrum results from a perspective on member-checking that emphasizes a “consensus-oriented interpretation of data” (366), which contrasts with an interpretive perspective in which disagreements are expected as a part of the politics of any human society.

writing. One of these is thick description. As Geertz (1973) famously explained, thick description sufficient to differentiate among an eye twitch, a blink, and a wink rests on an evidentiary base that demonstrates extensive understanding of members' worlds. Another indicator of quality is reflexivity, analyzing one's researcher self—theorizing and assessing, for example, how that self may be inhibiting (or facilitating) understanding of the research site and its various members. (For an exemplar, see Shehata 2014). Finally, use of triangulation (understood interpretively as intertextuality) attunes the researcher to conflicting perspectives of members, enabling analysis that traces out the sources of those views rather than seeking a consensus on meaning from members. None of these alternatives guarantee a researcher's understanding of members but, for better or worse, they preserve the researcher's epistemic authority as author of a study. In sum, if other criteria for demonstrating the quality of interpretive studies are explored, member-checking may not be necessary; it is certainly not automatically warranted.

Perhaps most important to recognize in the

contemporary context of political science is how member-checking reverberates with positivist philosophical presuppositions which, if embraced, undermine understandings of interpretivist approaches to it. Ontologically stable, quotidian facts are capable of being checked, but individual and collective meaning-making is sensitive to changing contexts throughout the entire time frame of a research project, rendering member-checking a useful means of generating new evidence—but not, with regards to interpretivists, for arriving at a singular truth. Interpretive researchers do not aspire to be mystery writers discovering facts to ascertain “whodunit,” much less to produce facts for legal proceedings rendering dichotomous judgments of guilt or innocence. Rather, we seek to understand *how* various positions, interests, and cultures produce and live with multiple truths. If member-checking aids such goals, researchers should consider it—always in comparison to other possibilities for representing those truths. But mandating member-checking misunderstands interpretive research goals, constituting yet another disciplining hand limiting researchers' academic freedom.

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"You're Asking the Wrong Question:" Member-Checking during Fieldwork

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In February 2014, I'd been living in Reykjavík, Iceland for just a few weeks. I was intending to study "the Icelandic Revolution" of 2008-2009, when a new series of large, public protests changed my plans. Over the course of a few weeks and then months, I met many of my research participants at these events. One such participant was Kristján¹, whom I met when he asked me why a "tourist" was interested in Icelandic protests. A few months later, in the run-up to municipal elections in May, he invited me to attend an outreach event associated with the Social Democratic Alliance (SDA), one of Iceland's four major political parties at the time. There, Kristján introduced me to another party member named Thorvaldur, and we struck up a conversation that turned into an informal interview. Not unlike Kristján, Thorvaldur expressed curiosity about why an

útlendingar (foreigner) such as myself would be interested in Iceland's local politics and asked me to explain the project. Thinking that I had successfully refined my pitch over the past few months, I explained my interest in the various ongoing sovereign debt crises in Europe and the Eurozone, which elicited the question of what the so-called "age of austerity" meant for the future of social democracy in the European Union. Then, Thorvaldur rather abruptly exclaimed that I was "asking the wrong question!" "Instead," he told me, "you should be asking whether social democracy has a future *without* the European Union" (personal communication, April 2014).

What happens when a research participant tells you that your questions are wrong and that you should be doing something else? Not only did that interaction

¹ All participant names have been changed.

catch me off guard in the moment, I also puzzled over it for a long time thereafter. *Was* I, in fact, asking the wrong question? And if I wasn't asking the wrong question, how could I make sense of this interaction? Should it impact my research plans, and if so, how?

The practice of member-checking might offer a solution to the situation I described. Since member-checking is often embedded in the process of research itself (Given 2008), it implies that participants' responses might not only challenge the validity of research findings, but also the questions driving the research. Accordingly, member-checking promises to help the field researcher avoid the potential pitfalls of operating in an unfamiliar context. When implemented during fieldwork, it potentially prevents the researcher from pursuing a line of inquiry that would ultimately prove invalid due to inaccurate assumptions. Moreover, at first glance, the approach seems compatible with the underlying ethos of ethnographic field research, especially in light of the participatory/collaborative turn among its practitioners (Rappaport 2008).

Despite its promise, member-checking also risks conflating the practice of taking research participants *seriously* with taking them at *face value*. Had I taken Thorvaldur's "check" at face value, I might have concluded that my project was invalid (because he said as much) and followed his directive. Instead, I took him seriously. I treated his objection as a new puzzle: what did he mean when he said my question was "wrong," and why might he have perceived it that way? The latter approach generated valuable insights that I might have otherwise missed.

In my analysis, I consider how researchers might respond to (sometimes unsolicited) "member checks" on in-progress research. The dilemma researchers confront is not *whether* to respond when your interlocutors tell you that your questions (or findings) are "wrong." Rather, researchers should consider *how* to respond to participants' objections. As a strategy for dealing with this issue, I suggest that researchers may find it generative to put objections on hold, that is, to recognize that objections are potentially valuable without necessarily following the directives that may accompany them. Rather than posing a validity problem, participants' objections can instead provoke new insights.

Responding to Unsolicited Member-Checks During Fieldwork

Member-checking is not only a post-hoc procedure but also often part of the research process itself (Given

2008). It's understandably attractive to ethnographically driven researchers because it reflects ethics of inclusion, reciprocity, and egalitarianism which increasingly inform fieldwork. However, it also generates tensions that should give field researchers pause before implementing the practice wholesale. Here, I consider ethical affinities between member-checking and fieldwork, and potential obstacles to implementing it.

First, member-checking appears consistent with the participatory and collaborative turns in ethnography, which seek to democratize field research by moving beyond traditional participant observation to make it a more inclusive process (McIntyre 2007; Wimpenny 2010). Collaborative ethnography "deliberately and explicitly emphasizes collaboration at every point in the ethnographic process, without veiling it—from project conceptualization, to fieldwork, and, especially through the writing process" (Rappaport 2008, 1). Similarly, in participatory action research (PAR), the project is not driven solely or even primarily by the academic researcher, but rather by a group of participants who also have control over the final product. Both collaborative ethnography and PAR emphasize that the goals of research aren't only scholarly but also political. As Bergold and Thomas (2012) write, these approaches aim to "change social reality on the basis of insights into everyday practices that are obtained by means of participatory research" (193). Academic researchers who engage in PAR or collaborative ethnography often self-identify as activists (Rappaport 2008).

Second, by recognizing that the relationship between researcher and participant is not severed at the end of an interview and/or period of fieldwork, member-checking can infuse ethnographic relationships with reciprocity. Researchers don't just take information from their participants; they develop relationships. They increasingly maintain contact with their participants and often continue returning to the field site even after the project is complete. For example, I later gave one of my participants, Jóhann, a transcription of our interview, because he had worried that he was beginning to forget the details of the events he was describing to me. In PAR and collaborative ethnography, sustained relationships extensively inform the research design. Participants are involved in making an array of decisions, often from the outset of project planning (Bergold and Thomas 2012). Although a more limited technique, member-checking enables fieldworkers to incorporate ongoing relationships and the sharing of work-in-progress into a variety of research designs. Because member-checking

calls for sustained interaction rather than one-time data extraction, it promotes a more reciprocal relationship between the researcher and participants.

Finally, PAR and collaborative ethnography have animated a broader shift in how the people involved in the research are conceptualized in relation to it; passive “research subjects” are instead active “research participants,” who have a recognized stake in the project and who have often generously contributed their time and resources. Similarly, member-checking reconfigures the dynamics of authority in the researcher-participant relationship. It affirms the idea that researchers should take their participants seriously (Astuti 2017) because they are experts on their own lives. Accordingly, it potentially helps the researcher avoid misrepresenting their participants’ beliefs, practices, experiences, and subjectivities (Koelsch 2013). In doing so, member-checking aims to make the researcher-participant relationship more egalitarian.

Altogether, member-checking offers fieldworkers a strategy for incorporating some of the inclusive, reciprocal, and egalitarian research ethics advanced by PAR and collaborative ethnography into a wider array of project designs. It’s also more adaptable to situations where the researcher might not have the established research relationships that PAR and collaborative ethnography presume. Yet it isn’t always a straightforward way of implementing the research ethics outlined above.

For example, although member-checking aims to make research more inclusive, it’s currently conceptualized in a way that isolates the interview from, and privileges it over, other ethnographic research activities as a metric of validity. While interviews often play a central role in ethnographic research, ethnographers typically don’t rely on interviews alone. They also draw on participant observation, various textual and archival sources, and material culture. As Nicholas Rush Smith’s contribution to this symposium proposes, it isn’t possible to check back with every kind of ethnographic data in the same way. Along these lines, the practice implies a hierarchy of validating sources, including among living participants in the research.

Despite efforts to promote reciprocity, member-checking still risks treating participants as repositories of information. It may misattribute a stable, static subjectivity to participants and consequently assume a one-way relationship in which the researcher merely collects knowledge or observations from the participant. The researcher (and participant) might fail to recognize that their interaction co-constitutes knowledge (Finlay

2002). Furthermore, participants don’t answer interview questions in a vacuum. They also respond to—and within—a particular milieu that may make certain questions more or less resonant with their experience and present thinking. Context shapes the extent to which the participant engages with a question, the length and detail of their answer, and the examples they discuss. Researchers therefore cannot assume that any given participant would provide the same response to the same question at a different time. By the same token, the researcher shouldn’t assume that she is static in any of the ways just described.

Lastly, in an effort to disrupt researcher-participant hierarchies, member-checking may imply that participant statements should be taken at face value, which isn’t the same thing as taking participants seriously. If a researcher accepts a participant’s claim that her interpretation is invalid on its face, it stands to reason that she should respond by discarding it. However, such a response puts the participant in the position of adjudicating not only whether the researcher’s interpretation is “right” or “wrong,” but also (indirectly) adjudicating the other participants and sources of data that inform the researcher’s findings. In contrast, taking participants seriously involves recognizing multiple sources’ contributions (Astuti 2017). As I will show in my analysis, taking all participants equally seriously creates space for conflicts and contradictions to coexist.

As presently understood, member-checking doesn’t provide much direction about how to respond to participants who say some aspect of the *research* is wrong while the project is in progress. If implemented as a continuous process, fieldworkers need a more robust way to handle unexpected objections that emerge while in the field. While member-checking does attempt to address the “known unknowns” of field research, its strategies are narrowly geared toward validity, potentially obscuring conflicts that would otherwise generate valuable insights. For example, Tanggaard (2008) argues that participant objections should not always be perceived as obstacles or negative events. Objections during interviews can be productive in the sense that they can provoke the researcher to rethink and reformulate their questions to make them more “valid” (Tanggaard 2008).

However, while participant objections can certainly generate new insights into the project itself, it is less clear that the value of engaging with them lies in achieving greater validity. Instead, the researcher may want to identify patterns of objection to expose conflicts that would otherwise remain overlooked. As Hammersley

and Atkinson (2007) note, “ethnographic research cannot be programmed...its practice is replete with the unexpected” (21). Fieldworkers expect their researchers experiences to destabilize some of their “prior knowledge,” which may require revising the project’s basic questions (Schwartz-Shea and Yanow 2012). Yet Hammersley and Atkinson (2007, 21) advise against adjusting to unexpected events in the field “by taking the line of least resistance.” Rather, they explain, “there is an important sense in which all research is a practical activity requiring the exercise of judgment in context; it is not a matter of following methodological rules, nor can all the problems be anticipated, or for that matter resolved” (21).

How, then, should the researcher respond when a participant says the project question is wrong and/or directs the researcher to do something else? Should the researcher assume that her project is invalid? If the researcher receives contradictory responses, should she put more stock in some interlocutors than others? When and how should she decide? The answers to these questions can have profound consequences for the direction of an entire project. Moreover, they are resistant to anticipatory responses on the part of the researcher; not every contingency can be planned for.

The idea of exercising judgement in context offers a strategy for implementing some principles of member-checking as part of the research process. It involves recognizing the constraints that the researcher faces while in the field. Part of that may mean that the researcher recognizes that she isn’t able to interpret or act upon a participant’s objection without more information. However, this doesn’t necessarily contraindicate the underlying ethic of member-checking. Rather than treating a participant’s objection as a directive, the researcher can instead meaningfully respond by putting the objection on hold until she has sufficient context to make sense of it. In the remaining sections, I further consider the promises and pitfalls of member-checking by discussing how I responded to Thorvaldur’s objection to my question, and the one he proposed instead.

Responding to Participant Objections and Directives

In this section, I examine my initial response to Thorvaldur’s (2014) claim that I was “asking the wrong question,” his directive of what he apparently saw as the right question and offer practical strategies for handling this kind of situation as it unfolds. In short, I unintentionally let his unsolicited “check” on my

question bother me throughout the subsequent year of my fieldwork. I didn’t understand why he thought my question was wrong. Moreover, what he considered “right” question flew in the face of what I thought I knew about the financial crisis in Europe. However, I went into the field open to the possibility that my prior knowledge wouldn’t hold up. On the one hand, I might have dismissed Thorvaldur’s remark. After all, it was unsolicited, and the interaction was relatively brief. On the other hand, who was I to maintain that my question was valid when an interlocutor said it wasn’t?

All along, I worried that my inability to make sense of it meant that I was missing something big. I didn’t explicitly adapt my project questions to his intervention, but if it kept nagging at me, shouldn’t I have been more inclusive of his feedback? Eventually, I concluded that Thorvaldur’s objection and directive were neither obstacles nor distractions, but rather a productive encounter that highlighted an important conflict. His objection elicited doubt not because of who he was or the nature of the interaction, but because I was unable to resolve it for quite some time. I couldn’t make sense of it without more context than I had in that moment. Nevertheless, I also would have lost out on some insights had I ignored or downplayed it.

I have since reframed the nagging doubts elicited by the interaction as a decision to put to what he said on hold. In other words, I suspected it was important but didn’t yet know why. Therefore, by 1) placing the directive on “hold,” I was able to 2) situate his response in a broader pattern of objections and 3) make sense of the conflicts between him and other research participants that would later emerge. Instead of demonstrating that I should have changed my “wrong” question to reflect one that my interlocutor thought of as “valid,” my handling of this interaction held the answer to bigger-picture questions.

Putting Checks, Doubts, and Contradictions on Hold

Putting an objection on hold makes it possible to avoid haphazard responses to unexpected developments during field research, including unanticipated friction with participants. As I noted in the introduction, significant political developments were unfolding around the time of my informal interview with Thorvaldur. In the wake of the 2008 financial crisis, a historically unprecedented Social Democratic Alliance/Left-Green government (elected in 2009) initiated the European Union accession process. However, in 2013, a coalition

consisting of the Eurosceptic, center-right Progressive and Independence parties returned to power, despite having shouldered popular blame for the unsustainable financial expansion of the preceding decade. When I began my major fieldwork in early 2014, the government moved to withdraw Iceland's pending EU application. Between February and April 2014, thousands of people gathered in the parliament square to protest the ruling coalition, claiming that the government reneged on its campaign promise of a referendum on the future of the EU accession process.

Although Progressive and Independence parties resumed control of the national government, the Reykjavik city council remained a stronghold of protest politics and the Icelandic center-left. The SDA was energized by popular outrage at the national government. In fact, some of the protest events were initiated by members of the party's youth organization. Because nearly a third of Iceland's total population lived in the city, the city council served as a substantial countervailing force against Althingi (the national parliament). Much was at stake for the SDA in the municipal election and at the time of my conversation with Thorvaldur.

In hindsight, I made three decisions after that interaction that shaped how my fieldwork would unfold. First, I decided that I lacked sufficient perspective to adjudicate Thorvaldur's objection. Because I had only spent three months in the field, I simply didn't have enough data to compare. Furthermore, due to the EU protests and municipal election, many of my interactions at that point had been with SDA members, which was not the only group I intended to work with. In this instance, inclusivity demanded that I not unduly privilege some participants' perspectives on the project. I held off on deciding whether my question was "right" or "wrong," but would later realize that the significance of the interaction lay elsewhere.

Second, I decided that his objection was nevertheless worth keeping in mind. I *did* have enough perspective and data to recognize that the interaction could have implications for the project and its findings. The decision to withdraw the EU application shifted public debate considerably since I first formulated the project. Third, because my informal interview with Thorvaldur took place during a moment of political disruption, I decided to wait for the election before reconsidering his check on my question. Between the recent national election, the protests, and the upcoming municipal election I suspected that SDA sympathizers were unusually agitated. If the collective outrage over the EU application were to blow

over, I wanted to find out how they might respond to my questions once the dust settled.

Situate Objections in Context

Although member-checking embraces sustained, reciprocal research relationships, it can ignore how participants' responses are shaped by time, place, and social position. Over time, my accumulated interactions made it possible to situate Thorvaldur's objection to my question in the context of several political developments that emerged during my fieldwork.

For example, my initial research trip to Iceland in June 2013 came just a few weeks after the post-financial crisis government, led by the SDA and the Left-Green Movement, was ousted. On that trip, I attended a public talk given by an SDA member of parliament. During the question and answer period, I asked how, after the financial crisis, Iceland avoided Greece's sovereign debt problems. The latter was in the Eurozone and was subject to the European Central Bank's demands for austerity policies. In contrast, the Icelandic government avoided a sovereign debt crisis by implementing capital controls, devaluing the currency, and repudiating responsibility for private banks' debts. However, the MP not only rejected the comparison ("Iceland is *nothing* like Greece") but also declared, "being a member of the Euro would have prevented the crash in the first place" (because it would have prevented the króna from becoming overvalued) (public lecture, University of Iceland, June 2013). Like Thorvaldur, the MP appeared to object to the question itself. Non-membership of the Eurozone was Iceland's downfall, not its saving grace.

It was only through accumulated interactions of this sort that I came to understand why it was that some participants might reject questions that assumed a critical stance on the EU. For EU supporters, it was a debate that was too easy to shut down by making, for example, appeals to the sanctity of Iceland's independence. A dramatic departure from the status quo, the post-crisis SDA/Left-Green government presented an unusual opportunity to put accession on the public agenda. In early 2014, the ruling coalition's rationale for declining to hold a referendum on whether Iceland should continue talks signaled the reconsolidation of hegemonic Euroscepticism. Since voters had elected parties that were explicitly opposed to EU membership, the coalition reasoned, there was no point in holding a referendum. The Progressive and Independence parties would not pursue membership under any circumstances. If the electorate really wanted a referendum, the coalition

claimed, voters would have supported parties for whom EU membership was an open question.

