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## Letter from the President:

It is my pleasure as incoming President of the Qualitative and Multi-Methods Research section to thank our sections' members for the excellent, cutting-edge, and intellectually engaged research and other scholarly activities that have established our community as home to many foundational contributors to political science and the social sciences more generally. In recent years, our section has had the opportunity to support the development and publication of important new work that is purely methodological, as well as exceptional applications of qualitative and multi-method research techniques to key substantive areas. We have recognized such work through our section awards, workshoped work in progress through sponsored APSA sessions, and published select contributions through the section's excellent journal.

We find ourselves in a broader political moment when efforts at exclusion based on race, ethnicity, religion, gender, and sexuality have gathered strength in many parts of the world. Members of our section have long offered scholarly analysis of these efforts and of the movements that arise in opposition to them. In this historical moment of increased opposition to efforts at reversing long-standing patterns of exclusion, it is imperative that we redouble not only our scholarly efforts to understand, document, and analyze these phenomena, but also our practical work to ensure that our scholarly community represents the kind of inclusion that we want to achieve in the broader world.

As one concrete step in this direction, I ask each member of the organized section to make a concerted effort to use our section awards nomination process to amplify the excellent work on and using qualitative methods by scholars who are women, people of color, working at institutions in the developing world, and/or in other traditionally marginalized categories. Each year, such scholars produce an enormous array of interesting and innovative books, articles, and conference papers developing and/or using qualitative methods. Yet our section's awards committees can only consider the pieces that receive a nomination.

By making a special effort to read, recognize, and nominate great work by scholars from historically marginalized communities and identity categories, we can help the QMMR section live up to its potential as an exemplar of social as well as intellectual and methodological diversity. This effort can have broader effects, as well. Section awards can draw attention to job applications, strengthen tenure files, and help build a case for promotion. Hence, the efforts we make to highlight the contributions of diverse scholars within the qualitative and multi-methods community can help increase the pluralism of academia more generally.

Furthermore, the effort involved in nominating deserving work is minimal. For the Alexander L. George Article Award, articles or chapters in edited volumes can be nominated simply by emailing a PDF of the work to [celman@syr.edu](mailto:celman@syr.edu) by the end of January 2020. No elaborate nominating message is needed, and the effort to include authors from diverse backgrounds who may not think to nominate themselves will surely strengthen the section. This call for nominations of scholars with diverse backgrounds and identities is also highly relevant to the David Collier Mid-Career Achievement Award, recognizing scholars who have made significant contributions to qualitative and multi-method research and who are still in the 15-year period after the receipt of their Ph.D. Here, a bit more work is needed. A nomination requires a cover letter making a specific case for the achievements of the candidate and also an up-to-date curriculum vitae. These materials can be sent to [j-seawright@northwestern.edu](mailto:j-seawright@northwestern.edu) by the end of January 2020.

On a different note, I wish to thank Melani Cammett for her excellent leadership of the section over the last two years, and also Henry Hale for his service as vice president. Jennifer Larson has organized an excellent set of panels as the 2019 division chair, and she deserves thanks and recognition for these efforts. Colin Elman has continued his decades-long indefatigable service to the section and the broader cause of qualitative and multi-method research as secretary-treasurer of the section and also as the leader of the Institute for Qualitative and Multi-Method Research. It is hard to overstate his contributions this and every year. Jennifer Cyr remains an innovative and creative editor of the section publication, but I wish to especially recognize the contributions of her co-editor, Kendra Koivu. Kendra put an enormous amount of work into the newsletter, intellectual and otherwise, in the face of immense challenges during the last years of her life. The section has benefitted greatly from her efforts. I think her work on the newsletter and more broadly will be fittingly commemorated by the section paper award, which has been renamed in her honor. Finally, I appreciate the intellectual vitality that makes this section such a stimulating, ever-changing space. My thanks to each of you for your ongoing contributions!

Jason Seawright  
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*“Estamos vivos porque estamos en movimiento...  
...si quieres que algo se muera, déjalo quieto”*  
- Jorge Drexler, Uruguayan singer, *Movimiento*

## Letter from the Editor:

This issue is filled with questions—questions directed toward how we think about our research and what (or whether) guidelines should be in place regarding how we present it.

The issue’s principle symposium takes a critical point of view on the latter debate. Specifically, it questions the continued advocacy of Data Access and Research Transparency (DA-RT) in political science. While the value of DA-RT has been debated in the past, here, the authors push the discussion forward in multiple ways. For example, they re-define those parts of the research process to which we, as a scholarly community, should have “access.” They question the feasibility of (full) transparency and the ethics of replication when data collection is based on relationships of trust, exchange, and with marginalized populations. They also tackle the problem of DA-RT from the, perhaps, less obvious perspective of political theory.

To accompany the symposium, we invited a set of scholars to engage in a bit of back-and-forth regarding the theoretical and normative importance of DA-RT for the knowledge-building enterprise. The resulting debate is animated and informative. Indeed, I find these kinds of exchanges particularly fruitful and illuminating. By carving out specific and often competing opinions on a particular issue, they help us (the readers) better define where we stand.

This issue also includes a stand-alone article that provides a provocative re-exploration of how we distinguish qualitative from quantitative work. Among other things, it suggests that the growing popularity of Qualitative Comparative Analysis (QCA), especially at institutes that typically focus on *quantitative* methods, blurs the supposed lines that distinguish qualitative from quantitative methods.

In all, these works invite us to question, critically, the exercise, use, and application of qualitative and mixed methods. This is a necessary task, I think. Our pursuit of good, methodologically sound research requires that we continue to examine those methods, as often as we can and from as many different perspectives as possible. Only in this way—by constantly pushing our methodological endeavors forward—will we ensure the vitality of what we do.

Following the words of Jorge Drexler, cited above, “We’re alive because we are in movement... If you want something to die, keep it still.”

Of course, this task—understanding and innovating in the practice of qualitative and mixed methods—is one for which QMMR is particular well suited. It is my goal, as editor of this publication, to push for innovative articles from a variety of viewpoints and voices. I do this in the name of the methods that this publication seeks to promote and understand.

I also do this in the name of my former co-editor and dear friend, Kendra Koivu, who recently passed away after valiantly fighting cancer for years. Her departure is an immense loss for those of us who knew and loved her. It will also reverberate in our study of methods. Kendra was tireless as a scholar and an editor. She was fantastically smart. Her insights on methods—among other things—were brilliant.

Kendra worked almost to the end on QMMR. Editing this publication was, I think, a real labor of love for her. Frankly, I cannot promise to produce this publication at the level that Kendra and I could have done as a team. Her brilliance, as I suggested above, is in many ways unmatched. I will, however, work my hardest to ensure that QMMR stays on the cutting edge of methodological debates. It’s the best way to ensure that the study of qualitative and mixed methods forges ahead—stays in ‘movement,’ if you will—and remains vital to the social sciences. It’s also what Kendra would have wanted.

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**Symposium:**

Critical Deliberations Concerning DA-RT

**DA-RT and Its Crises**

Dvora Yanow  
Wageningen University

The authors of such [historical] works espouse actuality,  
*but that is not to say that they have replicated it.*

They may have dipped into document collections that...are incomplete...  
If they were going to rely on oral testimony, they could not...interview the dead.

Even among the living, they may have engaged in...a selection.  
Claude Lanzmann’s massive film *Shoah*...encompasses the testimony of perpetrators,  
victims, and bystanders. It includes Germany, Corfu, Auschwitz, Treblinka,  
Sobibor, Chelmno, and...in its raw, unedited state,  
questions and answers take up something like 350 hours.

...It is at this point that we must become specific about rules [of writing].  
One of them is...silence. Of course, there cannot be silence without speech.  
Silence can only be introduced between words, sometimes with words.  
...[I]n Lanzmann’s *Shoah*...after the first trainload was delivered to the death camp,  
it was the silence that alerted [the Sobibor station master] to something ominous.

...[W]e historians usurp history precisely when we are successful in our work,  
...[when] some people might read what I have written in the mistaken belief that here,  
on my printed pages, they will find the true ultimate Holocaust as it really happened.