Much later, I realized that more or less overt objections to my questions connected Thorvaldur, the MP, and numerous other interactions. Only then was I able to make sense of Thorvaldur's directive. In order to forestall considerations that would shut down the debate about joining, he framed the accession question in terms of the risks that remaining outside of the Eurozone posed to Iceland's ability to support social democracy. In contrast, my question reflected the prevailing scholarly consensus at the time, which held that Eurozone members like Greece faced constrained prospects for economic recovery (Kolb 2011; Blyth 2013; Vasilopoulou, Halikiopoulou, and Exadaktylos 2014). My critical framing of the question made it incompatible with Thorvaldur's attitude toward the EU. Yet if I had shifted my approach based on his directive, I wouldn't have experienced the repeated objections that enabled me to later identify the boundaries of pro-European discourse.

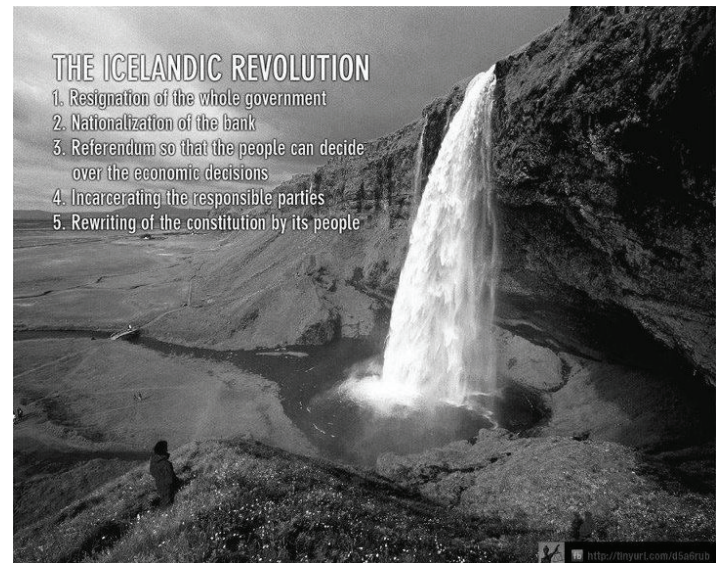
Make Sense of Conflicts

Member-checking implies that participants are authorities on both their own lives and on their group precisely because they are members of it. However, during fieldwork, it can obscure conflicts among participants because it can lead the researcher to reify participants' "groupness" (Brubaker 2002; Desmond 2014). The researcher might make problematic *a priori* assumptions about which participants should be included as members of a particular group. Consequently, the researcher also risks relying on check-backs that overlook divergent interpretations among different participants in the project. Instead, the researcher can approach the task of making sense of conflicts as an integral part of co-creating knowledge.

I initially conceived of each participant as a "member" of a targeted sample consisting of people who participated in contentious politics since 2008. However, I later found that this conceptualization submerged conflicts among and between participants. About a year after I met Thorvaldur, I interviewed Jóhann, a participant in the 2010 protests around the failed Icesave bank. That interview revealed deep fissures between different groups involved in protests since the financial crisis. Jóhann and Thorvaldur expressed such starkly contradictory interpretations of events and of each other that it is unlikely that either would "validate" findings that took both perspectives seriously.

Since 2008, protests in Iceland involved a number of claims that seemed internally consistent in the context of subsequent post-crisis protest movements on continental Europe. One meme circulating through social media around 2012-2014 enumerated the purported accomplishments of the so-called "Icelandic Revolution" (see Figure 1, below).

Figure 1: A widely circulated meme outlining the "Icelandic Revolution" (source unknown, ca. 2012).



The "referendum" in item 3 refers to two referenda that were held over the Icesave debt. When Iceland's banks collapsed in 2008, around 300,000 customers in the United Kingdom and Netherlands countries lost savings held in online banks that offered high-yield savings accounts. One of these banks was Icesave. Normally, deposits would be protected by an insurance mechanism, but due to the scale of the collapse and the rapid depreciation of the króna, Iceland's deposit insurance scheme ran out before all foreign priority depositors were reimbursed. In the meantime, the UK and the Netherlands reimbursed their citizens and demanded immediate repayment from Iceland. For Thorvaldur and the SDA, these events confirmed that Iceland should join the EU. Iceland's volatile currency and non-EU status exacerbated the crisis because the former made repayment more expensive and the latter deprived Iceland of international assistance.

However, Jóhann perceived the opposite. As an Icesave protester, he demanded that the government hold a referendum on whether it should issue a sovereign guarantee. In his view, the UK and Dutch governments' attempts to "bully" Iceland into a sovereign guarantee against Icesave confirmed his suspicion that more

powerful EU states had no problem infringing upon smaller states' sovereignty. By putting Thorvaldur's account in dialogue with Jóhann's, I recognized that despite the appearance of victory for critics of the establishment writ large, pro-European and anti-Icesave participants in fact harbored a deep disagreement over the issue of sovereign debt after the financial crisis. Thorvaldur, characterizing all Euroscepticism as irrational and anachronistic, had dismissed anti-Icesave protesters as "xenophobic nationalists" who had no compelling reason to object to EU accession. In contrast, Jóhann suggested that SDA members were "blinded" to the facts of the case due to their singular determination to join the EU.

Since pro-European Icelanders were associated with the ideological center-left, it was tempting to assume that their views on the Icesave debt would be consistent with other parts of the post-crisis SDA platform, such as the call for a new constitution. If the SDA saw the financial crisis as symptomatic of deep-set problems in Iceland's democracy, surely they would object to nationalization of private debts, which would transfer responsibility to the population as a whole. Instead, the party saw cooperation with the European Free Trade Area's (EFTA) demands for a sovereign guarantee on the debts as a prerequisite for a fast and painless EU accession. In contrast, Jóhann framed the guarantee as an unacceptable incursion against Iceland's sovereignty.

The SDA/Left-Green government would hold two referenda on proposed Icesave repayment deals. Voters twice rejected the proposals due in part to the demand for a sovereign guarantee. In response, the UK and Netherlands brought a suit against Iceland in the EFTA court. Contrary to its own preferences, the ruling coalition was forced to defend the referendum outcome. By early 2013, EFTA had ruled in Iceland's favor. For the Progressive and Independence parties, the decision vindicated their misgivings about EU membership, and they made the Icesave debacle part of their 2013 election platforms. Their stance on the debts ultimately helped deliver them back into power.

The conflicts that surfaced through my interactions with Thorvaldur and Jóhann unraveled my understanding of the Icelandic Revolution. Although they were both members of my research, it wasn't appropriate to think of Thorvaldur and Jóhann as members of an otherwise coherent group. Not only did their party allegiances diverge, their interpretations of events and their respective positionalities also contradicted each other. Had I taken Thorvaldur's characterization of anti-Icesave protesters

at face value, and adapted my research question to his directive, I might not have pursued the interview with Jóhann. Thorvaldur and Jóhann both unequivocally dismissed each other's claims not only about what happened but also what questions the events raised. While I regarded both equally as experts on their own experiences of the financial crisis and its consequences, each thought the other was missing the point.

For that reason alone, it would be problematic to check back with either of them to *validate* findings that take both participants' perspectives seriously. However, that doesn't mean that member-checking is a futile exercise. Rather, it needs to be recognized as a continuous, recursive part of the research process and treated as a way of making space for conflict and doubt on the part of the researcher and participant alike. Because I put Thorvaldur's objection on hold instead of either dismissing it or reframing my project around it, I acquired an understanding of why he rejected the basic premise of my question. Eventually, his objection generated insight into a broader set of developments, including the establishment parties' return to power, and protest over the status of the EU application.

Conclusion

As the concept of member-checking is presently understood, Thorvaldur's declaration that I was "asking the wrong question" represented an unexpected "check" on the validity of my research-in-progress. On the one hand, treating Thorvaldur's objection as such a "check" was potentially valuable because it prompts the researcher to take a more inclusive, sustained, and egalitarian approach towards participants. This potentially reveals faulty assumptions a researcher might have brought to the field. On the other hand, member-checking doesn't advise the researcher on how to respond to objections and directives that surface while the research is in progress (whether solicited or not). Because I expected the project's contours to change, I was deeply unsettled by Thorvaldur's declaration that I was asking the wrong question. However, as there is little guidance available for interpretive (field) researchers on the process by which questions and projects change, I was on the lookout for signals and thought that perhaps his directive was the clearest signal I was going to get.

Yet, as I later found, my questions were ultimately reshaped not by specific individuals' interventions but rather by the unpredictable nature of the changing context. Although separated by a year, my conversations with both Jóhann and Thorvaldur were marked by

protests over the EU application, municipal elections, lifting capital controls, and ongoing discontent about a yet-to-be-implemented constitutional measure that would have made it possible to force referenda by petition. The various controversies and conflicts that arose during my time in Iceland influenced where I went, whom I met, what participants had to say, and what conflicts and common ground animated our discussions.

Asking questions that provoked confrontational responses turned out to be revelatory in its own right. Thorvaldur's objection revealed an unexpected puzzle, but it did not invalidate my question. Instead, his objection pushed me to figure out what his objection meant. *Why* did he think my question was wrong?

Why did other SDA members also appear to bristle at criticisms of the EU? Eventually, my accumulated data revealed that pro-European Icelanders like Thorvaldur sometimes dealt with the hegemonic Euroceptic consensus by also treating the question of joining the EU as though the rationale was equally self-evident and incontrovertible. My tasks then became making sense of the broader context in which our exchange occurred and disagreements between my participants. Ultimately, Thorvaldur's claim that I was "asking the wrong question" generated insights into post-crisis political developments in Iceland that I would not have discovered had I either haphazardly adapted my research to his intervention, or if I had simply cast his remark aside.

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If My Participants Say, “You’re Wrong” Does it Mean I Really Am?

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In addition to the sharing of their lives, part of the close relationship that participants can have with researchers is an interest in the research project itself. Some participants are curious about the inner workings of the project and how researchers intend to use the information gleaned from field site visits. This subject differs from existing literature on the ethnographic endeavor. Some work focuses on the identity of the researchers and the implications for the relationship with key participants (Rabinow 1977). Other work, however, examines the challenges of inviting participants into the writing process (Shokeid 1997). The following discussion also considers the dilemmas encountered when participants become part of the analysis and writing process. Unlike previous work, it examines the point at which an analyst has several ideas on paper rather than a full manuscript. Additionally, it looks at a stage past the research design period.¹ Specifically, this essay addresses the following questions: Should researchers make significant adjustments when participants view them as incorrect? Are there additional factors at work that account for the discrepancy between participant and researcher interpretations?

Answering these questions is important in light of previous research that suggests the purpose of member-checking is to ensure quality (Schwartz-Shea 2014, 135). To this end, I will argue that participant claims that the researcher is wrong does not necessarily signify that the project has no validity. Rather, taking participant views into account in a critical way allows researchers to determine whether legitimate revisions are needed or whether there are factors beyond the researchers’ control at play. To that end, this paper will first address the nature of my original research project, which triggered these questions. Second, it will examine how member-checking featured in my research, and the effect that it had. Finally, it will suggest four ways in which researchers can take their participants’ perspectives seriously without entirely discarding their projects.

The Original Project and Member-Checking

My original research dealt with identity formation and resistance among ethnic minority groups in China. It asked two questions: Why do ethnic minorities in China express aspects of their ethnic identity despite the possibility of repression? How are their identities produced and reproduced over time? I operationalized ethnic expression as holiday celebrations, and argued that these gatherings, in the tradition of *Weapons of the Weak* (Scott 1985), constituted a form of protest called “ritual resistance” that reinforced ethnic identity. I argued that during these celebrations, ethnic minorities share stories, songs, and relevant political information. Those who engage in traditional protest are at higher risk for detention and legal action than their Western counterparts. Thus, much like Scott’s research participants, Chinese ethnic minorities were not passive political subjects, but rather found innovative ways around state restrictions.

The prospect that the ideas presented here could be “wrong” came about during a Uyghur language tutoring session during my fieldwork in 2015. My tutor, Rahile, took an interest in this project and requested a summary of my argument in Uyghur. After reading three or four lines she stopped and said, “Why do you think this? This is wrong.” After I explained that the breakdown was ahead, she read two or three more sentences and maintained her position: “I really don’t know why you think this.” My examples of politically significant songs about the Uyghur homeland were not enough to convince her: “We just like the songs. They don’t really mean anything” (Rahile, personal communication, January 2, 2015).

This conversation made me wonder whether I was too invested in my own ideas when they did not fit with how participants understood their experiences, forcing a conceptual and theoretical fit where none existed. Baogang He, a scholar of Public Policy and Global Affairs at Deakin University in Australia, writes: “Often studies that aim to use China to validate Western theories

¹ See Maraj Grahame, this symposium.

and concepts are irrelevant to China's reality" (He 2011, 270). Thus, researchers may be inaccurately rendering the analysis by applying these concepts to what participants report.

When my tutor said that I was "wrong," I became concerned that I was inappropriately applying concepts. A number of my participants made it clear they viewed their actions neither as political nor as resistance. Thus, it was possible I was working with a particular conception of resistance in a part of the world where it did not apply. At the same time, however, while my participants are experts on their own lives, their experiences by themselves do not constitute new knowledge. In this sense, it is important to acknowledge that there are times when concepts do not fit, but also recognize that a participant's view that researchers are "wrong" does not mean that the research is "wrong." The following four suggestions will demonstrate how researchers can make similar adjustments.

What is the Nature of "Politics?"

What led Rahile to question the validity of the motivating theory was a fundamentally different understanding of politics. As she explained, "Politics is what the government does, not us" (Rahile, personal communication, January 2, 2015). She was uncomfortable with a characterization of her and other Uyghurs' behavior as "political." In the American context, however, the use of "In God We Trust" license plates can be considered a political expression of banal nationalism (Airriess, Hawkins, and Vaughan 2012, 50). The chasm between how Rahile viewed political action and how ordinary Americans view it suggests a need to take seriously what constitutes "the political."

Other scholars have explored what constitutes "the political" and why it matters. Michael Schatzberg makes this argument in *Political Legitimacy in Middle Africa*, pointing out that Western political scientists assume that what they understand as political in their own context transfers seamlessly elsewhere. In particular, American scholars tend to assume a separateness of politics and religion. This same separateness does not exist in Africa, and Schatzberg implores scholars to think broadly about the state and politics to allow a role for the spiritual world, sports, and business. It is easy to assume that separateness of religion and the state exists the same way in other contexts as it does in the United States, but in doing so, researchers miss key dynamics of interest (Schatzberg 2001, 108-09).

Schatzberg's admonition applies to the research under consideration here. Rahile's (2015) statement, "Politics is what the government does," also raises the question: "What are politics?" In the Chinese context, however, the answer to this question suggests a need to restrict the realm of politics rather than widen it further. Another discussion with Rahile is instructive here. We discussed the concept of "family politics," and in particular, examples regarding how adult Americans negotiate relationships with their families of origin. The term "politics," however, did not resonate with Rahile. Interestingly, she was not opposed to the description of the dynamic. Instead, she replied, "No, we don't say 'family politics.' We say 'family relationships'" (Rahile, personal communication, January 2, 2015). The point she makes here harkens back to the idea that politics is purely the realm of the state. It is separate from the familial sphere; individuals in China would not think to marry the two.

In this sense, then, when participants say that a conceptualization is incorrect, they may take issue only with the term used. The dynamics at play do in fact exist. Holger Albrecht makes a similar point in "The Nature of Political Participation," writing that political participation can be found in any political system, whether democratic or authoritarian (Albrecht 2008, 15). In a similar spirit (if entirely different context), Locke and Thelen (1995) make what they call "contextualized comparisons," in which they compare "sticking points" across labor movements in advanced industrial economies (343). Although the sources of labor conflicts are different, they are still considered "analytically parallel" in the sense that they "capture the particular way that common challenges have been translated into specific conflicts in the various national settings" (Locke and Thelen 1995, 344). Thinking about concepts in this way, whether the nature of political participation or labor struggles allow researchers to make different kinds of comparisons. Nonetheless, there still seems to be a conflict with regard to differences in the nature of politics: If there are realms of society that are not considered political, arguing that people are in fact engaging in political action is difficult. Reconciling the two viewpoints is possible, however. Albrecht (2008) goes on to write that the political regime determines how leaders feel about political participation, and that this will ultimately shape outcomes, forms, and channels of participation (17). In other words, politics comes from the government, and it is thus the

state's interpretation of people's behavior that makes it possible to say that political participation exists under all regime types.

The above discussion demonstrates that there is no conflict, then, between a theory of resistance and Rahile's (2015) statement that "Politics is what the government does." Politics is indeed what the government does, and it is that fact that allows the Chinese party-state to view ethnic minority actions as political. Thus, ethnic minorities in China may not view their holiday celebrations as political, and they are right, at least as far as their perception of their behavior. The Chinese party-state, on the other hand, has decided that holidays are political and will view minority gatherings through that lens. In this sense, there is no need to abandon the project as a whole due to participants' disagreement. Rather, there are ways to reconcile both views.

It is also worth considering that whether participants and researchers agree on a particular behavior's characterization does not change the fact that there are power dynamics at play. Researchers may nonetheless ask whether they should label this behavior as political. In *The Spectacular State* (2010), Laura Adams demonstrates why this need not be a concern. While studying how Uzbek political elites used culture to create a nation-building program, she found that she and her participants were in disagreement about perceptions of their behavior. She handles the issue by labeling her work as a "partial perspective" of an outside observer, noting that her theories of power and agency are different from her participants' views (183). She also points out that there exist identity differences: Adams is a scholar who "deconstruct[s] power dynamics that they [her participants] might not be fully aware of" (183). Her participants, in contrast, are artists and are thus more invested in communicating the value of their work than engaging in scholarly discourse. She readily admits that she was not always persuaded by their views of their behavior, but that it was likely that they did not always find her persuasive. Nonetheless, Adams claims the work as her analysis (183).

In the case of my research project, it is possible to draw a similar parallel. Like Adams' interlocutors, my participants viewed the project from a different perspective. My participants' identities are more invested either as spokespeople for their cultures or as individuals going about their own lives. My identity as a scholar requires that I theorize and explain social behavior. Thus, their disagreement that holidays are political could very well have arisen from the fact that they do

not have a theoretical perspective or even an interest in viewing their behavior from that perspective. To fully accept my participants' viewpoint would necessitate the abandonment of my identity as a scholar and the project as a whole.

Ascertaining the nature of politics in a particular context, in line with Schatzberg's (2001) admonition, is certainly necessary to making an accurate interpretation of one's data. There is a sense in which Schatzberg's point is correct: "What are politics?" is a key question to ask when studying contexts other than one's own. In the case of China, "politics" are the purview of the state and not ordinary people. While my views on politics diverge from those of my participants, this disagreement does not connote a permanent impasse. If politics comes from the state in China, then that lens will be the most appropriate with which to view the actions of ordinary people. In this sense, there is far less of a conflict than what originally seemed to be the case: individuals' actions are political because the state sees them that way.

To bridge the gap between my conceptualization of what was happening in the field and my participants' views, I revised my theory so that it retained a portion of my original thought process, but also reflected how my participants saw their behavior. Rather than state that my observations simply constituted yet another form of resistance, I acknowledged that their celebrations at home were indeed apolitical in nature. I did so, however, not only on the basis of what they stated in interviews, but also what I observed at the field sites. In this sense, I was incorporating their views rather than taking them at face value. Next, I contrasted the apolitical nature of home celebrations with the highly politicized character of state celebrations, arguing that the home celebrations, even with their apolitical character, have implications for ethnic minorities' relationship with the Chinese party-state. In short, I found myself in a position where I agreed with my participants that their personal holiday celebrations were not political in nature. I did not agree, however, that personal holiday celebrations were insignificant. Feedback from initial readers of the project suggested that revising the entire project based solely on what participants had to say would have been problematic, but that revisions backed by my own observations were appropriate and even necessary in this case. Thus, these are not circumstances in which a researcher should abandon the project. Rather, considering the differences among ways in which the nature of politics may differ from context to context is instructive in understanding why such disagreements arise.

What is the Nature of “Political Science?”

If it makes sense to ask about the nature of politics in a particular context, it is also worth asking how individuals understand political science. One reason why participants may view a researcher’s conclusions as incorrect is that there is a disconnect regarding the understanding of a particular academic discipline. When discussing my project with graduate students at Central University for the Nationalities in Beijing, one of them responded, “This is political science?” When I asked whether he shared the interpretation that holidays are indeed politicized in China, he replied, “Oh they definitely are. You’re right about that. It’s just that this research sounds like an anthropology or sociology project. What does it have to do with political science?” (Personal communication, March 4, 2014). After learning more about how Chinese students and scholars view political science, the reaction began to make sense. While it is true that these graduate students were not “members” in the sense that I did not interview them, their perspective is still valuable in the sense that it provides context for interpreting what other participants may say about interview transcripts or manuscripts. This is especially true of the participants in the research project considered here, as most of them were students. To acquiesce to participants on this matter would entail a total abandonment of the research, as there is no way to reconcile these views. Thus, an explanation of how political science is understood in China is instructive here.

A pro-government approach tends to dominate political studies in China. In the Chinese context, political studies are in service of the political system and economic development. There are several ways in which this is the case. The first is the type of research the state funds. Funding tends to be directed toward Chinese diplomacy and socialist theory. In addition, former government leaders also find their way into formal academic appointments. For example, Zhu Rongji, China’s premier from 1998-2003, was Founding Dean of the School of Economics and Management at Tsinghua University in Beijing. These arrangements are about material exchange in the sense that the university is expected to benefit the officials in some way, and the name recognition the official bestows is advantageous with regard to funding. Finally, those engaged in political studies act as government consultants rather than pursue independent scholarly study. Ultimately, their primary aim is to develop policies for the state (He 2011).

The differences extend to political science departments and writing. Wu (2011) observes that it is uncommon in Chinese universities to find courses on Chinese domestic politics. In addition, departments of political science are few and far between; rather, they are typically referred to as “public administration.” Scholars in other social sciences and humanities fields such as sociology and history tend to focus on political science topics as understood in the American context. In addition, party-state dominated political writings, such as reflective opinions and policy discussions, are often misunderstood as political science. These are generally statements in support of the government. There is, in reality, very little criticism or reflection (Wu 2011). In short, there is no real boundary between policy discussion and scholarly work in the Chinese context. Thus, the characterization here indicates that a work on holiday celebrations’ political characteristics would not fit with the Chinese understanding of political writing.

These points demonstrate that there are real differences between the American and Chinese contexts with regard to political science. Chinese graduate students’ surprise at the nature of my project is thus understandable. They would characterize my research as sociology or anthropology, where matters of Chinese society are studied. Accepting my participants’ views regarding my conclusions, in the end, would require abandoning the project because research that is not tied to a state agenda is not understood as political science. This matter is entirely outside the researcher’s control and does not indicate faulty interpretation. In this sense, disagreement between researcher and participant does not suggest the project has no validity. Researchers should thus consider whether a different understanding of political science as an academic field is what accounts for the reason participants say, “you got it wrong.”

Understand and Document Differences among Participants

The statement, “my participants said I was wrong,” can lead one to ask the question, “which participants?” Each participant has a different background and varied experiences that lead him or her to respond in one way or another. Taking these factors into account is helpful both in interpreting responses and thinking about what participants mean when they say a conclusion is incorrect. They can also account for disagreements between researchers and participants. In this sense, there is no reason to change the direction of one’s research. Rather,

it is instead necessary to document these differences with the purpose of exploring the reasons why participants might not agree with the researchers' conclusions, or with one another.

My February 2015 field site visit to Yushu Tibetan Autonomous Prefecture in China's northwest sheds light on this situation. During a visit to a local's home for Tibetan New Year, Tinley, a monk at a local monastery, asked for additional details regarding the nature of my project. After telling him more about my research design and guiding theory, he stated that he agreed with the argument that holiday celebrations serve as a form of covert resistance, stating, "it's a chance for us to be ourselves" (Tinley, personal communication, February 20, 2015). Another participant, a local tour guide named Chodak, had a different response to the argument. "No, I think you're wrong," he responded. "These holidays aren't political. They're just a time to be with family and for me to take care of my mom" (Chodak, personal communication, February 20, 2015). Although these responses appear contradictory, they are not too different from one another. Both reflect an appreciation of Tibetan culture, seen in the focus on being oneself and on family. They also reflect a lack of emphasis on government and politics in the sense that these are not relevant factors for these participants. Yet Chodak disagreed with my argument while Tinley did not.

What may account for the difference here is personal background and priorities. Politics are not part of Chodak's identity. Throughout the field site visit, he made several remarks indicating that he thought festivals were becoming too political. Earlier in the visit, he commented that the Spring Festival Gala (*chunwan*), a televised variety show, was simply political propaganda. He remarked, "I think it's really political and the government wants to use it to teach people about that stuff. But I don't think it should be for that. I don't think it should be political at all. The holiday should just be a time to be with family." (Chodak, personal communication, February 17, 2015). Chodak often spoke of his commitment to his family, and that is reflected in his comment. It is hardly surprising, then, that he did not see Tibetan New Year as political. Tinley, on the other hand, given the political repression surrounding religion in Tibet, is more likely to view cultural expressions in political terms. In recent years, monasteries have faced destruction, and there have been cases of self-immolation as a form of protest (Makley 2015). Tinley's identity as a monk may perhaps be more implicated here than Chodak's, potentially accounting

for the difference between how the two men viewed my theory.

This discussion between Chodak and Tinley clearly demonstrates how two participants could come to different conclusions regarding the same theory. Interestingly, there is little substantive difference between their views. Stating that the holidays are a time to be with family and that they are a time to be oneself both suggest that Tibetan New Year is a time to leave politics aside. Both men, particularly Chodak, found the government intervention in the holiday unpalatable. The way in which they viewed that intervention as individuals, however, may have related to their respective backgrounds. It is plausible that a tour guide, whose main concern is supporting his family, is going to have a very different attitude than a monk who is faced with government restriction more frequently.

The ultimate lesson from this fieldwork experience is that Chodak and Tinley's views of their individual lived experiences are not synonymous with theoretical analysis. Each of them interpreted my theory through their own personal lens. Should each participant do the same, there is the potential for there to be as many judgments on a researcher's theory as there are participants. In this sense, each participant's personal view cannot be the arbiter on whether the research is headed in the right direction. Some will say it is correct, while some will inevitably say it is wrong. Considering the background and priorities of each participant, however, can give the researcher a better sense of why a participant has a particular attitude. There is no need, then, to give up on the research project because of different understandings. Rather, it is best to document these differences to allow them to give a richness to the data.

When Writing, Do Adjust when Your Observations and Those of Your Participants Line Up

According to Kapiszewski, Maclean, and Read (2015), there may be times that researchers begin to sense that what they observe in the field does not conform to their original expectations. Researchers may be wrong because they have not gone deep enough in the field, they may not have known enough about the topic prior to beginning their fieldwork, or they may discover that their theory is a poor fit for what they are observing (Kapiszewski, Maclean, and Read 2015). While these authors begin with the premise that the researcher discovers he is wrong and develop their advice from

that starting point, the same advice can be applicable when participants suggest that the researcher is wrong. There may be times when participants have a point and are trying to tell the researcher something, and for that reason, it is worth reevaluating the original theory.

My own work serves as an instructive example. The original theory conceived of ethnic minority holiday celebrations as opportunities to engage in covert resistance. In this manner, songs became thinly veiled criticisms of the state, and food was a way of explicitly reinforcing a culture different from the dominant one. Participants would exchange news regarding conflict between minorities and the state, ensuring that these events would never be forgotten, despite government efforts to cover them up. Thus, holiday celebrations also demonstrated the potential for future mobilization.

In addition to participants stating that holiday celebrations did not serve this purpose, I never observed anything that would indicate that they did. For the ethnic minorities I observed, their celebrations were about festive meals, light conversation, and connecting with family and friends. Thus, when participants stated that I was “wrong,” there was a sense in which they were right. Holiday celebrations were not political in the way I had originally thought. What was not incorrect, however, was that these holidays were relevant for politics. In the end, I still argued that these holidays nonetheless showed resistance because the Chinese party-state interprets these actions as such. Thus, it is the government’s interpretation, which is still in line with ordinary people’s view that politics is the purview of the state, that drives my interpretation of minority behavior.

Making adjustments in this way allowed me to both honor participant views and make a theoretically relevant contribution. There was no need to jump to the conclusion that the research had no validity. Rather, a reassessment was useful in steering the project in a different direction. In short, there was a sense in which my participants were “right” to say that I was “wrong.” There was no feasible way in which to make the behavior I observed conform to the original theory. In this sense, it was prudent to follow Kapiszewski, Maclean, and Read’s (2015) prescriptions for making adjustments. Nevertheless, where I still differed from my participants was in the realm of whether behavior was political. I acknowledged that they did not view their behavior as political while still maintaining that even this behavior had political significance. Thus, making adjustments is possible without abandoning the project.

Conclusion

To reach the current phase of the project, I have made adjustments to my original argument that incorporates a number of my participants’ insights, but that leaves others behind, particularly those that would render the research invalid. If ordinary people in China do not engage in politics, then political science as Americans understand it is not possible, and that would discount a number of works on the creative ways in which people protest the state. Nonetheless, there were places where what I observed and what my participants thought they were doing lined up—we agreed on the finding that holidays are political for the state but not for ordinary people. That was my way of not forging ahead with my original idea when it no longer made sense, but still showing how it was politically relevant.

I have suggested four ways in which researchers can retool and make adjustments when participants say they are “wrong.” When this happens, there is no need to abandon the project and begin again. As this paper has demonstrated, the research is not necessarily invalid. Researchers should first consider what constitutes politics in their field site and acknowledge those differences. Second, an understanding of what it means to be a political scientist is also necessary, as it is possible that researchers and participants understand the field differently. Third, working to document differences among participants can help shed light on “multiplicities of understanding.” Finally, making adjustments may be necessary in the end, as it was for me. I only came to this conclusion, however, after reevaluating the observations I made. Thus, being told one is “wrong” does not have to be a crisis. Rather, it is an opportunity to more fully engage with participants and produce richer and more robust writing.

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Member-Checking: Lessons from the Dead

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I sat back in the faded red chair, happy to see Bhuti for the first time in more than a year.¹ We exchanged the usual pleasantries. He asked about my wife. I asked about how the *majita* (wise guys) were doing. Bhuti named three young men who had died since the last time we had seen each other. The only name I knew was of an informal mechanic and alleged sometime car hijacker with whom I had a dispute several years earlier about repairs he performed on a car I owned. When I asked Bhuti what had happened, he replied, "He was sick"—a semantically-vague yet commonly-used code for HIV (Personal comm. recorded in field notes, December 23, 2016). I grunted an affirmation.

Bhuti then, excitedly, mentioned a fourth name—Vernon—because he had only died a few days earlier.

When I looked at him quizzically, Bhuti said that Vernon was a local drug dealer and insisted I knew him. I had encountered several such men during the roughly twenty months I had spent researching crime, policing, and vigilantism in South Africa. In this case, I could vaguely place Vernon's name but couldn't remember having met him. Bhuti sprang from the couch, walked into the adjacent kitchen, and returned with a local paper specializing in news from Durban's million-strong Indian community. Staring at me from the page was the placid face of an Indian man who looked just a few years younger than me. I didn't recognize him but quickly read through the story.

The newspaper reported that Vernon was shot nine times while sitting in his car a few streets away from where

¹ Except when referring to events or individuals described in publicly available sources, names are pseudonyms to provide anonymity to research subjects.

I was sitting reading about his death (see Somduth 2016 for an account). Two children (one of whom was his own) were in the back seat and were also hit by the gunfire. The story reported that no arrests had been made, although the police were treating it as a gang-related homicide, as Vernon was reportedly a well-known drug dealer.² I would eventually find a video purporting to be the killing online (South Africa Today 2016). Grainy, noiseless, and in black and white, it showed two men approaching a car nonchalantly, firing repeatedly into the driver side window, getting into a waiting vehicle, and driving away.

I open with this vignette not to shock but for the opposite reason: the conversation was fairly unremarkable in context. During my fieldwork, I have met many young men who are no longer living. Indeed, each subsequent return to my sometime home in Durban feels increasingly unhomely because many people I knew have passed, even as their presence still haunts conversations. My fieldwork in South Africa had been spread out over nearly a decade by the time I was reading about Vernon's death, so it is unsurprising that some of the older people I encountered in my fieldwork had passed. The volume of younger people, though, is striking from a middle-class American perspective. I cannot calculate how many interlocutors have died in that time, as I have lost touch with many acquaintances over the years. More viscerally, though, when I return to the field, many reintroductions start, as my conversation with Bhuti did, by talking about those who have died in the preceding months or years. This is never a purposeful conversation starter on my part. Rather, it is often an outcome of asking otherwise anodyne questions friends use to catch up with one another. We typically share news of change, and change in some of my circles can involve death.

This may seem like a mordant fascination that plays up hackneyed tropes about disease and dying in Africa.³ To be clear, my goal is not to reduce the extraordinary complexity of life in South Africa merely to the experience of death. Rather, I am asking from the vantage of a place where death intrudes regularly into the text of daily social life, how fieldworkers can write about one of the few universal human experiences. To put it differently, death's universality raises a set of universal challenges for fieldworkers, even as South Africa's high mortality rate shows the abhorrent ways in which death is experienced unequally across lines of race, class, and nationality.⁴ To

that end, in what follows, I raise two issues that have emerged from my research in South Africa over the years. First, I discuss challenges that come with trying to answer seemingly factual questions about death. Second, I raise ethical questions about how to write about those who have died, since the dead cannot speak for themselves or challenge how they are represented.

Two lessons emerge from these concerns for the practice of member-checking. First, attention to the politics of death suggests the need to think beyond the practice of verifying facts or confirming whether one's interpretation of an event is "correct" when member-checking. Instead, I suggest that attention to disputes about facts might sometimes be more revealing of local politics than the facts themselves. Second, I advocate for representing those who have died—even those who performed deeds in life that a researcher may find troubling—with critical empathy because the dead cannot speak for themselves. As I argue below, this does not mean agreeing with how one lived. But, by writing about someone's life within a thick context, it may help us understand why they lived as they did.

The Facts of Death

At first glance, death may seem to be about as fact-laden a social experience as there is. Partially, as a hackneyed joke laments, this is because death is as certain as taxes. And, as with taxation, death has been a constant concern of states over time. Indeed, scholars have shown that governing death was a major factor in the birth of modern states as nascent institutions tried to become sovereign over death, whether through the imposition of quarantines to regulate state-destroying plagues (McNeil 1976) or through the imposition of a justice system to regulate homicide (Lockwood 2017). The political necessity of governing death was eventually matched by a profusion of facts about death. Mortality rates, epidemiological statistics, homicide counts—all are measures of states' obsession with counting death and of the political importance in doing so, much like states obsess about collecting data for taxation.

Given the political importance of death-related data, the facts of death might seem to be a logical place for a fieldworker examining the politics of death and dying to look first. During fieldwork, however, I have found that the facts surrounding death may be in dispute, and the

² He had admitted as much in a court filing prior to his death as part of an application to interdict the police from arresting him (Govender 2015).

³ See Wainaina 2006.

⁴ Although they have thankfully decreased over the last decade as the South African government has rolled out anti-retroviral medications to combat the HIV epidemic, the country's mortality rates are exceptionally high and are at higher levels than when the country democratized in 1994. <https://data.worldbank.org/indicator/SP.DYN.AMRT.MA?locations=ZA>.

terms of the dispute may reveal more about politics than about how someone died. Take perhaps the first question a researcher might want to answer: how did someone die? The question seems straightforward, but in practice can be complex. For example, as I have performed fieldwork with young men involved in various illegal industries, the reasons given for their deaths are often varied. The most common response to a question about how a young man died tends to be, “He was sick,” or on occasion to show three fingers, signaling South Africa’s “three-letter plague”: HIV (Steinberg 2011). With some frequency, the reason given is police violence (Smith 2019, 191-212). In other instances, violence by other young men is suggested. Sometimes suicide or a car accident is mentioned. At times, witchcraft may be rumored to have played a role in the death (e.g., Ashforth 2002).

Generally, though, the answers are inconclusive or contested. The reasons for death are often subject to rumor, gossip, or outright misinformation, responses that might seem at first glance like “useless” data given that such responses do not communicate valid “facts.” In reality, such “false” responses are deeply consequential for understanding local context and how one’s interlocutors see and navigate it (Fujii 2010).