—Raul Hilberg, “I was not there” (1988, 22-23, 25; emphasis added)

Critical deliberations concerning the Data Archiving and Research Transparency effort (DA-RT) which had been set in motion within the context of the American Political Science Association’s (APSA) Qualitative and Multi-Methods Research (QMMR) Section had, by the Fall of 2015, resulted in multiple conference workshops and panels, email exchanges, webpage and listserv posts, and various Section newsletter publications. Most of these seemed to come from Comparative Government and International Relations (IR) scholars, who are the mainstays of the QMMR Section. Researchers in other subfields of political science—notably, public policy, public

administration, public law, and political theory—were less often heard from among those deliberations. And so Peri Schwartz-Shea and I, both of us working in the first two of those subfields, convened a roundtable at the 2016 Western Political Science Association (WPSA) meeting, “Engaging DA-RT: Critical Assessments from Public Policy and Political Theory,” to address this gap. The essays in this symposium—by Renee Cramer (Drake University), Samantha Majic (John Jay College, CUNY), Amy Cabrera Rasmussen (California State University-Long Beach), Peregrine Schwartz-Shea (University of Utah), and Nancy J. Hirschmann (University of Pennsylvania), ordered by appearance here—were

developed from those roundtable presentations. (Amy T. Linch [Pennsylvania State University] was also a member of the roundtable, but she has not joined in this written compendium.) As panel chair, I set the stage for the discussion; and it is those comments that I present here, expanded to situate DA-RT in its contemporary context.

## **What problem does DA-RT solve? A bit of history**

DA-RT became the focus of attention of a large swathe of US political science in the Summer and Fall of 2015, in large part due to its proponents' call to journal editors to endorse the policy that articulated its aims, the Journal Editors' Transparency Statement (JETS). This statement instituted changes in manuscript and publication procedures, to be implemented in January 2016. Presented in 2014 as a policy endorsed by APSA<sup>1</sup>—DA-RT continued to appear on the APSA home page through at least the Winter 2017-18 without any indication of the ferment surrounding it—JETS calls on researchers doing empirical work to submit their unrestricted data to a repository at the time of publication. In the case of “cited data” that are restricted,

(e.g., classified, require confidentiality protections, were obtained under a non-disclosure agreement, or have inherent logistical constraints), authors must notify the editor at the time of submission. The editor shall have full discretion to follow their journal's policy on restricted data, including declining to review the manuscript or granting an exemption with or without conditions. The editor shall inform the author of that decision prior to review (Journal Editors' Transparency Statement 2015).

What problem or problems in the discipline was this policy intended to solve? That issue was never directly addressed, such that one has to infer the character of the problem(s) on the basis of APSA roundtable and QMMR Section discussions, APSA Council minutes, various publications, and other sources over the years, from 2009 on. DA-RT has had two central proponents, listed as co-founders on the second's webpage (Lupia n.d.). One is Colin Elman (Syracuse University), an IR scholar who specializes in IR theory, security studies, and qualitative methods. He is also a founder and leader of the QMMR Section at APSA and creator of the Consortium on Qualitative Research Methods, which runs the very

successful summertime Institute for Qualitative and Multi-Method Research (Elman n.d.). The second is Arthur (“Skip”) Lupia (National Science Foundation and University of Michigan), an experimentalist whose research interests include voting and elections, political psychology, and political communication, with a strong engagement with science communication issues (Lupia 2010). On the basis of these leaders' respective interests, along with firsthand knowledge of the QMMR Section's history, one might reasonably infer that DA-RT has been conceived as a solution to a perceived dual credibility crisis concerning US political science research, one relating to the politics of methods within APSA, the other, political science's public face.

With respect to the internal politics of science at APSA, the section originally named Qualitative Methods was created to give an intellectual home and visibility to research done using such methods as focus groups, comparative case analysis, process tracing, causal mechanisms, qualitative comparative analysis, concept formation, historical analyses, textual analyses, interviews, and participant-observer ethnography. The petition to found a new section, circulated at the 2002 APSA meeting and signed eventually by over 1000 members, was accepted by the APSA Council, and the Section was created the following year (see the essays in Qualitative Methods 2003). The motivation underlying this move was to gain greater legitimacy for qualitative methods in the face of the dominance of quantitative researchers, who had taken the generic language of “Political Methodology” for their own section, created in 1986. (I have been told<sup>2</sup> that the latter's founders were also motivated by legitimacy concerns, in their case with respect to economists, who apparently held that political scientists' quantitative research, being an “application” of econometrics to political scientific problems, was an inferior version of that method.) Sadly—in my view—and despite what appeared to be great success, assessed in terms of the size of Section membership, engaging and sometimes standing-room-only panels at APSA meetings, and a supremely well-edited newsletter, many Qualitative Methods Section members apparently continued to feel that their research was disrespected within political science. This led the Section's leadership in 2008 to propose, and the membership to vote for, a name change—from the original Qualitative Methods to Qualitative and Multi-Methods Research (QMMR),

1 On this point, see Schwartz-Shea and Yanow (2016). The involvement of APSA's leadership and staff is also documented at Data Access & Research Transparency (2015-16).

2 I no longer remember who told me this; but I do recall thinking at the time that the individual was articulating firsthand knowledge.

thereby bringing natural experiments, critical juncture analysis, quantitative content analysis, Bayesian analysis, and other such methods into the Section's domain.

The addition of "multi" (or "mixed," as it initially appeared in various communications) to the Section name seemed intended to signal that "qualitative" methods had a place alongside "quantitative" methods in US political science, as equals. Whether the name change achieved its desired result is unclear. My sense is that the feeling of being not-fully-respected, second class APSA citizens has continued to hover over the Section; indeed, one would want to know what indicators Section members would recognize as demonstrating full recognition and acceptance within the discipline. In light of this history, DA-RT seems like yet another effort to establish the legitimacy of qualitative research within APSA and various journals. It appears as if the thinking were that if QMMR members would make their data available to other researchers—to which end a Qualitative Data Repository was created, linked to Syracuse's Center for Qualitative and Multi-Method Inquiry (Qualitative Data Repository 2017)—thereby facilitating replication of their research, "qualitative" methods' legitimacy within political science would rise. That replicability has been problematic in many natural and physical sciences was seemingly not attended to,<sup>3</sup> nor were the potential confidentiality, security, and other ethical difficulties arising from making field research transcripts and notes public. (These included—initially—possible violations of ethics review committee policies and practices for the protection of human "subjects," a topic reworded in the 2012 revisions of the APSA Ethics Guide. On these points, see the symposium essays by Cramer, Majic, Cabrera Rasmussen, and, on the science question, Hirschmann). Replicability (discussed further below) has dropped out of most of the explicit discussion (see Tripp 2018, 734–35, for an exception), but other issues continued to be discussed, especially in several APSA sections' newsletters

(see, e.g., Büthe and Jacobs 2015; Golder and Golder 2016; Roundtable: Debating DA-RT 2016; Friendly Fire: DA-RT 2016; Qualitative Transparency Deliberations n.d.;<sup>4</sup> and Dialogue on DA-RT n.d.).

With respect to the public face of political scientific research and the politics of science external to APSA, research funding was being battered in Congress, increasingly coming under attack through curtailed budget allocations to the political science unit within the National Science Foundation's Social, Behavioral and Economic Sciences Directorate. "Science communication" was a chief concern of Skip Lupia's, as he noted in at least one conference panel discussion. The DA-RT-related thinking may have been that if political science research could be made to bear a closer resemblance to natural or physical science, or even to economics and psychology, it would fare better in the halls of Congress. Archiving "raw" data, such archives to be made publicly accessible for purposes of research replication (an emphasis downplayed in later discussions, as noted above), would make the research more "transparent" and, presumably, make it resemble more closely the kinds of research that NSF funded under other programs and directorates. Here, then, was the second "crisis" that DA-RT seems to have been designed to resolve.<sup>5</sup>

This analysis is supported by the third slide in the presentation that runs on DA-RT's homepage (see Figure 1):

**"What is DA-RT?"**

- **Three sources produce **credibility** and **legitimacy****
  - Data Access
  - Research Transparency
    - Production Transparency
    - Analytic Transparency

**Figure 1. What is DA-RT?<sup>6</sup>**

3 For a sense of the methodological argument and its application to the social sciences, consider Katz' (2019, 39-40) discussion of the rationale underlying replicability: "Replication' is especially informative in pointing out the subordinate status of a reflection theory of truth in research..." by contrast with a pragmatist theory of truth such as that advanced by William James. Katz draws on Erving Goffman's example of researching the concept of "with" in relationships, to illustrate the impossibility of replicating that and other field research: "We cannot replicate his finding in the sense of going where he was and lying in wait to observe the 'with' he witnessed. We intuit it would not make sense to try to track down others who were present and verify that those Goffman observed were indeed 'with' each other. *We care whether he is right or wrong, but we do not care that there is no evidence to show that his description reflects a prior time and place*" (idem., emphasis added). Katz adds how his own behavior validates Goffman's theory whenever he encounters several people walking towards him on a mountain trail.