For instance, during one fieldwork trip, I was standing with a group of neighbors on the street where I used to live on a Saturday afternoon. I could hear gunfire coming from down the hill. It wasn’t violent. Rather, it was a gunshot into the air—a typical “salute” for a fallen gangster at his funeral. I asked the guys who had died. They replied with the name of a locally notorious young man whom I didn’t know. I asked how he had died. This basic factual question provoked conflicting, although revealing, answers. Everyone “knew” that his death was the consequence of a botched home invasion in a wealthy suburb. The question was *who* killed him. One of the young men claimed it was the homeowner himself, telling us that the owner had pulled a gun and surprised the gangsters as they were trying to sneak up on him as he got out of his car. His “evidence” was that he had seen a video of the young man’s death on someone’s cell phone. “You can really see the power of the gun” in the video, he said, before exaggeratedly acting out how the young man’s body flew backwards, as if in an action movie (Jabulani, pers. comm. recorded in field notes, August 15, 2015). The other men looked dubious and I pulled out my phone in an unsuccessful bid to find the video on YouTube, as such footage is sometimes posted online. Another interlocutor who knew the deceased particularly well contradicted the initial account, saying

instead he had died during a shootout with the police as he tried to escape the scene of the crime.

Later, in thinking through the conflicting answers, I decided that trying to ascertain the facts of how this young man died was unlikely to lead to a conclusive answer. Still, the different accounts were significant for understanding the relationship these young men have to their own lives, to their class positions, to their places in South Africa’s post-apartheid racial order, and in their relationships to the state itself vis-à-vis the police. In other words, the lack of clarity about the facts—the debate about the facts and how young men engaged one another—was important “data.”

There are several different possibilities for interpreting the dispute about this young man’s death which shed light on member-checking. One is that when people tell conflicting stories like this, particularly if one version of the story seems improbable (as most of the young men seemed to think of the account of the homeowner shooting), it may be because people are telling stories that “work, that convey ideas or points” (White 2000, 30). That is, in a dispute or argument a “false” story may convey something “true” about the subject under dispute. One way to read the dispute about whether it was a homeowner or the police that shot this young man, for instance, would be that the disputants are conveying different facts about the dangers that young men of color face in South Africa’s primarily white suburbs and the ability of both private citizens and the police to kill.

Silences or omissions in stories can be similarly important (Fujii 2010). What was unsaid, but universally understood in this conversation, for instance, was that nothing would happen judicially related to this young man’s death, regardless of who actually killed him. And, if the young man had been killed by police, the silence about the probable lack of judicial attention suggested a distant and uncaring state that had little regard for the lives of young black men. For the man who suggested the gangster had been killed by police, for instance, I took his account as evidence that he viewed the state as murderous, given that I had discussed this individual’s sense of vulnerability in the face of police several times previously and as we had been discussing a controversial and ultimately unsuccessful attempt to prosecute police officers for allegedly illegal killings. Indeed, I had heard similar accounts so many times over the years working in this neighborhood that I had come to realize that for many young men, rather than the state appearing as a protection racket (Tilly 1985), they saw the state as something akin to a large-scale vigilante group that had

little interest in protecting them (even in a double-edged sense) because it was focused on killing them instead (Smith 2019).

The more general methodological point is that trying to ascertain the “true” facts may obscure other types of “truth” that are more readily available to a fieldworker, and more revealing of local politics, but are nonetheless resistant to checking the facts of an account. Trying to determine the facts of death through a process of member-checking to the exclusion of understanding competing “facts” may have the unfortunate consequence for a fieldworker of ignoring the “truths” that disputes, falsehoods, rumors, lies, and silences might reveal about the facts of life as one’s interlocutors experience them (Fujii 2010). To take these multiple “truths” into account, one might think of the process of member-checking less as a process of determining the final truth of an event and more as a process of accounting for the multiple understandings interlocutors hold of an event and what those multiple understandings reveal about how interlocutors understand and navigate their political worlds.

Condemning the Already Dead

Checking the facts of death are not the only issues surrounding work on death and member-checking, though; the ethics of studying death are also fraught, given that one is unable to “check” with someone who has died. Where engaging in member-checking with living informants affords them agency to clarify or revise their thoughts at a later date, the dead have no such power, even as they may live on in field notes, interview recordings, head notes, or published work.

This ethical dilemma is particularly fraught when it comes to writing of the dead because of two broadly held, albeit opposed, ethical approaches to representing those who have died. Many have a commonsense that one should not “speak ill of the dead,” an ethical imperative rooted in the recognition that the dead cannot speak for themselves. Others maintain that the consequences of our actions may outlive us, which requires factual accounts of the dead even if those facts are unflattering as they may provide moral lessons for the living. How can a researcher navigate these two contrasting ethical imperatives?

To address this question, I would like to return to Vernon, whom we met above. Reactions to Vernon’s killing broke across these two ethical poles. His sister, for instance, denied to the press that he was a drug dealer, insisting: “My brother was a successful person. He

ran a successful taxi business and owned a sports bar” (Somduth 2016). She went on to cite how he had just spent R50000 (about \$4000 at the time) to buy groceries to distribute to community members as a Christmastime charitable act. Not all public remembrances were so glowing. About a year and a half after he was gunned down, for instance, a local columnist cited Vernon as a prime example of why “We Should Stop the False Praise for the Deceased” (as his headline put it) (Devin 2018). As the columnist wrote, when Vernon was killed “the media was justified in denouncing him. After all, he will be most remembered for destroying lives through the sale of drugs. You cannot make a silk purse out of a sow’s ear. If you are bad, then you are bad” (Devin 2018). For the columnist, making it apparent that some people are bad is crucial because how we understand the dead impacts how we see our own lives: “It is important to state factually the deceased’s strengths and weaknesses; one can learn wonderful lessons from both... Remember that death does not erase bad acts. If you want people to say good things about you when you are gone, do only good things when you are alive.” This ethical imperative emerges from a basic fact: our actions have consequences even after we are gone. Quoting Shakespeare, he writes, “The evil that men do lives after them. The good is oft interred with their bones” (Devin 2018).

Both positions here—what might be called the reverent position and the factual position—present an essentially binary view of our representational obligations to the dead. On the one side, Vernon’s sister refuses to acknowledge his alleged crimes, focusing only on the public services he performed. On the other side, for the columnist, Vernon was nothing more than a “scoundrel” (Devin 2018). A binary representation of the world, though, is not typically useful for subtle works of social science, given that the goal is typically understanding, rather than judgement.

To break free from this representational binary, I propose an alternative goal for representing those who have died: depicting their lives with critical empathy. By critical empathy, I mean trying to understand the actions one took in life in the context within which one lived, while also approaching those actions, the context, and one’s own emotional reactions to the person with reflective distance. Approaching those who have died with critical empathy does not mean blindly celebrating them or ignoring misdeeds. Nor does it mean trying to represent their lives in a straightforwardly factual way, as any representation already assumes one has selected certain facts to represent their lives to the inherent

exclusion of others. Instead, the goal of critically empathizing with the dead is understanding.

How can one achieve this goal? One approach for viewing the dead with critical empathy would be for the researcher to “thicken” the context in which they lived to help readers understand the complex social world the deceased navigated. This might involve approaching the dead with an “ethnographic sensibility”—seeing through the eyes of another, to the extent possible, to understand how they lived in the world and why they did so in a particular manner (Pader 2006). By placing the dead within a thick social and political world and trying to understand how they navigated it, one may be able to avoid the Scylla of celebratory depiction and the Charybdis of a “fact-based” moral accounting, while giving readers a sense of the complex and often contradictory worlds our interlocutors inhabit. In this sense, starting with a sense of critical empathy can help us understand context, which can aid in explicating actions.

Of course, empathizing with the dead is not without its own ethical dilemmas. It may be difficult to empathize with someone like Vernon who was allegedly responsible for much pain while he was living (see e.g., Blee 1993, 1998; Gallagher 2009). The difficulties of empathizing with someone whose acts a researcher may morally reject also presents representational, inferential, and interpretive dilemmas, as barriers to empathy may affect how we present others (Shesterinina 2019)—a dilemma that is compounded when the deceased are unable to respond for themselves. In such circumstances, “emotional reflexivity,” (Shesterinina 2019), in which one constantly checks one’s own responses toward the research subject, is particularly important for making sure that one’s writing does justice to the frequently multiple,

sometimes shifting roles subjects inhabited in their lives—in Vernon’s case, an alleged drug dealer who was also a brother and father. Such reflexivity can help create space for critical reflection on one’s interlocutors and the actions they took within the contexts they navigated to help us provide broader insights from particular cases.

After all, as we saw with the reactions to Vernon’s murder, death is often a polarizing, stocktaking moment. This places a particular ethical burden on the researcher because the researcher may act—intentionally or not—as an arbiter of the “truth” of the deceased’s life. In this regard, it is important for authors to remember that understanding one’s life does not necessarily mean agreement with how someone lived it. Yet, given that the dead cannot speak for themselves, a scholar speaks for them. This places a responsibility on the writer to seek empathy and to provide as richly realized a portrait as possible of the world in which they lived and in which they made choices, even as those choices may have been discomfoting.

In this sense, death presents an ethical dilemma for a researcher, particularly since member-checking with the deceased is not possible. Verifying facts about a deceased person’s life or checking the veracity of claims they made while alive is impossible through a process of member-checking. Even more, to write about someone who has passed away is to recognize that one has a certain power over their life because one has the power to represent them to the world.⁵ And, it is precisely because the dead cannot speak for themselves during a process of member-checking that the ethical burdens on a researcher are increased, giving extra responsibility for illuminating the context in which that person lived and to be reflexive when doing so.

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⁵ As historians have written when encountering the dead in archives, this relationship also rests on a certain intimacy, even if one has never met the person one writes about (see, e.g., Farge 2013).

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Innovative Data Collection and Integration to Investigate Sorcery Accusation Related Violence in Papua New Guinea

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Despite global outrage at several widely-publicized, extreme incidents of sorcery accusation related violence (SARV) in Papua New Guinea (PNG), research into SARV has been largely limited to ethnographic accounts, with little done to document its prevalence or the responses that prevent or limit its occurrence. This paper describes an innovative and collaborative approach adopted to generate and integrate data for a mixed methods study of SARV. This project has built two significant new datasets and collected extensive qualitative data through interviews, focus groups, and participant observation in workshops and meetings. We describe our participatory, collaborative, and ethical approach, and why a mixed methods research (MMR) design was essential. The key data generated by the project is explained, with special attention given to the most innovative and vital element of the project: the complex and detailed incident data collection in selected locations. The subsequent section summarizes how the principles of grounded theory are helping to develop and revise conceptual and thematic strands across multiple sources of data, and the practical use of spatial-temporal coding to link and compare different sources of data. Several examples of preliminary findings are provided in order to illustrate the analytical advantages of the project's MMR design and collaborative approach. The final section acknowledges the limitations of the study design and the ways these are being mitigated.

The Context and Its Methodological Challenge

Sorcery and witchcraft beliefs and practices, and subsequent violence against those accused of witchcraft, generate a range of law and order problems in the world, especially in the global South. In 2009, the Special Rapporteur on extrajudicial killings concluded that at a global level, "there is little systematic information on the available numbers of persons so accused, persecuted or killed, nor is there any detailed analysis of the dynamics and patterns of such killings, or of how the killings can be prevented" (UN 2011, 13). Although the common trend in the literature is an assertion that the number of cases is on the rise and that these cases are becoming more violent (Adinkrah 2004; Gardini 2013; Jorgensen 2014), the difficulty of proving such claims quantitatively is acknowledged. In fact, there remain considerable gaps in knowledge about the extent of SARV, whether it is increasing in severity, what interventions are effective in curtailing its severity, and why (Forsyth 2016).

Undertaking research on SARV is challenging for numerous reasons, including community complicity (meaning such crimes often do not come to the attention of the authorities), fear of re-victimization (meaning that victims often do not report the reasons behind the crime to justice agencies or hospitals), and the generally scattered and incomplete nature of official police and court records in the country, which often do not record a crime in relation to the motivations, such as SARV, that

lie behind its commission. Other potential data sources, such as official health records, are also of limited value, as victims are often afraid to disclose the reason behind their attack for fear of further violence or stigmatization.

PNG poses particular challenges because of its difficult terrain, language and cultural diversity, and poor reach and reliability of data collection from the government services that respond to SARV. PNG is a country of great geographic and socio-ethnic diversity, with at least 800 languages and a rapidly rising population of more than eight million people. Most of the population (over 80 percent) live in rural and remote areas, which are difficult to access. A country rich in natural resources, the benefits of this wealth are very unevenly distributed, and are most evident in the urban centers, most notably in the capital city of Port Moresby. In a country with a weak or fragile state, high levels of corruption, private and public violence, and an eroding public sector, it is extremely difficult, and sometimes dangerous, to conduct research.

The vast majority of literature on SARV in PNG is qualitative in nature, and most is localized, with no large-scale quantitative studies, except for an analysis of newspaper reports (Urame 2008). The majority of research in this field is ethnographic, an important source of information which demonstrates that although much has changed in a constellation of beliefs and practices, there are certain core continuities from the past, including widespread but often diverse beliefs in witchcraft and sorcery (Eves 2010; Schwoerer 2017).

In terms of the prevalence of SARV in PNG, official records are fragmented and often incomplete, and those that do exist are fraught with the potential to be misleading. Incomplete and fragmented data also distorts the characteristics of SARV and the association between SARV and related factors. As we demonstrate later in this paper, there are significant variations between regions in the characteristics of victims, often based on different cultural traditions and particular local history, which problematizes any analysis about sorcery at a national level.

In order to better address the serious harm caused by SARV, there is a clear need to develop a better understanding of the scope of the problem, its various dimensions, provincial variations, and trends over time. One of the main challenges is overcoming data availability and finding new ways of accessing and collecting data that accurately quantifies and describes SARV. The other major challenge is accurately identifying and connecting related social events and their impact on SARV and

building an evidence base of current and promising interventions that can inform future efforts to overcome SARV in PNG. The importance of an evidence base to underpin policy development and advocacy programs in this area is also recognized in PNG's SARV National Action Plan (NAP), developed in 2014 and endorsed by the national government in 2015.

The Aims and Methodology of the SARV Project

The current study commenced in November 2016 and runs for four years. The project is a collaborative partnership with academics from the Australian National University, Divine Word University, and the National Research Institute in PNG. Local researchers and data collectors also play a crucial role in gathering information. The main research questions are:

1. Who is being accused of sorcery, where, why, how often, by whom, how does this change over time, and why?
2. Why do accusations lead to violence at certain times and not others?
3. What regulatory levers exist to overcome sorcery accusation related violence, and what context or conditions are necessary for them to work effectively?
4. How is the SARV NAP working as a coalition for change network? What are its impacts, failures and challenges?

These two main requirements of the project (i.e., accurately describing and *measuring* events, and *interpreting* and influencing social meanings) call for different methodological approaches. Reporting on the prevalence of events calls for a positivist approach, which considers “social facts as things,” taking on an objective and therefore measurable character (Durkheim 1938, 14), while understanding the “meaning” of social action in order to explain it calls for a constructivist approach (Weber 1949). In other words, a research design is needed that can reliably quantify events, while accounting for the subjective beliefs and norms informing the actors involved.

To respond to these requirements, a convergent MMR design, combined with a collaborative team approach that draws on both qualitative and quantitative sources at national and sub-national levels, is used. Describing a social phenomenon as complex as SARV using numbers is highly challenging. Yet, in order to organize and summarize our knowledge of SARV, and facilitate decision-making, we require data in countable terms (Engle Merry 2016). While quantitative data conveys an

“aura of objective truth” despite the “interpretive work that goes into their construction” (Engle Merry 2016, 1), its value without context, history, and meaning is limited. To analyze how sorcery, witchcraft, and violent responses to accusations are understood in PNG, what they signify to people, and how these social meanings relate to and are influenced by current and historical processes, quantified data needs to be integrated with qualitative information to uncover the context-dependence of constructed meanings (Bazeley 2017). Adopting an MMR design also improves the transferability of our findings to other settings, and is consistent with the academic ideals of scholarship (e.g., Bergman 2008; Creswell and Plano Clark 2010; Tashakkori and Teddlie 2010).

There are several key dimensions to the methodology that coalesce into a collaborative, participatory and ethical approach. The collaborative and participatory approach is reflected in every facet of the study, from the composition of the team leadership and its members’ engagement with the SARV NAP, to the critical role of local people who collect data through what we term the “recorder networks.” There has been an ongoing refinement of the methods and tools through the feedback provided by the recorder networks. Our ethical obligations mean that the safety of those being interviewed and consulted, and of those doing the research, are prioritized. More is said on the protocol that was developed to minimize risks for the recorders, those who provided information, and those who have been or could be accused of sorcery in a forthcoming paper. The research design relies heavily on the lead academics and team leaders’ knowledge of risks, especially at a local level. There is constant monitoring and review of how the study is going, with a focus on the sites where incident data is being collected. At the outset, the Australian National University’s Human Research Ethics Committee scrutinized and approved the study’s approach and design and drew attention to the risk factors involved. These are discussed further in a forthcoming paper that describes in detail the development of the recorder network.

Multiple Data Sources

The initial phase of the project involved a comprehensive review of relevant literature and reports, discussions generated by three workshops in PNG and Australia on SARV in 2013 and 2014 (see Forsyth 2013a; 2013b; 2014 for more detail), consultations with key stakeholders (including members of the SARV NAP committee), and an assessment of the types of statistics and data collections in PNG that may have assisted with

the project. It was found that existing data sources were limited on a number of fronts, including in terms of accessibility, coverage, and reliability. A very partial and disjointed assembly of potential data included judgments reported on the Pacific Islands Legal Information Institution website (PACLII), which are primarily restricted to National Court decisions and higher courts; Village Court quarterly reports; selected police records; NGO reports; and data recorded by the offices of the public prosecutor and public solicitor.

None of these data sources regularly focus explicitly on SARV, and only in some instances, such as media reports, is it clear that SARV is being identified and described. Village courts’ quarterly reports only have the category “sorcery” to report, and this may be interpreted very differently by different clerks—it may mean offences that involve sorcery under the village court regulations (such as “practicing or pretending to practice sorcery”), or it may mean those cases where other wrongs or crimes are committed due to sorcery accusations. These data are therefore of little assistance as it is not possible to disaggregate the cases where sorcery concerns provoke violence (SARV), and cases where concerns about sorcery are brought before the court to manage. Similarly, the higher courts and police records relate to criminal offences generally, and it is only through a review of case files or through interviews with police or magistrates that the link between violent crime and sorcery accusations becomes apparent. Upon the evaluation of these potential data sources, it became apparent that different collections pick up different cases. Thus, it was decided to retain all data sources, but only report from them when integrated with, or related to, other data sources for a particular geographical coverage.

The initial phase demonstrated very clearly the need for the project to generate primary data, especially that which could meet several basic requirements in terms of volume and coverage over time and geography. Two key datasets have been developed and continue to be added to and expanded. The first focused on trends in cases of SARV reported by national newspapers and courts over at least a twenty-year period, and the second, which is the cornerstone of the project, is the collection of information on incidents of accusations of sorcery that result in violence, and those that do not result in violence, in a number of locations across PNG. More is said on these two collections below, along with a final section on the wealth of qualitative material being gathered as part of the project.

Media and Case Law Analysis

One of the important quantitative data sources to estimate SARV in PNG is the analysis of national media and case law from 1996 to 2016. Newspapers currently provide the most comprehensive dataset of SARV in PNG. Because of the unevenness of journalistic coverages, there are serious drawbacks to relying on media analysis alone. However, when triangulated with a range of other data sources and methods, it provides a unique, if far from comprehensive, account.

An earlier study by Urame (2008) of SARV in PNG over a seven-year period also used media analysis. A similar but more comprehensive approach was adopted for this project. The dataset comprises articles from two national newspapers: *The National* and the *Post-Courier*, and national court cases reported on PACLII over a twenty-year period (1996-2016). It was supplemented by searches of other media through the online FACTIVE database. The bulk of the unique cases were identified through newspaper articles (n=418) and when 51 national court cases were added, the total number of unique SARV incidents in the dataset was 452 (each incident often had a number of different reports). There was an overlap of only 17 cases when comparing these two sources, which speaks to the partiality of the datasets.

Incident Dataset

The above described nationwide newspaper and court case dataset is supplemented by the creation of a dataset of incidents of sorcery accusation in a number of selected provinces between 2016 and 2019. The multi-layered and cross-sectoral nature of the project means that the scope of the research is national. Yet, PNG's topographical obstacles and poor infrastructure makes a comprehensive data collection extremely difficult, especially in rural and remote areas. Accordingly, three locations (Enga, Bougainville, and Port Moresby) were selected as the first tranche of study sites to collect in-depth quantitative and qualitative data on SARV incidents.

Developing this dataset required the development of a new instrument to collect quantifiable data on SARV. In the initial stage of instrument development, to ensure content validity, the team members had extensive dialogue with key informants, research partners, and scholars in the fields of criminology and anthropology. Next, a pilot questionnaire was administered to collect data, and feedback was sought on the pilot form from a broad range of stakeholders. Feedback and suggestions were considered and implemented when deemed to be

appropriate. This included rewording, adding new items, and revising predefined categories. Clear specifications in instructional protocols and on the form are included to ensure that the same thing is being considered when reported. This was also ensured through translating the form into Tok Pisin, which revealed areas of ambiguity that needed to be addressed in the English version of the form. Pre-coded categories for responses was another strategy used to improve data reliability and consistency. These fixed categories had to be descriptive, specific, and straightforward. A preliminary sample of the collected data was coded and entered into SPSS to test how it would perform, which resulted in further fine-tuning.

Incident forms are completed by a network of data collectors recruited from the local community to reduce the understandable distrust of research and outsider researchers. Gaining access to sensitive information is one challenge; the other is obtaining as much information about the incident as possible and recording it in a consistent manner. Instead of recording a single person's experience or recollection of an incident, recorders are instructed to talk to a range of witnesses to collect as much information about the incident, victims, and accusers as possible before completing the incident form. The benefit of this approach, as opposed to interviewing a single person about the incident, is that more comprehensive information is obtained, minimizing:

- Multiple reports of the same incidents by various actors;
- Underreporting of incidents, due to the general and established issue of underreporting of crime experienced by victims to interviewers; and
- Missing information in data fields. Typically, different people know different aspects of the data collected; for example, one person may have more knowledge of the demographic characteristics of the victims, while another may know more about whether those accused of committing the violence were charged.

A limitation of this approach is the reliance on the recorder to synthesize information collected from multiple sources, introducing the potential for individual bias through different ways of synthesizing information, or giving more weight to one source than another. We have tried to account for this by developing clearly specified protocols on how to collect and synthesize information, and providing regular training to data recorders. For example, we have a sheet of written general instructions about how to fill out the forms. The first part contains

very basic information, such as “always try to speak with at least two and preferably many more people about an incident,” and “always write ‘don’t know’ if you do not know the answer rather than leaving it blank.” The second part contains detailed instructions about particular questions that we identified as causing problems, together with screenshots of what correct and incorrect answers look like. This information is then orally imparted to the recorders by the lead recorder in each province during training sessions that have been held at least three times with each group since the project commenced. The data recorders are all literate in Tok Pisin and many in English as well, but none has a university education, while the lead recorders are all significantly more educated and fluent in English. The lead recorder is also responsible for checking over the data before it is sent to be entered into the database, in some cases requiring the recorder supply further information, or to fill out the form again if it is unclear or contains logical inconsistencies. Additionally, data is collected on the level of agreement between the people that the recorders have talked to. Recording the level and detail of disagreement between these voices also highlights cases with a risk of poor reliability due to the accuracy of recollection by the people interviewed.

There are two incident forms designed to capture information on victims, perpetrators, state and non-state interventions, and responses to the incident. The first section in both forms focuses on the accusation of sorcery, while only the second form records details about the violence that followed from the accusation of sorcery. The dataset is designed in a way that data can be analyzed either at an incident or a victim level, as many incidents have more than one victim, and supports quantitative analysis to identify factors which correlate with, as well as predict, accusations of sorcery leading to subsequent violence.

Qualitative Data

Semi-structured (and in some cases, more free flowing) interviews are also being conducted with a broad range of stakeholders who hold official and non-state positions or deliver services at a provincial, district, or ward level, as well as some survivors, with an initial focus on the three locations where the incident data collection was first established. To date (in just under two years), more than 180 interviews have been conducted and recorded, as transcripts or as detailed notes. This cross-sectional sample includes survivors and perpetrators, those working in the justice system or for non-governmental organizations, and at the village or

neighborhood level, church and village leaders.

These interviews have been complemented by some participant observation of training, workshops, and other events, as opportunities have arisen. One of the project’s key aims is to document as many local initiatives to prevent or minimize the violence that stems from accusations of sorcery as possible. Participant observation and a series of interviews with key personnel also help inform the description and assessment of SARV NAP as a coalition of change network. Qualitative data were collected using digital recorders and through filming when possible, but as a degree of caution was required, in certain situations, conversations with participants were more open-ended, and not recorded. A geo-reference in the form of *ward* and *ward number*, or *urban community* in the case of Port Moresby, were noted by interviewers in the attached metadata. Qualitative data is being explored through a narrative analysis using NVivo.

Finally, information from social media (i.e., those who are linked on Facebook with key organizations and individuals involved in SARV in PNG, or who use Facebook to raise awareness of activities and advocacy events or to coordinate rescues) is used to alert us to intervention initiatives and incidents that have not been flagged by our recorder network.

Integration and Analysis of Data Sources

With an extremely difficult and under-explored research topic such as SARV, our study has to draw on the principles of grounded theory. Multiple sources of data are collated and coded to enable inductive analysis and the development of conceptual themes in a reiterative process. This process is performed through constant comparative analysis, moving back and forth with increasingly focused attention to themes within and across the data. Through various collaborative and specific mechanisms, one of which is elaborated on below, the process involves gradually linking initial codes or nodes into progressively abstracted higher level categories and conceptual themes (Charmaz 2006; Glaser 1992; Glaser and Strauss 1967), which are re-tested and adapted in a process not dissimilar to that advocated in Layder (1998). We are seeking to be rigorous while staying true to the context which is generating data, staying open to possibilities of new ways to theorize or conceive of SARV, and efforts to address it. A crucial dimension to the process is the regular reviewing of emerging themes and theoretical focus within the research team and with external stakeholders and academics. For example, an annual workshop organized by the funding body, Pacific

Women, is held each year in Port Moresby with NGOs encountering SARV in their service delivery, and with academics involved in film documentation of those who challenge SARV and assist survivors (Stop Sorcery Violence, 2020).

On a practical level, as a means to integrate different types of data from multiple sources, we are “anchoring” the range of data through spatial-temporal linkages. Data is related and linked through a common geographical location to produce spatially related and linked information. Coalescing the data from different sources, relating to the same geographical space, can show convergence, as well as variation in results. Finding the same results and gaining empirical support using different methods and data sources can strengthen the reliability and trustworthiness of findings. Contradictory findings, on the other hand, can be conceptually illuminating, and can lead to refined research questions and new conceptualizations or theoretical redescription of existing constructs (Bazeley 2017).

Findings from the data sources listed above are integrated and analytically linked by applying the spatial reference of each information source. Findings are also spatially related to contextual secondary data, such as the census, using ArcGIS software. A temporal and spatial matching of these secondary data accommodates the exploration of how socio-demographic, legal, civil, and religious society relate to, frame, and directly or indirectly impact SARV incidents.

Our systematic approach to data collection and analysis is captured in Figure 1, which displays the relationships between the data, analysis tools, and exemplar outputs. Figure 2 shows how the data collection and analysis is part of the broader, iterative spiral of adaptive theory building. At the heart of the process, driving the spiral, are the four key research questions.

Figure 1: Sorcery accusation and related violence in PNG: Data sources, software tools, and data integration and analysis.

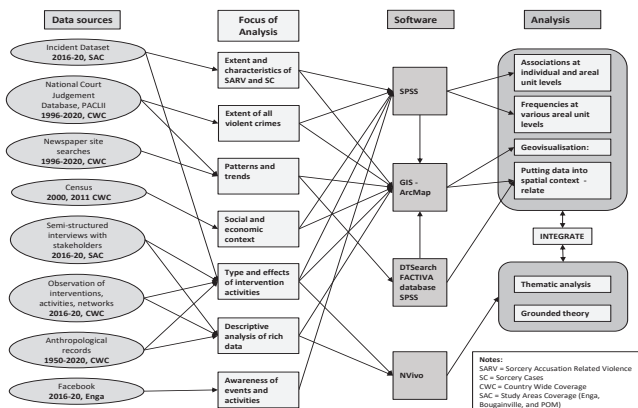
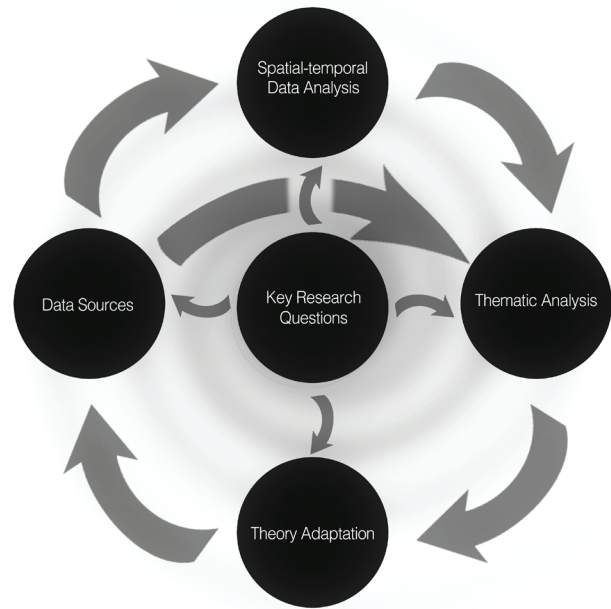


Figure 2: Spiral of adaptive theory building.



Discussion Including Preliminary Findings

Eighteen months after the commencement of the project, for the incident data collection, 357 questionnaires have been completed, resulting in detailed information on 240 non-violent and 117 violent incidents of sorcery accusations in the three study provinces. So far, a number of issues have emerged that warrant further attention and investigation. First, the volume of victimization and the number of injuries and deaths from SARV across the different locations in the country are very concerning. Second, in the majority of reported incidents, there were multiple victims and often extreme violence involved, including torture as well as ongoing insecurity and psychological damage. Third, there are significant differences in the way male and female victims are harmed, with the latter sometimes raped and more likely to be burnt and tortured than male victims. Fourth, this is often a type of mob violence which involves large groups of perpetrators.

Our unique MMR design, combined with a collaborative team approach, provides a valid and credible dataset for monitoring SARV in PNG, and an enriched insight into the impact of state and non-state interventions on these violent incidents.

Our quantitative data collection allows us to establish trends and patterns in behavior and to find general descriptors of SARV events. Our systematic approach to the collection and analysis of information from multiple sources provides a robust foundation for

monitoring change over time, while comparing trends and relationships between indicators and between regions allows us to make some inferences with respect to the size and scope of the issue.

To complement these findings, our qualitative in-depth interviews generate detailed data on the perceptions, emotions, experiences, meaning making, and behavior of actors. It provides insight into how people make sense of or rationalize external events, and their motivations for specific behaviors. It also captures different interpretations, dynamics and norms in different regions. As demonstrated by Searle (1996), social realities are humanely created, and the continued existence of institutional or other group practices lies not only in the beliefs of the individuals directly involved, but also a sufficient number of members of the relevant communities.

While using different methods allows us to investigate different components of the research question, the real benefit of using MMR is bringing together and integrating findings from these two approaches to develop insights into SARV. The integration of data from multiple sources and the use of various techniques means that information included from a range of participant groups (witnesses, victims, perpetrators, family members, NGOs, officials) enhances the validity of the findings.

Findings from different methods have importantly complemented each other and also usefully suggested other avenues of inquiry and analysis. For example, identifying the gendered dimension to SARV has been a critical but confusing component for which to account. The incident data collection has been critical in supporting, and being supported by, the media analysis and anthropological literature in identifying very strong gendering of victims based on location. Somewhat fortuitously, the two first provinces we focused on, Enga and Bougainville, have almost completely mirror images of each other in terms of women or men being targeted. In Enga, 96 percent of those accused of sorcery were women, compared to 4 percent in Bougainville. By drawing on the other data sources, we are able to question and interrogate what appears to be a gender bias in the state justice system towards supporting male victims, rather than revealing men as being predominantly the subjects of SARV.

Another example illustrates the ways in which mixed methods data collection enables the development and testing of hypotheses about our data in an ongoing iterative process. We learned from the anthropological evidence and from our interviews that SARV was a new

phenomenon for most of Enga, entering the province around 2010. We developed a hypothesis that one reason explaining the explosion of cases was a form of contagion relating to a particular narrative about women being possessed by evil spirits that cause them to seek out and “eat” the hearts of living people, causing them to become sick or die. This narrative is accompanied by a behavioral “script” about how to respond to fears about such women, which involves torture and interrogation. We were able to find considerable evidence to support that hypothesis by drawing upon the incident collection data, which revealed very obvious temporal and geographical clustering of cases, as well as a remarkable similarity in narrative associated with the accusation and the form of violent response.

Limitation of the Approach

Research of this nature has significant limitations. Often, data is partial, not always reliable, and never replicable. Much is hidden and where overt, not necessarily recorded with consistency. As noted earlier, the incident data collection is heavily dependent on individuals accessing and accurately recording data. Another limitation is the exclusion of more remote areas in most if not all data sources used in the study due to difficulties in accessing these areas. To correct for this, we have mapped out the collection sites in which recorders collect data, and these boundaries are related to spatial analysis and visualization of data. Data collection with spatial information attached provide a different level of accuracy and are geo-referenced to spatial boundaries which do not always align. These limitations mean that we are not able to produce choropleth maps. In addition, detailed in-depth accounts of SARV in geographic areas and among certain cultural-linguistic collectivities of people underline the diversity and specificity of beliefs and practices, and how they change over time (see e.g., Forsyth and Eves 2015; Zocca 2009). However, informing policy and support for national and local efforts to address SARV requires evidence that has a wider scope (in time and place), and multiple sources of information. Documenting how we are trying to do this is the first step in being transparent about methods and the tentative conclusions that emerge from the study, so that others can engage with us in debates about their significance and merit.

Conclusion

This article described the innovative, collaborative and evolving approach that has been adopted to study SARV in PNG. Our unique MMR design, combined

with a collaborative team approach, provides a valid and credible dataset for monitoring SARV, and an enriched insight into the impact of state and non-state interventions on these violent incidents.

It is important to note that while this methodology provides a reliable estimate to monitor the extent and type of SARV (and approaches to prevent it), it cannot be used to measure the absolute level of SARV at either the national or provincial level. All of our data sources are likely to underrepresent the true numbers of incidents to varying extents, and we do not know what this “dark figure” may be. However, it does provide us with some credible statistics in relation to incidents we can be reasonably certain at least occurred, although we cannot have full confidence in all of the details of the incidents themselves. Additionally, in the three study provinces, the triangulation of findings from different data sources reduces the gap between the actual and our measured levels of SARV incidents.

Along with this quantitative analysis, our rich, qualitative, in-depth interviews generate detailed data on the perceptions, emotions, experiences, meaning making, and behaviors of actors. It provides an insight into how people make sense of or rationalize external events

and their motivations for specific behaviors. It also captures different interpretations, dynamics, and norms in different regions. As demonstrated by Searle (1996), social realities are humanely created, and the continued existence of institutional or other group practices lies not only in the beliefs of the individuals directly involved, but also with a sufficient number of members of the relevant communities.

Importantly, by linking people’s behavior to social structures, institutions, and the changing historical context of PNG society, we gain a better understanding of the *relational powers* and *contingent conditions* producing and mediating SARV. After all, no social action can be understood without understanding the broader context in which it takes place. At the same time, people’s actions are never determined by structures alone; people can see, choose, or be forced to choose alternative actions (Danermark et al. 2002). Our design is set out to capture these relational interplays between social agents and social structures to study how social actions of committing violence, condoning violence, or standing up against violence despite the risks involved, are generated and produced.

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In Memoriam:**A Tribute to Kendra Koivu**

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[https://DOI: 10.5281/zenodo.3946830](https://doi.org/10.5281/zenodo.3946830)**Kendra was Everyone's Teacher**

Jennifer Cyr

University of Arizona

Kendra Koivu was a phenomenal scholar. She was an even better friend. She was whip-smart and had a delightfully dark sense of humor. Kendra made time to help you make sense of a budding research project or to commiserate about a personal struggle. She set aside her own problems to help you with yours. Kendra was reliable, tireless, and uniquely brilliant.

Last fall, we lost Kendra and all of her gifts to breast cancer.

In the pages that follow, Kendra's friends, colleagues, and students remember how magnificent she was. You'll find that many of the reflections in the introductory paragraph above appear, over and over, in their words.

Before turning to them, though, I would like to share my own story of Kendra's lasting impact. As our readers will recall, Kendra and I began as co-editors of QMMR. She was my colleague, of course, but she was also my friend. We bonded over many shared experiences: graduate school, new jobs on the tenure track, our love of mixed methods, QMMR, and, eventually, cancer.