4 Although discussion at this webpage ended some time ago, it continues to provide a description of the process by which QTD reports were generated, an overview of the topics taken up, domain statements of each, and links to the comments made at the time. Citations to specific reports relevant to this symposium's topics are included at the end of this introduction and in the essays.

5 As this symposium goes to press, NSF has gone beyond merely cutting funding to renaming what had been its political science programs, effectively eliminating "political science" as a funding category from the national scientific agenda. See, e.g., Political Science Now (2019). All 18 living APSA presidents since 1999 have signed a letter asking for at least a delay in implementing these changes.

6 Source: Data Access & Research Transparency (2015). This is the original layout, "credibility" and "legitimacy" appearing in red font. It is replicated at Elman, Lupia, and @DARTsupporters (n.d.).

Furthermore, Lupia's personal webpage, under "Projects," describes DA-RT this way: "A project that seeks to increase the *legitimacy, credibility, and public value* of social science by developing and supporting greater transparency in scientific practice" (Lupia n.d.; emphasis added). But how precisely (and why) accessing data and transparency would enhance the credibility and legitimacy of political science and resolve the perceived double crisis has not been sufficiently explained—nor has empirical evidence of missing legitimacy and credibility been provided (ironic, given that the slide that follows the one reproduced above leads with the need for "evidence-based knowledge claims"). Moreover, the process by which DA-RT was instituted as APSA policy, including the Association's ongoing support, was problematic. It involved consultation with a relatively narrow range of APSA members, leading to several unwarranted assumptions concerning its uptake in various corners of the empirical research world (see Schwartz-Shea and Yanow 2016 for details, and Hirschmann in this symposium on its status vis à vis political theory). Several scholars have noted a range of methodological assumptions embedded in the policy which do not fit with various forms of research, most of them qualitative-interpretive. Specifically, DA-RT assumes a methodological universality across the political science discipline, with "data," "archiving," and "transparency" presented as unproblematically common across all forms of research (on these points, see Cramer 2015; Pachirat 2015; Parkinson and Wood 2015; as well as the section newsletters cited above and the essays that follow in this symposium).

### **Rhetorical Arguments and the Politics of Science**

The "crisis" in the internal politics of political science that DA-RT addresses appears to use some of the same rhetorical devices at play in other scientific crises. One of the methodological issues underlying DA-RT's development and its implied or explicit claims, which has not received the discussion it warrants, is replicability. It remains the rationale underpinning the demand for transparency in qualitative research (as distinct from interpretive research), even if it has not recently been a central part of the discussion. As the APSA Ethics Guide (2012; emphasis added) states:

6. Researchers have an ethical obligation to facilitate the evaluation of their evidence-based knowledge claims through data access, production transparency, and

analytic transparency *so that their work can be tested or replicated.*

Replicability was also part of the initial JETS statement by way of a footnote to Andrew Moravcsik's "Active Citation: A Precondition for Replicable Qualitative Research" (2010, 29), which ties it to a "crisis" in political science: "...qualitative political science finds itself in self-imposed crisis. This crisis stems, above all, from a failure to impose firm standards of replicability." That substantive link disappeared subsequently from the JETS statement. However, "replication" featured in the "APSA NEWS" announcement on the Association's webpage for many months (last accessed for these purposes on January 31, 2017) announcing a "participatory, open-to-all members discussion" sponsored by QMMR, under the headline "Replication and Data Research Transparency (DA-RT)."

I know no one who is opposed, in principle, to "transparency" in research proceedings; but, as Stanley Fish (2018) and Lawrence Lessig (2009) both argue, the concept warrants much more detailed examination than it has received to date as to its meanings, both "real" and idealized. Fish reminds us that "transparency is not unambiguously a good thing," quite aside from whether it is, in fact, even attainable. With respect to speech—consider interviewing, for example—he writes, "silence and the withholding and sequestering of speech may be useful and even necessary in some contexts," something Raul Hilberg observed years ago (see epigraph) and colleagues doing research in conflict settings continue to remind reviewers and editors. Lessig (2009) asks,

How could anyone be against transparency? Its virtues and its utilities seem so crushingly obvious. But I have increasingly come to worry that there is an error at the core of this unquestioned goodness. We are not thinking critically enough about where and when transparency works, and where and when it may lead to confusion, or to worse. And I fear that the inevitable success of this movement—if pursued alone, without any sensitivity to the full complexity of the idea of perfect openness—...will simply push any faith in our political system over the cliff.

Much of its DA-RT-based discussion unfolded as if "transparency" were an unmitigated good, with singular meaning for all forms of research, and, furthermore, as if "replication" were a recognized and accepted hallmark of science (of any sort). Indeed, the language of replication or replicability, which has its origins in

experimental research design, has at times been conflated with the call for the reproducibility or duplication of findings when original data are re-used (see Schwartz-Shea, this issue, n. 5). More problematically, a lack of replication is sometimes treated as the lack of testability (see, e.g., Flaherty 2015). Additionally, the same examples of egregious scientific misconduct—e.g., by psychologist Diederik Stapel or biologist Marc Hauser, each of whom fabricated data in his experiments—are invoked over and over, to justify the need for making data available for replication even in research that is not experimental. And arguments concerning research design criteria that may well fit psychological or biological experimentation are being extended, in a methodological leap of faith, to participant-observer, ethnographic research, with Alice Goffman’s (2014) study serving (without warrant) as the most recent example of misconduct. Some, at least, of DA-RT’s initial critics accepted the legitimacy of having and instituting such a replication-cum-transparency policy, looking only to tinker with its language to enhance its suitability for a wide range of qualitative and interpretive methods, rather than challenging its fundamental framing (but see Fujii 2016 for an exception).

Several of the essays in this symposium show the extent to which DA-RT constitutes the political construction of a crisis. I would extend that to a consideration of the politics of changes in higher education worldwide, and in particular to both funding cuts in and the curtailed independence of social science research (e.g., the UK’s “Research Excellence Framework” and its predecessors and current version; see Wright 2016; in her essay Hirschmann sounds a related theme). These manifestations of New Public Management thinking (see also Schwartz-Shea’s essay) are also playing out, to some extent, in some IRB research regulation practices: all of them seek to rein in researchers and regulate their actions, flying in the face of longstanding values concerning academic freedom. The essays presented here help to clarify what questions need asking, if not what the answers might be. How, and why, have these “normal science” research procedures suddenly become crises? What are the implications and consequences of framing them as a crisis? What are the implied threats of “non-DA-RT research”—and who is being threatened? What are the impending perils of *not* adopting a DA-RT-like policy (an idea signaled by the crisis framing of the issues)? How is it that so many proponents continue to ignore the consequences—for

research, for newer political scientists’ careers—of their stance?