When I was diagnosed with breast cancer in February 2018, Kendra had already been living with it for some time. Her first response to the news was to ask a series of questions about cancer types, drugs, and treatment regimes. I stood, mouth agape, unsure how to respond. Like a first-year graduate student, I felt clueless about the world I was about to enter. Kendra was my teacher. As with set-theoretic logic (Mahoney, Kimball, and Koivu 2009) or illicit crime syndicates (Koivu 2016), Kendra had become an expert on breast cancer.

We ended up undergoing the same chemotherapy regime. Her twelve-week cycle began shortly after mine came to an end. During that time, we attended the QMMR Section Business Meeting at the 2018 Congress of the American Political Science Association (APSA). There, we officially assumed the mantle as QMMR co-editors. It was a quick thing, our introduction to the section. But to me it felt very powerful. We stood there as two women, two junior scholars, and two cancer patients. We were both bald, although Kendra wore a stylish headscarf.

Kendra took her cancer in stride—living and even thriving with it for years. I did my best to emulate her example. When I found I could not work, Kendra was there to pick up the slack of our shared responsibilities. When I needed to unload about my fears and my pain, Kendra was there to listen. I'd like to think I provided her some comfort as well, but Kendra gave so much more than what she asked for in return.

I will never forget standing with Kendra, on that evening, in front of our peers. It is, without question, one of my proudest accomplishments as a scholar.

Kendra's legacy to scholarship and to the academy, as our readers will see, is indisputable. What I will celebrate most and remember always are Kendra's gifts as a friend and a teacher. She helped to empower me as I took on cancer, just as she empowered her students (see the pieces by Calasanti and Vera-Adrianzén in this tribute); her colleagues (see the pieces by Nelson-Nuñez, Brookes, and Niedzwiecki); her friends and co-authors (see Day's piece); and even her own mentors (see Mahoney's piece).

I miss Kendra. I will never forget her.

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Dance Lessons

Marissa Brookes

University of California, Riverside

On a warm night in Washington, DC in late summer 2014, Kendra Koivu and I sat next to each other at a large table inside the never-not-crowded restaurant-bar-café Open City, unwinding from a fiery day at APSA alongside a handful of other old grad school friends. We indulged in carefree chatter and swapped stories over drinks and a wild mushroom pizza about our then-new lives on the tenure track. Kendra was entering her third year at the University of New Mexico (UNM); I was starting my second at the University of California Riverside. Two or three beverages in, we began to wax lyrical about our mutual passion for qualitative and multi-method research. We then reflected on the fact that the two of us happened to get jobs in the Southwestern US, as had friend and fellow Northwestern PhD Jen Cyr, who was beginning her third year at the University of Arizona. The irresistible combination of methods enthusiasm and geographical proximity—along with conversations about a methods network between Kendra and UNM colleague Sari Niedzwiecki—inspired the four of us to co-found the Southwest Workshop on Mixed Methods Research (SWMMR), an annual (and now international) conference devoted to discussing the theory and practice of mixed methods in the social sciences.

Just over a year later, in November 2015, Kendra and I once again found ourselves seated side-by-side at a large table, this time in a packed conference room in Albuquerque, about to kick off the very first SWMMR with co-founders Jen and Sari. We were all assistant professors. We all still had something to prove. Right before our formal introduction, Kendra, as if sensing my tension, leaned over and whispered to me, with a facetious dramatic flourish, “I’m going to open by telling them, ‘On that fateful night, we shared a pizza ... *and a dream!*’” I burst out laughing. I relaxed. Everything was going to be fine.

Kendra was hilarious. She had a way of injecting levity into tense situations with a signature humor that ranged from dark to absurd. She knew how to make fun of something while at the same time taking that exact same something completely seriously. Her approach to much of life struck me as somewhat akin to the advice she once gave me about dancing: “Dance like you’re making

fun of someone else.” I’ve tried it. It works. It turns out, if you let loose and abandon your self-conscious preoccupation with correct form, if you relax and stop taking yourself so damned seriously, you can actually be an excellent dancer—or political science researcher, or teacher, or mentor, or conference organizer, or cancer fighter.

And maybe that is what I liked best in Kendra as a friend. Neither of us was a born dancer. We grew up without the financial advantages that some of our better-off peers seemed to take for granted. We got into graduate school and basically just had to figure it out. Kendra was two years ahead of me in the program when I began my first year at Northwestern in the fall of 2005. We bonded over not coming from privilege, though Kendra had been through so much more than I could begin to understand. I sought her wisdom on nearly everything: how to handle coursework, the job market, grad school social norms, teaching, dissertation writing, and imposter syndrome (long before I had ever heard that phrase). She cheered me on every step of the way, like a big sister who was also a role model who was also my colleague who was also my friend. Kendra selflessly offered others her time and energy—and books. I still have her copy of *Bringing the State Back In*.

Kendra loved co-organizing the SWMMR. She was instrumental in ensuring its success year after year as our growing methods workshop bounced from Tucson to Riverside to Santa Cruz. So many of our lively discussions at the first four SWMMRs about causation and case selection came from Kendra’s careful commentary on others’ papers, combined with her own deep knowledge as a methodologist. She loved the debates, but above all she loved connecting all of these people: past participants with SWMMR first-timers, junior scholars with seasoned seniors, qualitative scholars with their quantitative counterparts. Kendra left us just weeks before the fifth SWMMR in Mexico City, but not before vetting every abstract, reading papers, and helping coordinate the conference logistics with the same force of passion and excitement she had from day one.

The last time I saw Kendra Koivu was on a warm night in Washington, DC in late summer 2019. We sat next to each other at a large table inside a hip ramen restaurant,

unwinding from a long day at APSA, alongside a handful of other old grad school friends. We indulged in carefree chatter and swapped stories over drinks and dumplings. Most of us had meant to attend the QMMR reception, but we came here instead, perhaps instinctively opting for a more intimate and exclusive gathering because we knew it would be the last one like this. That night we

talked about tenure, travel, cancer, children, spouses, and friends. That night we joked and laughed and kept it light but somehow also dug deep into the serious stuff. That night we lived out a shared vision of focusing on what matters most: our loved ones, camaraderie, human connection, and terrible jokes. That night we shared some dumplings...and a dream.

No Causality without Correlation: On Learning from Kendra Koivu

Anna Calasanti

University of New Mexico

On the cover of *The Sagas of Icelanders*, there is a quote by the great Seamus Heaney. He writes that the almost-800-page tome is “a testimony to the human spirit’s ability not only to endure what fate may send it but to be renewed by the experience.” When I picked it up for her in September of 2019, I couldn’t help but be struck by how much that sounded just like Kendra Koivu.

I first met Kendra when I came to visit the University of New Mexico, before I knew whether I would attend, but after I knew I had been accepted to the PhD program. She was, without a doubt, one of the brightest lights on the faculty, even as its newest member. I remember being excited by the prospect of working with her and was thrilled when she sent me an email saying she wanted to work with me, too.

The semester she was diagnosed, I was her TA for Comparative Politics. She sent me an email saying that she wasn’t sure what things would look like yet or what kind of treatment options she might have. She wrote: “We’ll take things as they come.” And we did, at least in terms of class. In those early days, I didn’t see anything “behind the scenes,” but I witnessed how she handled the diagnosis with her class and her students—she met it head-on, like a warrior. She announced to the room of 80 or so undergraduates, in a voice that quavered with emotion but was the epitome of strength and grace, that she didn’t know what was going to happen. At first, the room was silent. Then, she cracked a joke about how she might end up bald, so she thought she might dye her hair purple or green first. Everyone laughed—maybe a

little too quickly—relieved that she had provided them an outlet.

After the intensity of that semester, things settled into a pattern. Sometimes she’d need to get a class covered or cancel a meeting. Still, the cancer felt more like something we could incorporate, something that could be handled, rather than a looming threat.

During that time, she heartily supported my wanting to take one of my comprehensive exams in methods, part of which included attending the Institute for Qualitative and Multi-Method Research (IQMR). She came out for an author’s workshop, and we met up for dinner. We waited forever to get into a restaurant she wanted to try. We had taken a shuttle from the hotel to get there, but by the time we had eaten we had missed the window to take it back. We traversed the different neighborhoods on foot, discussing the *why* of inequality as the scenery changed. Kendra loved the *why* and would approach it as an exploration of necessary and sufficient conditions, no matter the topic. “Are these really causal mechanisms,” she would ask, “or are they more like contributing factors?” Over the years, I had a number of similar conversations with her; in the classroom, in a cab, in a fancy wine bar. In those moments, it seemed as though she could conquer anything.

On another summer day, a few years after our rendezvous in Syracuse, Kendra sent me an email inviting me to lunch. At the time, it felt out of the blue. My dissertation had fallen apart after one of my field sites collapsed, and I couldn’t figure out an ethical way to salvage it. I had a one-and-a-half-year-old at home who

wouldn't sleep through the night and not enough money to afford full-time daycare. Since becoming a mother, I'd encountered a range of experiences on campus that completely floored me—sometimes in a positive way, but more often in a demoralizing, I-can't-believe-that-just-happened kind of way. In a word, I was struggling. I'm not sure I even realized how much I was struggling—and then, the email notification came across my screen. It was simple: “How are you? Let's do lunch!” I responded, we set up a time.

The first few minutes were filled with small talk, but she quickly launched into what must have been a prepared speech, or at least, a series of things that had been firmly in her mind. She knew what was going on with me, she'd been paying attention. At first, I protested— not wanting to admit to feeling weak or powerless. But she didn't let up. She laid it out for me: specific observations about balancing motherhood with graduate school, the concepts I'd been wrangling in my new dissertation prospectus, the blows that had shaken my self-confidence. It was brutal, and honest, and I knew I couldn't deny the truth behind her words. And then, she shared with me parts of her own story, things she had experienced, lessons she'd learned. She let me know I wasn't alone.

After that meeting, and through an intentional series of very small steps, she helped me to rebuild— my dissertation, yes, but really only as a byproduct of learning how to trust myself again. In a time when I felt too defeated to put any kind of meaningful words on paper, she gave me the courage to write a bad first draft. She printed out a calendar and we mapped out a plan— times to meet, times to turn in work. When we met, she would help me draw out my ideas, pushing me to connect with them on a deeper level. Somewhere, I have a folder filled with her writing—notes on papers of all sizes, full of diagrams and arrows and big-picture questions. A love

language of enthusiasm and excitement and scribbles.

The last semester she was at UNM, I was teaching my own class, and had a student who was giving me a really hard time. When I'd planned the class originally, she'd been excited to do a guest lecture on Ottoman rule in Turkey. Once the semester was underway, however, her strength had begun to falter, and I instead incorporated some of the materials she gave me into my own planned lecture. But after she witnessed some of my difficulties with the student directly, she changed her mind. I could see how drained she was— but she insisted it was more important that she come to class. It wasn't sufficient, she said, to believe in my ability to teach the class; it was necessary to *show* the students that she respected me and to confer her approval of me publicly. She not only validated the difficulty I was having, she wanted me to know it wasn't my fault. She went out of her way to tell me that she knew how much I cared about teaching and my students, and then she put herself on the line for me. It was one hell of a lecture.

The last time I saw her, I was with my colleague, Fiorella Vera-Adrianzen. Kendra was at home in hospice care. At first, the heavy air was filled with awkward musings about food and politics. She asked about our families, our work, a conference presentation I was scheduled to give at Notre Dame. She reiterated positive comments about the paper— she called it the “shadow institutions” paper, although it was never as cool as that name implied. She apologized that she hadn't given me feedback on the most recent draft. Even in that space, in that time, she was giving us advice, encouragement, support. We wanted to tell her how much she meant to us, how amazing we thought she was. But it was too hard; we couldn't do it. Instead, we told her we'd do our best to make her proud, to share what she'd taught us. Her response was certainty—of course we would. She believed in us. There was no doubt.

On the Loss of a Dear Friend

Erin Kimball Damman
University of Idaho

Kendra was one of my best friends. She was a brilliant scholar and a wonderful collaborator, but she was so much more than that. She was warm and generous, with an open-hearted acceptance of people that always amazed me. I miss her terribly.

When I showed up at Northwestern, I was the only woman in my cohort. My male colleagues were great, but I was slightly adrift with no female counterparts. A year ahead of me, Kendra quickly took me under her wing, and we became fast friends. During my second and third years, we shared an office in Scott Hall. Though

we spent time studying together and bouncing ideas off one another, we spent a lot more time talking through personal problems and joking around. We may have acted more like adolescents at a sleep-over party than serious graduate students, but the light-heartedness kept our spirits up.

I don't think Kendra was afraid of anything. In our first co-authoring experience, James Mahoney asked us to present the paper that we had written with him to a special breakout session at the Institute for Qualitative and Multi-Method Research (IQMR). Though early in our graduate careers, we had both experienced presenting at conferences, so an informal presentation should not have been a big deal. However, when we got up in front of some of the biggest names in qualitative research, I froze. Kendra had given an eloquent introduction, but when she turned to me, I just stared dumbly back at her. In quintessential Kendra fashion, however, she saw what was happening, gave me a quick smile and took over my part of the presentation without missing a beat. Afterwards, many co-presenters would probably have been annoyed with their partner for this. Kendra was not. She simply made a joke about how intimidating this audience was, solidifying our partnership and brushing off my apologies and gratitude. This was Kendra to a tee: graceful, unfailingly kind, and fiercely intelligent with a quick wit.

Towards the end of her life, Kendra was sometimes confused by all of the praise she was receiving for her scholarly work. She didn't think she deserved it, but she could not have been more wrong. Her ability to think through the logic of a methodological problem was expansive. She was comfortable debating theory and techniques in an abstract sense, but extraordinary at seeing how these techniques should be applied to substantive projects. Her work on organized crime was thus exciting not just for its contributions to scholarly literatures on state building, but also for its clean and well-identified use of within-case analysis and comparative methods. Kendra was also willing to extend herself to understand perspectives and tools that she herself did not use. When we wrote "Qualitative Variations," she took on the

section about interpretive methods. Though neither of us operated from this ontology, nor had much training in its epistemological grounding, she worked her way through the literature and ably found the parallels and differences to our other qualitative schools of thought.

Kendra was also exceptional at helping others think through their projects systematically. It was as if she could see a project from beginning to end, and help craft everything from the question to the research design. I can only imagine what an excellent dissertation advisor and teacher this made her.

As a single parent trying to make it through grad school, Kendra had a lot more challenges in her life than I did and faced some serious discrimination (both structural and individual), but she never gave up. When I had children later during the dissertation stage, I got through it mostly by thinking about Kendra. I remembered watching her balance single parenting while earning her degree, and being amazed by her simple acceptance of all the added pressure and time. I honestly don't know if I would have finished writing my dissertation without her example of perseverance to turn to. Indeed, though I never shared this with her because I'm pretty sure it would have embarrassed her, thinking about Kendra's tenacious spirit continues to motivate me. After she became an assistant professor, she had a second baby, faced cancer, and still got tenure. When I think a current project is hard or feel less than motivated, I often think, how would Kendra have handled this?

After Kendra passed, I was deeply, deeply sad. I still am. I never truly accepted that her diagnosis was terminal. Even when sitting beside her in her last weeks of life, I kept feeling that she would somehow beat this. Her indomitable spirit had bested so many other challenges in life that it seemed like cancer couldn't possibly take her from us. Nothing about losing her so young was okay, and this world is less bright without her. She left behind an amazing legacy of two beautiful and talented children, a host of well-trained students, and many, many friends and colleagues that will miss her spirit and intelligence. Once again, in trying to manage my own grief at her loss, I am left thinking, how would Kendra have handled this?

Kendra Koivu: One of My Favorite People

Christopher Day
College of Charleston

Kendra Koivu was one of my closest friends and most influential intellectual playmates. We met in 2006 at Northwestern University. I was an incoming graduate student in political science, and she was a more seasoned veteran in her third year.

From the beginning, Kendra and I became fast friends, both members of the "Will Reno Mafia," that shared love and respect for our mentor while exploiting the vast material which he regularly provided us with to roast him. Will made the crucial mistake of letting us use his

office in the Political Science Department, where Kendra put up an old photo of Cheech & Chong and labeled it “Will and Georgi [Derlugian]: The Early Years.” Our crowning achievement of sophisticated-yet-immature hilarity was when I acquired a giant cardboard cutout of the Incredible Hulk from Blockbuster Video. Kendra printed out a life size page of Will’s face and taped it where the Hulk’s head was with a word bubble asking, “Where’s the gym?” (Will was known for his workout regimen and use of gyms worldwide.) We got zero work done in those months. But we laughed constantly and bonded permanently, with humor compensating for our debilitating impostor syndromes.

We were also among the only political science graduate students at Northwestern with small children. Her daughter Cosette and my son Sam became friends via this shared identity, and I get the sense that even now, not having seen one another for years, they continue to view one another as extended family—as they should. But where I might have seemed an innocuous oddity as a student parent in the department, as a single mother at Northwestern, Kendra faced a bizarre form of discrimination from graduate students and political science faculty alike. How I wish that this experience was something that did not haunt her to the end, but it did. It was a hard thing to witness and an even harder thing to forgive. But she finished a PhD while raising a daughter, went through a divorce and other forms of life upheaval, got an interview at bloody Harvard Business School, ended up with an amazing job at the University of New Mexico, and settled into a life as a well-respected scholar, well-loved human being, and a new mother again. So... fuck those people.

If I leaned on Kendra personally, so did I come to depend on her intellectually. Our research agendas overlapped—organized crime and rebel groups, respectively—so we found common cause in our scholarly pursuits. But Kendra was always way smarter than me and had a natural fluency in methodological language that I struggled to master. But she was no intellectual bully—she was kind and self-deprecating and explained things effortlessly. As we both left Northwestern and got jobs, she found her place within the professional community of qualitative methodologists, no doubt mentored along by the good and great Jim Mahoney, who identified and supported Kendra’s abilities in a field that I still only pretend to fully understand. Kendra was the real deal and on the cusp of becoming a total rock star in qualitative and mixed methods.

I am so damn proud of the article we wrote together, “Finding the Question: A Puzzle Based Approach to the Logic of Discovery” (Day and Koivu 2019). The piece grew out of a series of chats where we shared our struggles with teaching undergraduates how to ask research questions (we talked about a lot of other essential things too like what ever happened to Miranda Cosgrove). While the intent of the paper is pedagogical (how we both cringed at that word), the intellectual bones of the paper—the logic of discovery—that’s 100% Kendra. Theoretical and methodological puzzles? All her. When we presented an early version of the article together at APSA we surprisingly got all sorts of love from a room of uber-nerdy qualitative methodologists, where she was right at home, although easily the coolest among them. We then took a well-earned victory lap around the conference hotel district of San Francisco and planned world domination.