The authors of these symposium essays are not opposed to the idea of science as a public good (Kolowich 2016; indeed, see Cabrera Rasmussen, this issue, on this point). But trust in science rests on more than just replicability. The practices of ethical research conduct (including, in many cases, complying with ethics review board requirements) and of data sharing are on a collision course, as more and more editors, granting agencies, and other gatekeepers demand from field researchers that, if they cannot share fieldnotes and interview transcripts, they identify research participants in their published work by various demographic details (see, e.g., Harper 2018, Krystalli 2018)—not recognizing or accepting that such revelations, even when names are anonymized, could lead to job loss, torture, and even murder (e.g., Georges Condominas’ experience with the unauthorized translation of his ethnography into English for circulation among US Green Berets in Vietnam, leading to the subsequent torture and killing of one of his “informants” [Salemkink 2003: 3-4]; see also Harper 2018; Knott 2019, esp. 145-47, 148; Krystalli 2018; Tripp 2018, 733). Several of the difficulties posed by DA-RT for qualitative and interpretive research were discussed and debated in various of the QTD workgroups as they produced their reports. Among the more relevant of these to the issues engaged in this symposium are:

- on vulnerable groups: Lake, Majic, and Maxwell (2019) (discussed by both Samantha Majic and Renee Cramer in this symposium);
- on peer review: included in Schwedler, Simmons, and Smith (2019) (see discussion in Schwartz-Shea’s essay in this issue);
- on ethnographic and participant-observation research: Schwedler, Simmons, and Smith (2019) (discussed in Renee Cramer’s essay);
- on interpretive research and its methodological implications: Björkman, Wedeen, Williams, and Hawkesworth (2019) and, on transparency specifically, Luke, Vázquez-Arroyo, and Hawkesworth (2019) (both of these informing much of the methodological backdrop to this symposium’s essays).

Underlying DA-RT’s policy and procedural thinking is a curiously, and doubly, un-political assumption: that “data” can be freed of their political contexts (in the process of “sharing” them through a collective storage facility) and that they can then be re-used (in replicating the research that produced them) in a similarly apolitical

process, transplanted into a new Petri dish that itself is absent all politics. Lessig (2009) quotes Archon Fung, Mary Graham, and David Weil from their book *Full Disclosure: The Perils and Promise of Transparency*: “[R]esponses to information”—think: archived data—“are inseparable from...interests, desires, resources, cognitive capacities, and social contexts. Owing to these and other factors, people may ignore information, or misunderstand it, or misuse it. Whether and how new information is used depends upon its incorporation into complex chains of comprehension, action, and response” at the hands of researchers who did not generate that information, accessing it, instead, detached from the complex chains in which those researchers who did generate the data were immersed. This is a way of thinking about research that treats it, as the late Lee Ann Fujii put it (personal communication, March 13, 2014), as if it were not “a highly social and socializing practice that bumps up against and lives through enduring social structures of race, age, gender, social class, etc., etc.”

The assumption that data can exist in a pure, uncontaminated state and can in that way be conveyed via a repository to other researchers itself rides on a particular, older model of communications—that message “senders” and “receivers” can, in a perfected state, communicate with one another through a channel that is, or can be rendered, devoid of any “noise” distorting the communication. The latter is the second assumption of “un-politicality” in the DA-RT policy. That model was relinquished years ago in communication studies as the discipline’s scholars recognized the impossibility of noise-free communication and the presence of what in literary analysis is known as reader-response theory: that “receivers” interpret “messages” in light of their own experiences, etc. In other words, data are not, and cannot be rendered, “pure,” neither in their generation nor in their use. Data “have” politics (to paraphrase Langdon Winner, 1980). That is the point made in one way or another by the scholars writing these essays, as when Renee Cramer says,

I accept that what anyone can know about the “rightness” of my work relies on their own evaluation of the interpretation I make of the world I observe and interact with. This isn’t as simple...as checking my math in an equation, checking my code book for errors, or thinking about the variables I use and the value I assign them. Knowing if I am “right” becomes less important than the superordinate question: “Is my interpretation persuasive...[,] plausible...

[, or useful in] shed[ding] light on related phenomena?” (this issue, page number)

This is what Hilberg (in the epigraph) was driving at, in part, in noting the massive amounts of “field” data that historians draw on, the “multi-sitedness” that characterizes their research, the silences that cannot be duplicated (or replicated), and the mistaken belief that a realistic replication of field experiences can yield any form of “truth.”

## The Essays

### Five essays follow this introduction:

- Renee Cramer, in “Trust, Transparency, and Process,” thinks through the impact that DA-RT has for scholars engaged in participant-observation and ethnographic research, those doing interpretive work, and those working with IRB approval meant to protect vulnerable subjects from adverse impact.
- In “Not There for the Taking: DA-RT and Policy Research,” Samantha Majic draws from her investigations of sex work-related policies and political activism in the United States to challenge DA-RT’s conception of research, and in particular policy-focused research, as an “extractive” enterprise (Pachirat 2015) that simply draws on materials that are generally visible and available to the public, such as administrative documents and court decrees.
- Amy Cabrera Rasmussen’s “DA-RT: Prioritizing the Profession over the Public?” engages the impact that the DA-RT policy will have on political science’s engagement with the public, especially on its marginalized segments, and on members of the discipline lacking professional status and security. In particular, she argues that DA-RT is an example of ways in which political science has tended to prioritize discipline-focused ethics over researchers’ ethical responsibilities to the public. In light of the external “crisis” noted above, I find tremendous irony in her argument that DA-RT threatens researchers’ relations with members of the public—the very audience whose esteem of science DA-RT was intended to raise.
- Peregrine Schwartz-Shea’s essay, “Every Reader a Peer Reviewer?” examines how DA-RT’s founders failed to take up two key dimensions of the policy: whether a “full” or “complete” explication of the steps in a research process is possible or desirable; and the relationship of DA-RT to peer review, including the “de-skilling” of researchers implied when *any* reader—

as contrasted with a peer reviewer—is assumed to be competent to assess a researcher’s decisions and interpretations.

- And in “Data, Transparency, and Political Theory,” Nancy Hirschmann considers these several issues from the perspective of political theory, which she maintains already adheres to the spirit of transparency and access. She raises the dangers of political theory’s further marginalization from the discipline and makes a plea to political theorists to join the conversation.

DA-RT policies and practices remain a contested issue. The number of journals whose editors signed on to the JETS statement remains, as reported on the webpage, at 27;<sup>7</sup> but the rhetoric used to persuade scholars of the necessity of DA-RT as a policy has been ramped up, as can be seen in this statement from the Data Access & Research Transparency webpage (Elman, Lupia, and @DARTsupporters n.d., penultimate slide; emphasis added): “By date X [sic], all of the journals *in which you most want to publish* will require data-sharing and comprehensive documentation.” And in a 2018 review essay, Elman, Kapiszewski, and Lupia (2018, 29; emphases added) claimed, with respect to DA-RT, that “...despite the challenges, *consensus* about the value and practice of transparency *is emerging* within and across political science’s diverse and dynamic research communities.” Such messages are being repeated, inter alia, to doctoral students attending the summer Institute for Qualitative and Multi-Method Research at Syracuse’s Maxwell School, their repetition ignoring both problems and contestations. However, as Nancy Hirschmann (personal communication, September 24, 2018) observed, Aili Mari Tripp’s (2018) assessment of DA-RT’s implications for those doing field research in authoritarian and politically sensitive contexts suggests that perceptions of consensus likely depend on which scholars and scholarly discourses perceivers are interacting with.

Tripp (2018, 737) voices another concern—that scholars working in such contexts are leaving political science for other disciplines, such as history and sociology, including because of DA-RT:

Political science as a field at times appears to be asking narrower and narrower questions

of decreasing importance[,] and the attempt to discipline (in the Foucauldian sense) qualitative researchers through DA-RT is yet another step in this direction. ... [T]his could also be seen as an attempt to...discipline scholars who study the messy reality of politics in such contexts—and to get us to study people and places using methods that are considered more “scientific” than others.

The authors of the essays in this symposium share these concerns. We join our voices to others critiquing the adoption of DA-RT as APSA policy, as if all of the Association’s members, not to say all political scientists, everywhere, endorse the policy’s narrow understanding of social scientific methods and purposes.

Finally, it is nearly a truism in public policy studies that policies are often crafted “today” to address “yesterday’s” problems. That surely seems to characterize DA-RT at the moment, in light of Donald Trump’s muzzling of the EPA, NIH, and other agencies engaged with “hard” science—something none of us, including DA-RT proponents, could have envisioned. I join my colleagues in this symposium in seeing DA-RT coming across as a misguided effort to address the internal and external “crises” delineated above. In the contemporary political world, DA-RT seems even less adequate in the face of the wholesale attack under way, in the US and worldwide, on the standing of science, facts, and truth in politics and society.