Even today when I hit an intellectual obstacle, my first reflex is to reach out to her to help me work through whatever incomplete thought I’m struggling to develop or embryonic idea I’m trying to waken. She was really good at doing that, having a natural gift for looking at a phenomenon and putting things into creative categories with cool labels. So, while I miss her for a million personal reasons, my heart breaks that we won’t be able to collaborate again.

I cannot say for certain that I was as good a friend to Kendra as she was to me. The years after her initial diagnosis flew past, full of false starts, setbacks, temporary reprieves, and eventual decline. I fell into a sort of complacent denial and figured she would outlive us all. And I was a pain in her ass for sure, foisting my drama and bullshit on her even when she was suffering from cancer. Of course, she let me know it, and often. But probably not all the time. Maybe that’s why I did it, because I knew I could and because she was the truest of friends.

When Kendra came to APSA last year, it was after a terminal diagnosis, and it was clear that she had come to say goodbye to her professional life. I am full of love and gratitude that I got to be her playmate for those few days. We had a lot of heavy conversations about what mark she was leaving on the world and what her final thoughts might be. A short time later, right towards the end, I was lucky enough to spend time with Kendra at her home in Albuquerque. With Jami Nuñez and Erin Damman—stalwart members of Team Kendra—among others, we shared a few precious moments of hilarity even as she

suffered from a horrible cocktail of toxic medication and the looming reality of hospice care. We said a beautiful goodbye and stayed in touch via texting until it likely just exhausted her and she just sort of faded and vanished.

I am still waiting for her to text me back. I miss her every day.

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Kendra Koivu: Remembering a Qualitative Methodologist

James Mahoney
Northwestern University

“I think fuzzy-set analysis is really useful.” Those are the words that I remember Kendra Koivu saying as she began to make a comment during an APSA meeting in which some leaders from the qualitative methods section were chatting with graduate students. Kendra was still a graduate student herself, and the context of the meeting was a brown bag lunch for students participating in the qualitative methods working group sponsored by APSA. I blushed as she began talking because she learned about fuzzy-set analysis from me, and I wanted to keep anything related to set-theoretic analysis out of the discussion. As she continued to speak, however, my emotion shifted from a twinge of discomfort to a sweeping feeling of admiration and pride. She spoke about the value of set-theoretic methods with authenticity, conviction, and intelligence. I thought her remarks were courageous. I never told Kendra that her comment was inspiring for me, but it was. I returned to that memory many times over the years.

Another memory: Kendra Koivu and Erin Kimball (now Damman) come knocking at my office door to visit me to discuss methodology. Kendra gets right to the point, “You said not enough women are working in methodology in political science. We are here to try to change that.” Kendra was referring to my complaining about gender bias in the field of methodology that generations of Northwestern students have had to endure. Kendra and Erin wanted to work in this area, and they proactively reached out to me seeking collaboration. I was working on an article related to set-theoretic causality and historical sequences, and I was pretty stuck on several fronts. We soon began a collaboration that led to one of my all-time favorite articles for which I am an author. In that article, we coined the term SUIN

condition, which is now often used in the QCA field.

Kendra was fascinated with set diagrams illustrating the set-membership relations between categories, and she did much to move forward the visualization of set-theoretic analysis. Along with her, I became fascinated with set diagrams. I trace our fascination back to Charles Ragin, who suggested a solution to a problem we were having with our article on historical sequences. We were trying to figure out how we could help people understand why certain causal conditions were necessarily more important than others in causal chain arguments. Ragin suggested that we illustrate the idea with diagrams, and Kendra and Erin carried out the task of working out our argument in diagram form.

For Kendra and me, this work led to a subsequent interest—some might say obsession—with using diagrams to explore and understand the logic of social science arguments. Kendra and I never discussed academic matters without drawing pictures and creating set-theoretic figures to illustrate our ideas. Whereas some scholars communicate using the language of statistics, algebra, or calculus, we communicated using the language of logic and its set-theoretic expression.

Kendra had a talent for thinking spatially and relating set-theoretic logic to social science matters. This way of thinking came naturally to her, and I know she loved to think abstractly in this manner. The logic of methodology no doubt gave her that sublime worldly escape that comes with totally engrossed intellectual thinking. Kendra and I could discuss issues that built on an enormous shared foundation. This shared foundation allowed us to achieve the kind of intersubjective understanding that makes you feel as if you are on a special intellectual wavelength with another person. We were right there together appreciating

the ideas that reverberated on that wavelength.

For me personally, Kendra was an extra special colleague: she embraced a set-theoretic methodology for the same reasons as me. Kendra believed that set-theoretic analysis is an ontology for understanding the social world. Set-theoretic analysis is a tool for capturing the way in which categories reflect our substantive knowledge and embody substantive claims about the world. Kendra believed that our categories construct our social reality as much as the reverse. And she believed that set-theoretic analysis could capture this interaction between categories and social reality.

For the discipline more generally, Kendra was also a special colleague. Her methodological work made substantial contributions on a number of fronts besides set-theoretic analysis, especially in the field of multi-method research. Her substantive research made significant contributions to the study of political order, political violence, and the rule of law. She was a generous colleague, offering high quality insight, comments, and help with regularity and without an expectation of reciprocation. She was a rising star in the field of qualitative methodology, serving in leadership roles for the APSA section. She participated in research development meetings in conjunction with the Institute for Qualitative and Multi-Method Research at Syracuse. She was a founding director of the Southwest Workshop

on Mixed-Methods Research. She was centrally involved in the development and proliferation of new initiatives in the field of qualitative methodology.

Another memory: On a gray morning, Kendra and I walking uphill together by Syracuse University approaching the building that looks like the house from the Addams family. We are discussing a paper she is writing on counterfactual analysis, but the conversation shifts to how things are going more generally. She gives me a truthful summary of the life of an academic with children at a major research university who is living with cancer and worried about getting tenure. At the end of the conversation, Kendra peels off because she is not feeling well because of her cancer treatments. I peel off to go to the men's room so I can cry quietly for a couple minutes. Those tears consisted of both sorrow for what Kendra had to endure and admiration for the way in which she was enduring it.

Kendra Koivu was a passionate, kind, generous, and original scholar with an ability to both think abstractly about general categories and conduct serious field research on the ground. She was a deeply valued colleague and friend to many of us. Kendra's academic contributions will continue to influence the field for years to come. Her presence will stay with us through these contributions, and, even more, through our fond memories of good times together.

Sisu

Jami Nelson-Nuñez
University of New Mexico

When we lost Kendra Koivu in September 2019, we felt the impact in so many different spaces and ways. Her contribution and impact in her role as a professor at the University of New Mexico (UNM) was powerful. I only knew UNM with her in it, as I started as an assistant professor four years after her. She brought a lot of laughter to our hallways and created a supportive space that immediately made me feel like I belonged. It's been a challenge to write this tribute—for the obvious reason that the pain of her loss is still sharp—but also because I know many of the people who are reading this were incredibly important in Kendra's life. So in this tribute to Kendra, I want to honor her scholarly achievements and her contributions as a teacher and mentor at UNM, but I also hope to relate how special she was given her perseverance, rare talents,

and the impact she made on so many people.

Kendra saw the world from an uncommon angle, one that reflected (and perhaps resulted) from her unconventional path to this profession. Her lived experiences coupled with a sharp mind allowed her to make connections that many miss. For example, in the epic tales of the Icelandic sagas, she saw a case study of a unique state-building process. In studying Finnish history, a case connected to her family's roots, she focused on what was missing and how the case demonstrated overlooked variation in the persistence of criminal activity. She tore into historical archives to develop the Finnish case of the Age of Knife Fighters, tracing not only how they arose, but also brilliantly finding ways to test her theories on why gangs sometimes disappear. She extended Tilly's work on state-building and war to the connections between state-building and crime, focusing

not on the Latin American cases that garner significant attention, but on the overlooked cases of Turkey, Japan, and Finland, leveraging them to sift out new insights in how states “consort with criminals.”

Other specialists who work on organized crime, state-building, or research methodology can speak to Kendra’s contributions to the field more precisely than I can, but at UNM I had the chance to watch her and be a part of the process of Kendra’s production of knowledge. She preferred pen and paper to laptops, and her desk was regularly decorated with large, dusty books and post-it notes. Kendra held a deep understanding of causal logic, one that would manifest with equal force in seminars and in casual conversations. She often drew parallels between the challenges of research design and the limitations of the medical research she studied related to her own health. Her most frustrating moments were when someone argued with her using a logical fallacy.

I relied on her. We would regularly have lunch at the faculty club at UNM and discuss our work and lives. I looked forward to these times and would queue up challenges we could discuss like hard situations in the classroom or issues with my research. Kendra would carefully listen and innately point to the research question that I had been dancing around for days. I would spin an idea and she would immediately find its connection to much broader implications than had occurred to me. I came to depend on her as a colleague and friend for her wit and brilliance.

Much of the same creativity and support Kendra extended to me she also gave to her students. She met students where they were at, coaxing out their ideas and helping them find direction among seemingly disparate threads. Kendra was an enthusiastic professor. She was always developing new classroom activities, such as working with the library to create an archival research activity and devising new simulations and debates that her students still remember today.

Kendra burned hot and emitted remarkable energy. Within five years of getting her PhD, she had written a book, published several articles, collaborated to start an annual methodological conference, developed several courses, married, given birth to a son, and cared for a growing teenage daughter. She also became a central voice in the QMMR community, taking on an editorial role of this publication.

When she was healthy, and even during her sickness, she swam regularly at 6 am. She swam fast and forcefully, compelling one of my colleagues to abandon her new swimsuit and goggles in the gym locker room forever

after just one “fun swim” with Kendra. She swam outside, during the winter, even when the pool heater broke. (Maybe *especially* when the pool heater broke.)

Kendra worked in between doctor’s appointments; she battled insurance companies between classes; eventually she would lay down and rest when office hours were slow. She always kept going even when most of us around her were encouraging her to rest and ease up.

Kendra’s productivity and energy were stunning given the obstacles she had to confront. While I am hesitant to dwell on Kendra’s hardships as a student and later as a scholar (since she certainly never did), I think it is important to recognize them because so many people in her professional life had no idea. In fact, Kendra worked especially hard to be judged by her work and not the extra mile she had to walk to succeed despite significant challenges. At her memorial, her family described Kendra’s demeanor as “Sisu,” the Finnish term for persistence and stoic determination. She single-parented her way through both undergraduate and graduate school; she endured medical malpractice that refused her cancer screening when she told them she felt a lump in her breast; she fought for tenure with a terminal health condition and two young children.

And one of the most subtle and important parts of the remembrance of Kendra is not just that she had grit and talent. It’s that she had so much compassion for others who struggle. It is unfortunate that among those who have struggled and eventually achieved, many not only flaunt their bootstrapped accomplishments, but they also expect others to suffer and work as hard as they did. That was not Kendra. She fiercely defended others who are disempowered—be it on Facebook or with a sharp witty reply in conversations. (She would have had a lot to say in 2020.)

Kendra remade her syllabus to add policies to support students who are parenting. She became graduate advisor even when she was battling cancer so she could better advise and encourage students. And she had endless patience with others’ doubts that they could be capable of comprehensive exams, of dissertations, and of tenure.

Kendra just kept going. I think she accomplished so much in the face of cancer because she wanted to be an academic and was passionate about research, but also because challenge gave her purpose, as so much of her life attests. Yet, I also think she did it because she didn’t have the option of not doing it. Of the many things Kendra has left me, one is the reflection that we need to do more to support people with health conditions. She

was surrounded by colleagues that made the institution and the profession more humane, but the ambiguities of whether she should use her medical leave (she never did because she was worried of using it up when she might need it more later) or whether she could lose her insurance benefits if she didn't get tenure, were heavy and unfair burdens.

Kendra had the energy and potential not only to contribute in important ways to scholarship but also to challenge aspects of the institutions in our lives that perpetuate inequalities. The enlightened among us learn more about life through hardship. Kendra was so wise—naturally, but also through her experiences. I feel devastated that she is no longer here to share the things she learned or the rare perspectives that students need so badly to hear, because it would make them feel heard and because it would make them think and because it would

make them better scholars.

In the final weeks of Kendra's life, I was privileged to be with her as she contemplated what was to come next. Kendra's idea of heaven was "complete knowledge." Of what, we asked? She said, "Of everything. Like gravity." And when someone began to explain the concept of gravity to her (as though she didn't understand and we all so badly wanted to be helpful), she stopped them and said, "No, I know *how* gravity works. But *why* gravity?"

Kendra had the drive and the commitment to make all the spaces where she worked and lived better. She made the University of New Mexico better. She made me aspire to be better—and to keep going. Her sighs and laughter will be so missed in our hallways, and I hope, like many, that she is peacefully resting in complete knowledge, especially the knowledge that she leaves behind so many who love her and miss her and who will strive to actively carry on her memory.

Kendra Koivu: A Brilliant Methodologist and a Dear Friend

Sara Niedzwiecki

University of California, Santa Cruz

I met Kendra Koivu when I was a newly hired assistant professor at the University of New Mexico. Kendra quickly became a role model for me: intelligent, brave, and with a great sense of humor. She had a brilliant mind and a way of writing and teaching about qualitative methods and political economy that made it easy to understand complex ideas. She also had the uncanny ability to bring levity to even the most uncomfortable faculty meetings.

I only knew Kendra with cancer, as she was diagnosed soon after we met. The first prognosis was a dim one: she was given only months, perhaps a year, to live. I remember having numerous conversations with her about how one lives their last months on earth. She mentioned her family and Chicago, perhaps the main place she considered home. After that, she entered into a number of successful clinical trials that allowed all of us to have her for longer. I remember when the first clinical trial was showing signs of progress, Kendra said: "I guess I have to finish that book now." And she did finish it, while battling cancer, raising two kids, and contributing to the field.

Kendra's book manuscript "Consorting with Criminals: Prohibition and Statebuilding in the Interwar

Period" is currently under review with Cambridge University Press. One of the anonymous reviewers of the manuscript wrote: "This is a fascinating book manuscript. Although there are many bits and pieces in the literature that connect criminalization and state-making, this book makes these connections in a more explicit, systematic, and methodologically self-conscious way to identify and explain variation across multiple cases. This in itself is a worthy contribution. Tilly has inspired an entire literature on the interaction between state-building and war, and this book extends the inspiration to the far less explored intersection between state-building and crime(fighting)."

Kendra's main contributions were in the field of qualitative methodology. Her work on set-theory (see Mahoney, this volume), counterfactual analysis, mixed methods, and case selection procedures was like a trip to Ikea: you don't understand how you lived your whole life without each particular item. Once you learn of its existence, you know exactly how it helps you understand and explain your own work. That is particularly the case of one of her co-authored articles on case selection (Koivu and Hinze 2017). The first time I saw Kendra present this work at APSA in Philadelphia,

I deeply identified with their emphasis on the logistical constraints that shape the cases we select for research. The authors argue “that methodological rigor in case selection overlooks the human element in social science research, thereby diminishing transparency” (Koivu and Hinze 2017, 1023). Fully addressing logistical constraints, such as funding, language skills, or access to data can complement rigorous case selection. This article succeeded at bringing the theory and practice of case selection together. It made me realize that as an Argentine, of course studying Argentina and Latin America had not been a random selection. And it was fine, and methodologically transparent, to acknowledge that human considerations had played a role in my case selection strategy.

Kendra’s work has contributed not only to the research and practice of qualitative methodology, but also to undergraduate teaching. She was a marvelous teacher. Her co-authored article “Finding the Question: A Puzzle-Based Approach to the Logic of Discovery” (Day and Koivu 2019) is an example of writing with the classroom in mind. Koivu and Day argue that while researchers have ample guidance for causal inference, we need further discussion on the logic of discovery. They explain: “The logic of discovery is a stage in the research design process that is often bracketed off as an unexplainable moment of inspiration, and is largely missing from the research design literature” (Day and Koivu 2019, 1). The authors develop a novel typology to guide the process of discovery: puzzles can originate from gaps or contradictions in the literature (“theoretical puzzles”), from real-world events (“empirical puzzles”), or from debates on measurement or research techniques (“methodological puzzles”). I assign this article in the first week of my qualitative methods class. The group discusses where research questions come from before thinking about qualitative data collection and analysis. The students (generally sophomores and juniors) appreciate the class assignment included in the article’s appendix, as it helps them differentiate between interests and research questions.

Kendra also provided crucial service to the discipline. She was an active member of The APSA Organized Section for Qualitative and Multi-Method Research. The Section named an award after her, the “Kendra Koivu Paper Award,” to honor her legacy and contributions. She was also the co-editor of this very venue, the *Qualitative and Multi-Method Research Publication*, and the co-founder of the “Southwest Workshop on Mixed Methods Research” (SWMMR, pronounced “swimmer”).

Marissa Brookes, Jennifer Cyr, Kendra, and I co-founded the SWMMR in 2014. The four of us had an interest in deepening the discussion of how to combine multiple methodologies, and had coincidentally accepted positions in the Southwestern United States. Our role in the SWMMR allowed me to learn from Kendra’s works in progress and from her insightful comments to people’s drafts, including my own. She was a generous commentator and had the ability to read people’s work under the best possible light.

The fact that our workshop was named “the swimmer” could not be more perfect. Kendra lived her life like an Olympic swimmer. After her initial diagnosis, she started swimming at five in the morning in an outside swimming pool, even during Albuquerque’s frigid winters. She told me this gave her a sense of control over her own body and life.

The SWMMR allowed me to share a wonderful road trip to Tucson with Kendra. In that trip, she discovered the most delicious Philly cheesesteak she had ever tried at a gas station on the border of New Mexico and Arizona. We talked for a while about when to go back to that gas station to enjoy that cheesesteak again, a mere 6-hour drive from Albuquerque. Kendra had a deep appreciation of good food and saw nothing wrong in driving hundreds of miles for a good dish.

Kendra and her husband Tony were fantastic cooks. Their parties were like her work, they raised the bar for everyone else. Kendra and Tony didn’t just order pizza and ice cream. They made pizza and ice cream from scratch and asked the guests what toppings and flavors they wanted to try. I was able to see Kendra weeks before she passed, and her house in Albuquerque again became the venue of a celebration, a place where friends and family from everywhere stopped by to celebrate her life with her. She was telling funny and detailed stories that ranged from fieldwork with William Reno, to Doctor Who, to buying shoes in Finland. Her sense of humor was intact.