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7 This is up two from the initial, October 31, 2014 list; but the reported numbers are different from the number of journal titles listed both in 2014 and currently, making the changes difficult to assess.

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# Trust, Transparency, and Process

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Recent calls for Data Access and Research Transparency (DA-RT) come from a reasonable desire to hold scholars accountable for their research practices and processes (Lupia and Elman 2014). These calls, however, also come from disciplinary perspectives that seem to misunderstand interpretive and qualitative research in some fundamental ways; further, they offer solutions to problems that would be much more appropriately fixed through processes of peer review. My not-so-revolutionary position on DA-RT for scholars who undertake interviews and fieldwork—especially for scholars who do so with vulnerable populations and around sensitive questions—is that *where data cannot be transparent, explanations of the processes of data collection must be*.<sup>1</sup> But, because explanations of these processes are standard parts of scholarly articles and books that elaborate these projects, and because peer review often centers on questions of method and interpretation, an additional layer of expectation—such as that proposed through a requirement for active citation (Moravcsik 2010)—is not only unnecessary, it is onerous, and potentially chilling for research on vulnerable populations and sensitive questions.

My work focuses on people and groups of people who want to be seen, and not seen, by the administrative state (Cramer 2015, 2009, 2006). I am an interpretive law and society scholar trained as a political scientist, and I publish within both disciplines. My current project, funded by the National Science Foundation, examines the regulatory landscape for midwives in the United States, some of whom are operating in states where their practice is criminal. It also examines the legal, political, and cultural mobilization to seek legal status and decriminalization undertaken by midwives and their advocates.

This research involves participant observation, ethnographic immersion, and interviews—as well as archival work and content analysis of news stories, trial transcripts, and legislative testimony. Drake University's Institutional Review Board (IRB) agrees with me that there

are substantial risks to many of my interview subjects (some of whom are practicing midwifery without benefit of license) and that making even some interview data publicly available would jeopardize those individuals. My institution's IRB also agrees that releasing field notes detailing my participant observation with midwives and their advocates would, even if redacted, have the potential to jeopardize these women. Further, those with whom I interact and interview have made clear that they do not want their interview transcripts or my notes of our interactions made public. These participants place their trust in the relationships we have formed, my scholarly credentials, and in my previous work. They do not place their trust in the hands or minds of other researchers, whom they do not know.

As the authors of the Final Report of the QTD Working Group on Ethnography and Participant Observation write, “ethnographers not only grapple with questions of openness vis-à-vis the scholarly community, but also in our research sites and with our interlocutors when we are in the field” (Schwedler et al. 2019, 2). My accountability first and foremost lies with the people who participate in my study.

I agree that I am also accountable to my discipline. And, especially because my research is publicly funded, I am accountable for being as transparent with my data as is ethical, responsible, and possible. I achieve this transparency of data through partnerships with midwifery organizations that can be more public in collecting statistical data about birth, outcome, and perceptions of legality.<sup>2</sup> I further achieve this by making all of the trial transcripts, press, and archival material that I use in my analysis available online at the close of my project.

What I will not make available, as should be clear from the opening paragraphs of this essay, are the interview transcripts and field notes that I collect as the bulk of the data for this project. Transparency, for research on sensitive topics with vulnerable populations, should be about transparency of *process*. As the Final

1 Though I should not be confused with Katherine Cramer (2015), my position on this issue mirrors hers.

2 I work with members of the Midwives Alliance of North America statistical research team (MANA Stats), as well as with a group of interdisciplinary scholars collaborating with MANA Stats. We have articles under review in medical and public health journals, and are in the process of creating a website that features searchable maps overlaying data relating to outcome, regulation, demographic measures, and scope of practice.

Report of QTD Working Group IV.3 (“Research on Vulnerable and Marginalized Populations”) emphasizes, the process of peer-review for this work will focus on factors such as:

the relationship between the time a scholar spent in their field site and their stated methodological approach; knowledge of the geographic variation/specificities; language skills and embeddedness; the use of local labor in the form of RAs, or facilitators; and discussion of how the political environment might have influenced their work (Lake et al. 2019, 3).

Data access for ethnographic and interview work—especially that work done with vulnerable populations—should be *access to my way of thinking and analyzing*, not access to those who participated in the research. Transparency about process and method, rather than transparency that enables an intrepid soul to find my field sites or study participants, is necessary not just because it keeps participants safe but because it enables them to have “security and peace of mind” (Lake et al. 2019, 6).

What is more, the research can neither be replicated,<sup>3</sup> nor reproduced, because it relies on *relationships*—my expertise, my showing up, their trust and capacity to share. No other researcher walking into the same situation would have the same access (some would have more, some less) or experience. A clear tenet of feminist political theory and critical race theory is that positionality, standpoint epistemology, and acknowledgment of embodied knowledge are tremendously important.<sup>4</sup> Simply put: my social identities influence my access and my interpretation. The relationships I build with participants also influence access and interpretation. While anyone could walk into the homes and offices of the people I interview, absent a relationship with them, the quality, content, and nature of the conversation would be different. Other researchers can have access, in other words (where safe for my participants), to the identities of those with whom I work; they cannot have access to the relationships we forge. But, as Schwedler et al. note, “ethnographers encounter, absorb, and process more data than could ever appear in field notes” (2019, 6).

In other words, even if I were to grant access to them, it must be understood that having my field notes is not the same thing as having my data. My data are, in

part, embodied in my own memories of and experiences with the research project itself.

It is my job as an interpretive scholar engaging in embedded interview and fieldwork research to explain my process of data gathering, to be reflexive and clear about my relationship to the work and the participants, and to convince you of my interpretation of what I observed. I should be clear, in my work, about how many field sites I had, how many hours I spent in them, how many interviews I did, and how many informal conversations in which I participated. You should know which archives I visited and where the archival and web data I found can be accessed. I should be clear about how many pages of field notes and transcripts I worked from, and how those field notes and interviews contributed to the way I searched archival material. And, as a key part of a transparent process, I should be clear about how I analyzed and interpreted those notes, transcripts, and other data. These practices are all widely acknowledged to be “well established standards of evidence and agreed-upon means of evaluating whether claims are valid and have been adequately substantiated with evidence” (Schwedler et al. 2019, 5; citing Yanow 2014, 2009, 2006).

Instead of focusing on what may be a strawman argument at this point in the DA-RT debates, though, regarding whether my interview transcripts and field notes should be available via hyperlink to any journal reader who wants them, I’d prefer to focus on the question that I have heard posed, in good faith, from scholars who do not do interpretive work. They ask: “How do you know that you are getting the world right? Why should others give credence to your claims?”

I appreciate the question—because it is a reminder that we all work, even within the same discipline, from such disparate traditions. Indeed, how do we know we are getting the world right?

Well, we might not.

That is why I do interpretive work.

I accept that what anyone can know about the “rightness” of my work relies on their own evaluation of the interpretation I make of the world I observe and interact with. This isn’t as simple (and I do not mean simple in a derogatory way, the most beautiful things can be simple) as checking my math in an equation, checking my code book for errors, or thinking about the variables I use and the value I assign them. Knowing if I am

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3 Schwedler et al. (2019) make this clear at page 6: “Ethnographic work cannot be replicated.”

4 There is a rich literature to consult, here. I often rely upon: Strolowitch 2013; Alexander-Floyd 2012; Hancock 2007; Milner 2007; England 1994; Wasserfall 1993; DeVault 1990; Collins 1986, as well as the essays collected in Alcoff and Potter (1993).

“right” becomes less important than the superordinate question: “Is my interpretation persuasive?” Or, “Is my interpretation plausible?” Or even better, I think, “Is this interpretation one that can shed light on related phenomena?”

Interpretive ethnographic and interview-based researchers do have standards for articulating answers to these questions. These questions come up routinely in peer reviews of our work. There are entire journals dedicated to thinking through how to evaluate an article or book’s qualities of reflexivity, validity, interpretation, method, self-reflexivity, articulation of positionality, reciprocity, and ethics.<sup>5</sup> We have vibrant conversations about them, and we argue over whether a work achieves them, and in what ways. Those conversations are at the heart of our scholarly endeavor as a community—just as similar conversations are at the heart of those who practice other methodologies or speak to other traditions.

There is simply no need to recreate the wheel to include the work of interpretive scholars; and there is no need to create a state of exception for our work, either. We should expect data to be available where it is possible and understand and believe explanations when it is not. We should evaluate the processes by which data were gathered and analyzed and hold an expectation that good scholarship is indeed transparent in that regard. And we should take a deep breath and trust in the processes of mentorship, peer review, and data analyses that have brought us decades of good, valuable, and persuasive interpretive work.

Political Science has had crises, of late, that may make it feel difficult to do just this: to breathe deeply and trust.<sup>6</sup> I am not advocating naiveté, certainly—and I absolutely do not want to see a reduction in rigor and accountability for scholars, mentors, and reviewers. But I would argue that perhaps our discipline has more important crises to attend to than the data access and transparency concern raised by singular hoaxes and occasional lies. I am more concerned with the perception that our discipline lacks relevance to the very topics we purport to care most about: politics, policymaking, and political engagement.

Some argue, convincingly, that as journals in our field publish more and more statistical work and formal

theory, they publish less work that includes policy analysis and policy recommendation (Desch 2019). Others allege that as we focus on speaking to policymakers, political scientists forget about speaking to the general public (Farrell and Knight 2019). Often, those most suited to speak to either constituency find themselves ignored. As is clear from the existence of successful movements like #WomenAlsoKnowStuff, many members of our discipline never get the phone call from a journalist, or the invitation to be on a panel even when we want to be relevant, simply because it is presumed that women might not actually “know stuff” about our field (Beaulieu et al. 2017). Let’s not lose sight of the fact that much of the research done on issues that impact vulnerable populations and marginalized groups is produced by members of those communities themselves: women and feminist scholars, scholars of color, queer scholars, scholars with disabilities. When norms of the discipline make it more difficult for us to publish, and when those norms seem to construct barriers to the publication of studies meant to inform and improve policymaking for and politics by marginalized populations, we further reduce our relevance as a discipline.

Certainly, it may be easier to put statistical analyses through the paces of an elegant DA-RT procedure than it is to find peer reviewers who can read the nuance of an ethnographic methods section. But I am concerned that incentivizing that kind of ease in research and publication means we are missing—indeed turning our backs on—the kind of work that led many of us to the study of political science in the first place: data-informed policy analysis that enables us to understand how particular political manifestations and policies impact the daily lived experience of average, ordinary humans. If we want political science to be more accessible and transparent, an effort to speak to and write for policymakers and the general public—indeed, to make our work *accessible* to them—is a legitimate, and important, place to start.

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5 For instance, bAoth *The International Review of Qualitative Research* and *Qualitative & Multi-Method Research* devote much space to these issues. Other disciplines, such as Anthropology, also have spaces in which these conversations occur.

6 I am thinking of the LaCour data falsification scandal that rocked UCLA and the discipline in late 2015, among others, as well as the widely reported “Sokal Squared” hoax articles that came to light in 2018, which purported to show lack of rigor among journals dedicated primarily to women’s and gender studies, queer theory, and post-modern theory.

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# Not There for the Taking: DA-RT and Policy Research

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Political scientists frequently study government policies—the tools that shape behaviors towards certain outcomes and allocate values in a society. Found in laws, administrative documents, court decrees, and the practices of government administrations, these tools are generally visible and available to the public. Therefore, one may assume that DA-RT—the APSA-sponsored initiative that requires scholars to reference the data they generate and provide other scholars with access to these data by depositing them in a “trusted digital repository”<sup>1</sup>—will not impede public policy researchers. In the pages that follow, I draw from my experience conducting research about sex work-related policies and political activism in the United States to challenge this assumption. To do this, I question DA-RT’s conception of data and its understanding of (policy) research as an “extractive” enterprise (Pachirat 2015).

Sex work involves the exchange of sexual services for cash or other trade. In the United States, public policies variously define these services as legal (e.g., exotic dancing, pornography) and illegal (e.g., prostitution). My own research has considered sex workers’ efforts in the San Francisco Bay Area to oppose criminalizing prostitution and create nonprofits that improve sex workers’ occupational health and safety (e.g., Majic 2014a, c). I have also studied policy initiatives to “end demand” for prostitution and other forms of sex work in the US, including “john schools,” where men arrested for soliciting a sex worker pay a fine and attend class to learn about consequences of their actions (Majic 2015), and public awareness campaigns (directed mainly at men) about the harms of prostitution (Majic 2017).

Like many other policies, prostitution laws and the details of “end demand” initiatives like john schools and public awareness campaigns are publicly available. Therefore, one may assume that these policies are easily “research-able” and that referencing them and providing other scholars with access to related data (e.g., the text of a state’s prostitution law or a poster from a public awareness campaign) is a simple matter. But while such assumptions are certainly reasonable, they reflect and reinforce the power relations implied by DA-RT’s model

of research, where, as Timothy Pachirat writes, “the researcher’s relationship to the research world is *extractive* in nature and ... transparency and openness are prized primarily in the inter-subjective relationships between researchers and other researchers, but not between the researcher and the research world from which he *extracts information* which he then *processes into data for analysis*” (2015, 29-30).

Put simply, DA-RT’s “extractive ontology” (Pachirat 2015, 30) assumes that data are simply available for the researcher to take and share as she sees fit. While this may certainly apply to some forms of policy research that use data from large, publicly accessible sources (for example, studies assessing state crime statistics, or studies of the use of public programs over time), this is not the only way to study public policy. In fact, policy research may also be a highly *interactive* endeavor where data are *not* simply extracted and, by extension, easily shared.

My own studies of sex work-related policies have considered how sex workers resist policies (namely, laws criminalizing prostitution) and create alternative health and social services for their communities within an otherwise hostile “policyscape” (Mettler 2016). And, in my study of policy initiatives to “end demand” for prostitution, I have considered the implementation of john schools and public awareness campaigns to understand the normative discourses they convey and reinforce about sex work, gender, and race. To conduct these studies, I could not simply “appear” among sex worker rights activists or at john schools and “extract” the data I needed, such as interviews, documents, observations, and program histories and statistics.

Instead, to conduct my research, I had to develop relationships with different communities in order to access data. In my study of sex worker rights activism, this included relationships with individual sex worker activists and representatives from the nonprofits they formed, namely the St. James Infirmity (SJI), the world’s only occupational health and safety clinic run by and for sex workers; and the California Prostitutes Education Project (CAL-PEP), which offers mobile HIV testing and other health services by and for sex workers and

1 <http://www.dartstatement.org>

members of other street-based communities. And in my studies of john schools and public awareness campaigns, I developed relationships with the representatives from the district attorneys' offices, police officers, and charitable organizations that oversee these programs in cities as diverse as New York, Phoenix, San Francisco, and Atlanta.

Certainly, one may ask why I needed to develop relationships to obtain data about these communities and programs. After all, they receive public funding, and they aren't "secret" in any way. However, this presumption is rooted in the "extractive" understanding of research, noted above, which is of limited use when studying marginalized communities like sex workers, who have long been subjects of research but rarely partners in the process (Bowen and O'Doherty 2014). As Gloria Lockett, CAL-PEP's founder and executive director explained to me, she was "sick of mostly white researchers coming to the [sex worker] community, taking what they needed, and leaving" (Majic 2014b, 12). And in john schools, the administrators were often concerned about protecting the privacy of the men in the classes, and they were also often anxious about what I might write about their programs.

As a result, my access to research sites and communities has *depended on* establishing trust and engaging in some kind of exchange. In my research with CAL-PEP and the SJI, for example, in exchange for interviews and access to program data, I volunteered as a grant writer and helped with various tasks at these organizations, such as filling baskets of condoms and doing the dishes. To visit the john schools and conduct interviews with individuals who initiated and ran various public awareness campaigns, I agreed to various conditions (e.g., not recording the john school classes) and offered to share anything I wrote and published about their programs. In all cases, the research participants presumed that I would be the sole proprietor of any notes I took during interviews, while observing john school classes, etc.