Kendra was a brilliant scholar and a dear friend. Her scholarly contributions will continue shaping our work and the discipline. My world is less bright without Kendra in it. But I feel deep gratitude to have crossed paths with her and will remember Kendra with a smile in my soul.

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Kendrachaychik Ñuqanchikwan Tukuypuni (Our Dear Kendra, with Us Always)

Fiorella Vera-Adrianzén
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I met Kendra when she first arrived at the University of New Mexico (UNM). She was enjoying the *alfajores* I brought to the departmental potluck. I told her these were Peruvian-style cookies made with *manjar blanco*. She knew the cookies well, being a foodie, but had not heard of *manjar blanco*. I explained that in Peru we refer to *dulce de leche* as *manjar blanco*, to which she replied in perfect French, "like *blancmange*!" A discussion about her fascination with other aspects of Peruvian cuisine, love of languages, and experiences traveling abroad ensued. I felt instantly at home. Following that brief but insightful moment, building a connection with Kendra became one of the most meaningful experiences of my life.

Kendra was devoted to being more than a professor to her students. I, along with three of my colleagues, took a directed readings seminar with Kendra in preparation for our Comparative Politics comprehensive exam. Although her son Enzo had just been born and she had a lot on her plate, she took on that extra class to help us. I was particularly nervous, as English is not my first language. Kendra believed in us—more than we believed in ourselves, at times—and met with us weekly, challenging us at every step and helping our confidence. Our discussions were full of diagrams and pop culture references (from which I learned a lot about the US). After we were notified that we passed the exam, I recall her saying we "made collective action work."

During that time with Kendra, I also discovered my passion for methods. That summer, she encouraged me to attend IQMR, where I learned critical tools that helped consolidate my dissertation proposal and created lasting friendships which have accompanied me through many hardships. Taking a qualitative research methods seminar with her was one of the most memorable moments of my time at UNM. Kendra's unique and refreshing way

of teaching qualitative methods and her ability to lead a constructive discussion was remarkable. She advised us on how to balance life and the pursuit of an academic career as she was simultaneously navigating her own work-life balance while undergoing cancer treatment. She would talk about causality while drawing a truth table or Venn diagram using the clinical trial data she relied on to make decisions about her health. Kendra was insightful, transparent, exemplary, and resilient. By sharing with us the enormous challenges she was experiencing, she was preparing us for life. In 2016, my friend Anna Calasanti and I, inspired by Kendra, embarked on a mission to create a space at UNM to talk about how to best prepare to conduct fieldwork under complicated circumstances. Kendra—with her characteristic encouragement, creativity, and enthusiasm—helped us develop this interdisciplinary conference on fieldwork practices, the first of its kind at UNM. Kendra believed in us; she not only participated in multiple roles, she also pushed us to keep going at every stage.

Her teachings and constant guidance prepared me for my dissertation fieldwork, working with Quechua communities affected by the Peruvian civil conflict. The two times I returned from the field, I came to her overwhelmed with stories, details, and questions that I felt were not addressed by my dissertation. On her ever-present yellow legal pad, she created multiple diagrams that helped structure my thoughts. So simple, but so symbolic of her. She enjoyed looking at my photos depicting Quechua customs, colorful Andean landscapes and clothing, and empowering moments I witnessed. She said it reminded her of her fieldwork in Turkey and speaking Turkish. We enjoyed discussing similarities between these two languages. She believed in my ability to complete a mixed-methods dissertation on post-conflict justice and always supported me, especially when coping

with secondary trauma from this research—a topic she knew we needed to improve on in academia.

Kendra and I enjoyed the food scene in Albuquerque. She introduced me to authentic ramen, which I had not tried before. She was convinced that needed to be fixed, so we enjoyed one at a restaurant near UNM every once in a while, when the weather was “cold and perfect for a ramen,” as she used to say. Whether she was enjoying the *pan con chicharrón* or *tres leches* cake from the Peruvian bakery or the *arepas* or *cubano* sandwich from the Guava Tree Café, I was happy to share a moment with her at some of her favorite places in town. Many of these meals were full of chats about academia, but also about family and health. Even during difficult times, she had the mental capacity and strength to advise me on how to work with my insurance company and organize my medical records when I was undergoing my own health issues—she wanted to make my experience easier. For her, it was always about being more than a mentor. She cared, listened, and empathized with us. She knew I loved dogs and she was happy to let me watch Butters in my office when she had her at UNM.

Kendra and I talked a lot about revitalizing the way we think of multifinality, where “a single cause leads to different outcomes.” Reflecting on this now, I feel the connection many of us built with her was one of multifinality. Although the bonds we have with her are not sufficient to understand where we are standing in life now, they are necessary. For all whose lives she has influenced and will continue to do so—as a scholar and as an amazing human—she will always be that “superset.”

The last day Anna and I visited her, I relived all the moments we shared throughout these years. We brought cards and letters written by many colleagues Kendra had mentored during her time at UNM. She told us she made an awesome *manjar blanco* ice cream from scratch. She remembered the time we gave her a *tres leches* cake for her birthday from the Peruvian bakery. We shared IQMR memories, a brief chat about methods, and updates on our dissertations. Somehow, we found ourselves discussing what superpower we would choose if we were able, and she said: “to speak all languages in the world.” She was surrounded by loved ones, with her kittens by her side, while watching *The Golden Girls*. This is Kendra. Whether we talked about academia, food, languages, culture, health, family, or more, I always felt included and cared for. She told me and Anna, “I am sure you will make me proud.” She believed in us. She believed in living fully and above all, being present. She inspired me in ways I never anticipated and will continue to do so. “*Kendrachaychik Ñuqanchikwan Tukyupuni*” I told her in the last card I gave her, filled with Quechua words, along with pictures from the Andes. Kendra came into our lives to stay with us. Always.

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Giovanni Sartori QMMR Book Award

This award recognizes the best book, published in the calendar year prior to the year in which the award is presented, which makes an original contribution to qualitative or multi-method methodology per se, synthesizes or integrates methodological ideas in a way that is itself a methodological contribution, or provides an exemplary application of qualitative methods to a substantive issue. The selection committee consisted of Alisha Holland (Harvard University), chair; Nuno Monteiro (Yale University); and Andrew Bennett (Georgetown University).

Winner of the 2019 Award: Simeon Nichter. 2018. *Votes for Survival: Relational Clientelism in Latin America*. New York: Cambridge University Press.

Honorable Mention: Abigail Fisher Williamson. 2018. *Welcoming New Americans? Local Governments and Immigrant Incorporation*. Chicago: The University of Chicago Press.

Prize Citation: The Committee is delighted to award the 2019 Sartori Award to *Votes for Survival: Relational Clientelism in Latin America* by Simeon Nichter. In the crowded field of studies of clientelism, Nichter breaks conceptual ground by underscoring the importance of relational clientelism, or exchange relationships that occur between election cycles. Nichter also highlights vulnerability, as opposed to poverty, in explaining important puzzles about the persistence and targeting of clientelistic benefits. The book provides a convincing account of how citizens actively sustain clientelistic relationships through their demands for benefits and ability to signal their loyalties. It stands out for its analytical clarity. Nichter derives a range of testable propositions and evaluates them across different scales of analysis, from the effects of changing national-level electoral laws to individual-level benefit receipt. The book is particularly suited for the Sartori Prize given its ability to seamlessly incorporate various types of evidence and methods. A formal model is beautifully integrated into the text, a natural experiment

pins down the importance of economic vulnerability, interviews in rural Brazil substantiate the mechanisms, and two original surveys (as well as compiled survey data from across the region) elaborate the core arguments and extend them beyond the case of Brazil. *Votes for Survival* is an exemplary work of scholarship that will reorient debates around clientelism and serve as a touchstone for scholars looking to conduct mixed methods research.

The Committee has decided to provide an honorable mention to *Welcoming New Americans? Local Governments and Immigrant Incorporation* by Abigail Fisher Williamson for the 2019 Sartori Award. *Welcoming New Americans?* asks a pressing political question—why do some communities accommodate immigrants, while others ignore or restrict them? The book establishes surprising variation in local responses to immigrants. It is not just urban, liberal areas that welcome immigrants—quite the opposite, small and mid-sized towns often engage in practices to accommodate immigrants and do so more over time. It advances an intriguing argument that bureaucrats who are required to provide services to immigrants under national laws then become important pressure groups to push for more accommodating local practices, whereas local politicians responding to electoral pressure often push against such approaches. In the spirit of the Sartori award, the book provides a rigorous conceptualization of formal and informal local accommodating policies. It then evaluates the causes of variation through an impressive original survey and compelling qualitative research in four new immigrant destinations that vary in their responses and trajectories over time. Williamson’s work shows the importance of using mixed methods to uncover nuance in a polarized debate like local responses to immigrants.

Alexander George Article / Chapter Award

This award recognizes the journal article or book chapter, published in the calendar year prior to the year in which the award is presented, which—on its own—makes the greatest methodological contribution to qualitative research and/or provides the most exemplary

application of qualitative research methods. The selection committee consisted of Lindsay Mayka (Colby College), chair; Kate Baldwin (Yale University), and Jack Levy (Rutgers University).

Winner of the 2019 Award: Jennifer M. Larson and Janet I. Lewis. 2018. “Rumors, Kinship Networks, and Rebel Group Formation.” *International Organization* 72, no. 4 (Fall): 871-903.

DOI: <https://doi.org/10.1017/S0020818318000243>

Honorable mention: Paul Musgrave and Daniel H. Nexon. 2018. “Defending Hierarchy from the Moon to the Indian Ocean: Symbolic Capital and Political Dominance in Early Modern China and the Cold War.” *International Organization* 72, no. 3 (Summer): 591-626.

DOI: <https://doi.org/10.1017/S0020818318000139>

Prize Citation: The award committee is delighted to award the 2019 George Award to Jennifer Larson and Janet Lewis for “Rumors, Kinship Networks, and Rebel Group Formation,” published in *International Organization*. Larson and Lewis’s creative article is a superb example of how careful qualitative methods can drive theory forward by identifying overlooked questions and cases.

Larson and Lewis start from a crucial yet understudied moment in civil conflict: the “launching” of new rebel groups. Whereas most studies focus on established rebel groups, Larson and Lewis ask why some groups are able to consolidate during their vulnerable early days. Through an analysis of Uganda, Larson and Lewis argue that different kinship structures shape communication networks, enabling the spread of rumors that help nascent rebel groups gain the trust of local communities. This trust is essential for emergent rebel groups to consolidate and become viable.

Larson and Lewis’s article has several methodological strengths that are worthy of commendation. First, the paper reveals ways that qualitative research can uncover overlooked political phenomena, thereby opening up new lines of inquiry. The paper starts from an empirical oversight: quantitative datasets omit most cases of rebel groups that fizzle out before gaining viability. For example, the Correlates of War dataset includes only 1 out of 16 rebel groups in Uganda, while the more complete PRIO dataset still only includes 7 out of 16. Through qualitative analysis, Larson and Lewis explore the question of why most rebel groups failed to launch, while only a few succeeded. Second, the study serves as a model of mixed-methods analysis, bringing together

a game theoretic model, network analysis, a paired case comparison, and extensive field research which generated 200 interviews and four focus groups.

In summation, Larson and Lewis’s article serves as a model of qualitative political science research, in line with the legacy of Alexander George.

The award committee awarded Honorable Mention for the 2019 George Award to Paul Musgrave and Daniel Nexon for their article, “Defending Hierarchy from the Moon to the Indian Ocean: Symbolic Capital and Political Dominance in Early Modern China and the Cold War,” published in *International Organization*. Musgrave and Nexon ask: Why do leaders invest in costly projects that they expect will not yield appreciable military or economic benefits? They point to the ways that concerns about legitimacy lead states to seek to dominate areas of high symbolic value—steps that may, on the surface, seem like inefficient investments of wealth and labor. Perhaps the paper’s most impressive methodological contribution is Musgrave and Nexon’s use of Annotation for Transparent Inquiry (ATI), which allows them to share additional evidence, context, and insights about their interpretations of source material. Beyond its transparency-related merits, Musgrave and Nexon’s article reveals how ATI can make process tracing more rigorous and effective. The authors draw on a wealth of evidence to evaluate expectations both from their theory and from alternative theories. It is no surprise, then, that this article has been held up as a model to teach others how to use ATI, and of ATI’s benefits.

Sage Paper Award

This award recognizes the best paper on qualitative and multi-methods research presented at the previous year’s meeting of the American Political Science Association. The selection committee consisted of Matt Amengual (Massachusetts Institute of Technology), chair; Sara Newland (Smith College); and Elliot Posner (Case Western University).

Winner of the 2019 Award: Paul Schuler and Chad Westerland, “Reconsidering the Rubber Stamp Thesis: A Consolidation Theory of Expropriations and Legislatures in Party-based Autocracies.”

The Sage Paper Award had a number of very high-quality submissions. Ultimately, the committee was especially impressed by Schuler and Westerland’s paper, “Reconsidering the Rubber Stamp Thesis:

A Consolidation Theory of Expropriations and Legislatures in Party-based Autocracies.” This paper explores the question: Do authoritarian legislatures prevent autocrats from expropriating? To answer this question, the authors apply the most cutting-edge Bayesian qualitative research methods, including those being developed by Fairfield and Charman. The authors clearly lay out observable implications of competing theories, develop an original dataset, explicitly examine clues in the data, and systematically address alternatives. The data analysis is transparent through the use of a well documented appendix. The paper stands out by showing how powerful new methods can be for disentangling complex causal processes. We believe that this paper will serve as a model for qualitative researchers to follow in the future.

David Collier Mid-Career Achievement Award

The David Collier Mid-Career Achievement Award of the Qualitative and Multi-Method Research (QMMR) Section of the American Political Science Association (APSA) honors the important contributions of David Collier to the discipline through his research, graduate teaching, and institution-building and, more generally, as a founder of the qualitative and multi-method research movement in contemporary political science. The award is presented annually to a mid-career political scientist to recognize distinction in methodological publications, innovative application of qualitative and multi-method approaches in substantive research, and/or institutional contributions to this area of methodology. The selection committee consisted of Melani Cammett (Harvard University), chair; Markus Kreuzer (Villanova University); and Jason Seawright (Northwestern University).

Winner of the 2019 Award: Carsten Schneider, Central European University

Prize Citation: The committee enthusiastically awards the David Collier Mid-Career Achievement Award to Professor Carsten Schneider of the Central European University. For his research and teaching contributions to qualitative and multi-methods research, and to the discipline of political science more broadly, he is without question the most deserving recipient of this 2019 award. This citation briefly notes his contributions to research and institution building.

Carsten Schneider is widely regarded as being one of the most important qualitative methodologists in political

science of his generation. His research and writings on Qualitative Comparative Analysis (QCA) are well-known, influential, and agenda-setting. His publications in this area cover an extensive range of material, including both general overviews of QCA and original research on specific topics related to combining QCA with case study research. Schneider’s own empirical research includes some of the most important applications of QCA. Furthermore, he is a major player within the field of qualitative and comparative methodology, and within the QMMR section.

The influence of Schneider’s work is easily documented through standard metrics such as Google Scholar citation counts, which have been growing at a rapid clip, as well as other indices. These measures show that Schneider is on track to become one of the most cited political scientists in his cohort, regardless of field or topic.

Schneider and Claudius Wagemann’s 2012 *Set-Theoretic Methods for the Social Sciences* deserves special mention. The book encompasses the whole family of set-theoretic methods, ranging from large and medium-N QCA to set-theoretic case studies, and illustrates these methods vividly with many examples and applications. This widely-acclaimed book has had a large impact—one that continues to grow—and has become a go-to resource for QCA users. In a series of articles published in top peer-reviewed journals, Schneider has also developed several related methodological innovations. Given the ambivalent views of the disciplinary mainstream of political science towards qualitative methodology as well as the very low acceptance rates in top journals, Schneider’s ability to repeatedly publish in the major disciplinary outlets is strong evidence of the excellence of his contributions.

In addition to his methodological interests and talents, Schneider also has a strong record of accomplishment as a scholar of political regime change. (This work has also received numerous citations.) Early in his career, in collaboration with Philippe Schmitter, he led a major effort to gather data on components of regime change, including liberalization, modes of transition, and consolidation in different world regions. A co-authored article analyzing these data won the 2004 *Democratization* Frank Cass Prize for the best article by a young scholar. His first book, *The Consolidation of Democracy in Europe and Latin America* (2008), also addresses the theme of regime change and itself makes important contributions to the study of regime consolidation. Various scholars who have discovered the importance of asymmetric hypotheses in

the analysis of democratic transitions and consolidation are now returning to its core findings, which are cast in terms of necessary and sufficient conditions.

Schneider's role in institution building around qualitative methodology and research is equally impressive. In Europe, he has organized and taught at the European Consortium for Political Research (ECPR) Methods Summer School for the last ten years and, in 2019, was the lead organizer for ECPR, which is being held at Central European University (CEU). Schneider has been an instructor of QCA methods at the meetings of the International Political Science Association (IPSA) and at the Global School in Empirical Research Methods (GSERM). At CEU, Schneider has also served as director of the Center for the Study of Imperfections in Democracies (DISC). This center deals with a broad range of topics related to the quality of democracy.

Schneider gives frequent talks on qualitative and multimethod research at various universities in Europe and Latin America and has been invited to teach short courses on methods at more than 20 universities in Europe. It is worth noting that Schneider has been able to achieve all of this while working at CEU, including as Department Chair of Political Science, which has faced a most challenging and uncertain academic environment

since the government of Victor Orbán came to power in Hungary.

In the United States, Schneider has played major roles in teaching and advancing qualitative methodology at APSA and the Institute for Qualitative and Multimethod Research (IQMR) in Syracuse. He has taught several times at IQMR, serving as the module leader for set-theoretic methods, and has taught short courses on QCA methods at APSA meetings. He has been an active member of and participant in the QMMR section and has published widely in the QMMR newsletter.

Schneider has consistently strived to build bridges among scholars using different methodologies. He has long advocated the linking of established case study methodologies, such as process tracing and typological theory, with the tools of QCA. Similarly, he has explored the appropriate relationship between regression analysis and QCA. In all of these efforts, Schneider has participated in debates about methodology with a respectful and constructive approach and is one of the most important voices in fostering linkages across methodological approaches in political science.

For these reasons and more we are pleased to award the 2019 Collier Mid-Career Achievement Award to Carsten Schneider.