Many scholars have expressed concerns in various articles and through the Qualitative Transparency Deliberations (QTD) process (among other forums) that the DA-RT initiative will force researchers to make data such as interview and field notes widely available when they publish in subscribing journals. DA-RT proponents have responded that this is not the case, particularly where privacy and confidentiality risks exist.<sup>2</sup> While I

generally side with DA-RT's critics in these debates, I raise here a slightly different concern: How does DA-RT ensure that those who access data that were originally collected and made available by scholars who conduct *reciprocal, interactive* policy research *also* develop trust and relationships with the communities under study?

To illustrate this concern, my time at the SJI is instructive. Here, among other things, I wrote grants and helped out during clinic nights in exchange for conducting interviews with staff and accessing organizational documents. In one instance, a grant application that I submitted to the National Minority AIDS Coalition funded a new computer for the SJI's community room. Now, if the journals in which I published my research had been DA-RT subscribers at the time, they might have asked me to make my interview and observation notes available to other scholars. Presumably, I would have asked the editors to exempt my interview notes (sex workers are, after all, a marginalized community, and many of my interviewees did not want me to use their names or other identifying information). But perhaps the journals would have required me to share my observation notes from my time at the clinic.

My concerns about sharing these notes are two-fold and extend from my practice of reciprocal, interactive policy research. First, to collect the data contained in these notes, I had to expend a significant amount of "sweat equity" to develop the relationships and trust needed to access my research sites. How is it fair to me, then, for another scholar to access my notes when he or she has done none of the (often frustrating and time-consuming) relationship-building work? Second, and more significantly, how can I guarantee that the researcher who accesses my data will *also* offer something to the community/organization studied? For example, will he also write a grant to the National Minority AIDS Coalition to obtain a (second) computer as "thanks" for the data?

Certainly, expressing such concerns may cast me in a petty and selfish light (or, at the very least, as someone with something to hide); however, this is not my intent or motivation. Instead, I raise these concerns to indicate how DA-RT inadvertently encourages a particular type of top-down policy research with data that are publicly available and easy to obtain. While such research is certainly important, ease of data access should not be the key indicator of "good" (transparent, publishable)

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2 For a more detailed discussion of this debate, see the 2015 issue of the Qualitative and Multimethod Research Newsletter (Various Authors 2015) and work published by DA-RT's key spokespeople (Lupia and Elman 2013). The QTD discussions and reports are available at <https://www.qualtd.net>.

public policy research. Instead, as the QTD Report “Vulnerable and Marginalized Populations (Working Group IV.3)” indicates, we can assess the quality of research by reviewing the extent to which authors *explain their process* of generating and analyzing evidence (Lake, Majic, and Maxwell 2019). With this standard, scholars are better able to pursue and publish policy research that involves data that may be difficult to access and share, as they would not be expected to provide it in raw form. Instead, they would have to explain their collection and

analysis process so others may understand (and even replicate) their methods and assess the veracity of their findings and conclusions.

Given the range of policies that impact our lives on a minute-by-minute basis, I believe the discipline of political science must encourage research on a wide range of policy questions with diverse sources of data. By ignoring policy research that requires interactions, relationships, and reciprocity, DA-RT’s “extractive” ontology may just encourage the opposite.

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# DA-RT: Prioritizing the Profession over the Public?

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Critics of the American Political Science Association's Data Access and Research Transparency (DA-RT) policy have targeted the initiative on many fronts, not the least of which is the impact that the policy will have on political science's engagement with the public—evocative of the recurrent appeals to remake the discipline with an eye to relevance, usefulness, and comprehensibility (Isaac 2015).<sup>1</sup> DA-RT's proponents seem assured of the positive impact of the policy, heralding its contributions to transferring knowledge beyond disciplinary boundaries, and in particular toward improving political science's public face, making it more credible and legitimate (Lupia and Elman 2014). However, I argue that DA-RT overemphasizes the purported disciplinary benefits without adequate consideration of the probable harms to the public. This is especially the case for marginalized communities and the policy issues that affect them, and is likely to result in a chilling effect on such research.

## The Discipline and the Public Good

The Preface to the APSA's *Guide to Professional Ethics for Political Science* describes the organizing impetus for the association's Standing Committee on Professional Ethics, stating that the Committee was created in 1968 to “protect the rights of political scientists” (2012, i). Despite subsequent iterations of both the ethics committee and the ethics guide, there remains no introductory reference to the public or the public good. For instance, when compared with the preamble to the document produced by our cognate discipline, sociology, and its professional entity, the American Sociological Association, a clear distinction becomes apparent. In the ASA's *Code of Ethics and Policies and Procedures of the ASA Committee on Professional Ethics*, disciplinary goals are laid alongside what is termed “social responsibility”: “Sociologists... strive to advance the science of sociology and to serve the public good” (2008, 6-7). Here, and elsewhere in the ASA ethics guide, ethics are delineated that go well

beyond individual scholars and the discipline and are explicated in detail.

It is perhaps no coincidence that a close examination of the documents and arguments undergirding the DA-RT policy also makes clear a primary focus upon benefits for the discipline, with only secondary and vague assertions of the contributions that could be made to the public good. Both “Draft Guidelines for Data Access and Research Transparency in the Qualitative Tradition” and the APSA ethics guide give a nod to a limited set of alternative professional ethical responsibilities, but they tilt the balance of focus toward the asserted disciplinary benefits. For example, responsibilities are listed—to human subject requirements and professional norms (privacy and confidentiality), as well as to legal principles and institutional actors (copyright rules, law enforcement, grand juries). Yet, these largely legal responsibilities amount to a constrained sense of the potential ethical responsibilities of a researcher.<sup>2</sup> Absent is a more thoroughgoing examination of potential alternative ethical concerns in any level of specificity comparable to those goals and values that are discipline-specific.

Further, researchers are advised by the APSA ethics guide to prioritize transparency over competing values. “Decisions to withhold data and a full account of the procedures used to collect or generate them should be made in good faith and on reasonable grounds”; further, researchers are instructed to “exercise appropriate restraint in making claims as to the confidential nature of their sources, and resolve all reasonable doubts in favor of full disclosure” (APSA 2012, 10). Yet, questions remain: What would entail “reasonable” grounds, especially as the default is specifically set to be the provision of transparency, operationalized primarily as data access? (More on this below.) Surely if dedication to the public good were prioritized, it would lead to erring on the side of preventing harm or exploitation. Here again, the ASA *Code of Ethics* provides a contrast: “While endeavoring to always be collegial, sociologists must never let the desire to be collegial outweigh their shared responsibility

1 See the Perestroika-themed symposium in *Perspectives on Politics* accompanying the Isaac (2015) piece for an important recent iteration of this discussion.

2 A similar point is made in the Final Report of QTD Working Group on Research Ethics and Human Subjects: A Reflexive Openness Approach. Here MacLean et al. assert: “Conducting ethical research goes well beyond the IRB review process and the Belmont principles, however, which are themselves incomplete and subject to internal conflict” (2019, 4).

for ethical behavior” (2012, 6). ASA guidelines also note that shared data ought to be subject to safeguards equal to or greater than those established by the originating researcher.

In the draft guidelines and their introduction to the *PS* symposium “Openness in Political Science: Data Access and Research Transparency,” Lupia and Elman (2014) also expand upon the underlying logic of these ethical guidelines and reveal their clear priority concerning openness: data access. While suggesting that three “constitutive elements” are involved—data access, production transparency, and analytic transparency—they reveal in many ways their primary preoccupation with data access. Political science scholars largely agree that production transparency and analytic transparency, in one or another form, are worthy goals. Disagreement increases when considering whether such transparency is truly a problem in all sections of the discipline, and concerns among interpretive scholars like myself have also coalesced around the “DA” portion of DA-RT: namely that data access requirements are either unworkable in practice or are deemed to be fundamentally detrimental to the research and to those individuals and groups being studied.

### **Toward a More Thoroughgoing Engagement with “The Public(s)”**

DA-RT’s proponents tell us that in addition to replication, there are other compelling reasons to support the value of openness. They assert that if one cares about underrepresented individuals and groups, one ought to support data access because it will increase the strength and span of these communities’ voices. In the draft “Guidelines for Data Access and Research Transparency in Qualitative Research in Political Science,” Lupia and Elman assert:

For instance, those who believe that an important social scientific task is to encourage the recognition of the extent and importance of cultural, historical and social diversity should acknowledge the value of transparency in permitting the record of actors speaking in their own voices to reach readers of social scientific texts (2014, 28).

Yet, as argued, the DA-RT policy does not seem to include as its originating impulse any connection to such publics. The DA-RT policy seems to be centrally focused on the public as a circumscribed set of elites, predominantly policymakers and especially funders. Certainly, the insights garnered by political science as a

discipline and individual political scientists can usefully inform political decision-making, public discourse, and assessment of policy alternatives both domestic and global. Yet, many of us are engaged with the public in a different way—not as an audience but rather as the subjects of our research, gathering and, in some cases, co-producing empirical research. We work closely with community members and organizations that are deeply involved in public matters of importance. Cramer (2015) powerfully advances the argument that DA-RT would affect a researcher’s integrity and interactions with respondents on matters of public opinion. Scholars who do work within authoritarian regimes (Shih 2015) or on violence (Parkinson and Wood 2015) have compellingly made the case that DA-RT would make such work, already difficult and dangerous, less safe for those with whom one might engage, interview, and garner data. While IRB processes establish certain groups as “vulnerable” or “marginalized,” a recent QTD report on the subject argues that the range of groups that may fit into this category is surely broader, more fluid, and should be understood as context-driven (Lake, Majic, and Maxwell 2019). The need to comply with DA-RT guidelines for publication would in some instances mean that, ethically, it may not be advisable for researchers to undertake studies of vulnerable communities.

In particular, I assert that such likely harms and ethical conflicts would hold within far less dramatic and immediate life-and-death circumstances, such as those that exist in marginalized communities in the United States, especially when we take on questions of significant importance and relevance—what might be deemed true public controversies—such as immigration, climate change, or voting rights. Through such work, we necessarily engage people and organizations that are living amidst adverse circumstances and usually lacking traditional sources of power. Those of us who study public policy and work in community contexts understand the history and legacy of academic researchers’ engagement in communities of color and poor communities, which far too often has been brief, instrumental, and of value primarily to one side of the exchange—the researcher. For these reasons, those of us who do this research must work (and it is work) but also employ our personal engagement and individual qualities over a period of months and sometimes years to develop a rapport and trust with people and organizations within often understandably skeptical and cautious communities (Majic’s piece in this symposium addresses a similar point). In some cases, the community members and

organizations are even engaged in cooperative research efforts with us. We get to know people and organizations personally (and vice versa), as we learn their stories or study the experience of systemic processes, all the while establishing our genuine respect for and reciprocation of the time and access that they share with us. In short, we may be initially viewed warily as representatives of our profession, but it is with us as individuals that trust is developed. Not our discipline, nor our colleagues. And it is appropriate that this is so. An inherent part of this work is safeguarding the “data” that result. I feel strongly that part of my responsibility as a researcher—who has the attendant privilege that comes with my degree, institutional standing, and expertise—is that I must remain the caretaker of the research, the stories and experiences and institutional insights that are shared and garnered, as I am the one to whom others have entrusted this responsibility. As noted in the recent QTD Working Group Report on Research Ethics and Human Subjects, a researcher’s responsibility persists “from a project’s very beginning and throughout data collection and analysis, writing, and publication stages” (MacLean et al. 2019, 15). For me, this means not turning those materials over to someone else—an editor, a database, or something else over which I have no continuous control. The constrained notion of the public advanced by proponents of DA-RT does not seem to carry with it sufficient recognition or concern about DA-RT’s impact on the public that I and so many others engage through our research. It is likely for this reason that the urging to see DA-RT as a means to expand the reach of these marginalized voices rings somewhat hollow.

Without a belief in these potential benefits, it is understandable that so many feel DA-RT asks us to abdicate the caretaker role we hold dear without adequate reasoning. The kind of data we produce needs nuance and context, so erring toward openness in data access is likely to be an error with consequences. “Data” without context are more likely to be misinterpreted, and this is likely to be to the detriment of the people and places we study. Despite assurances of the ability to anonymize or veil transcripts and records to ensure the confidentiality that we promised to those we study, the level of contextual detail is difficult to modify in such a way that retains utility while simultaneously assuring that the information cannot be linked back to an individual or community. Even with one’s best efforts (especially for

vulnerable groups like the poor or undocumented), the risks clearly outweigh the benefits. This is especially so when both past and present remind us that communities are far too often stereotyped and scapegoated through oversimplified argumentation, and that facts and anecdotes can be manipulated without adequate mooring to context. From shaping the public’s sense of causation and accountability to jeopardizing government oversight or funding, the risks are real and could severely impact public health and public safety in already overburdened communities.

### **The Effects of DA-RT**

DA-RT will create barriers to achieving the public good because it will make it more difficult to engage with the public. The time, trust, and rapport needed to conduct certain types of research are themselves sometimes a barrier, but the possibility that one would be unable to publish without putting others in jeopardy is likely to be the straw that breaks the camel’s back. While exemptions, work-arounds, and customization to different research approaches have been promised, the originating logic and motivation that imposed a narrow definition of disciplinary interests around the public interest suggest otherwise. The lack of specification about research transparency in contrast to the greater emphasis on data access suggest otherwise. What we know about how established defaults can guide individual decision-making also suggest otherwise,<sup>3</sup> and this is especially likely to be the case among junior members of our discipline whose professional security and advancement requires conformity to prevailing publishing norms.<sup>4</sup> The history of our discipline in regard to addressing inequalities and its yet incomplete aspirations to endorse a shared commitment to the public good suggest otherwise.<sup>5</sup>

Expertise cannot substitute for a democratically engaged citizenry (Schneider and Ingram 1997), and contributing to the public good is surely a weighty task. Perhaps political scientists can simultaneously aspire to contribute while also employing more humility about how and to what extent our research and engagement can potentially have an impact. A first step might include prioritizing caretaking of the information the public entrusts to researchers and how this information is used. Doing all we can to avoid harm to the public—especially those who entrust us with examining the details of their stories and problems—could potentially be something

3 See Thaler and Sunstein (2008) for a discussion of this research trajectory.

4 See Lake, Majic, and Maxwell 2019 for a more extended discussion and existing signs of the impact of the DA-RT debate.

5 On political science’s engagement with race and racial inequality, see e.g., Smith 2004.

upon which agreement can be built. Perhaps the critical debate around DA-RT and the mirror it holds up to our disciplinary priorities might serve as an impetus to further advance longstanding discussions about our discipline and the public good.<sup>6</sup>

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<sup>6</sup> Here, see MacLean et al. 2019, which suggests, among other potential reforms and institutional changes, that political science as a discipline renew its attention to the ethics of research with human participants. In particular, it suggests redoubling efforts to incorporate matters of ethics into professional training, whether in graduate school or via association-led institutes. They also advocate for a much-needed update to APSA's overall ethics guidelines, a position with which I would concur, with particular attention to the critiques noted earlier in this piece.

# Every Reader a Peer Reviewer? DA-RT, Democracy, and Deskilling

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...the reason to believe a scientist's claim is not because he or she wears a lab coat [or has] a PhD...  
—Lupia and Elman (2014, 20)

...documenting every step in the research process of designing and executing a research project—in a format that *everyone* can access and understand—surely will lead to better research...  
—Büthe and Jacobs (2015, 61); emphasis added

Like manual labour in the past, intellectual labour now also begins to undergo a process of deskilling and precarisation.  
—Previtali and Fagiani (2015, 89)

Over the course of the unfolding of the DA-RT initiative and the debates that have ensued, one of the most curious silences has been an absence of explicit discussion of how DA-RT relates to the practice that has historically differentiated social scientific claims from other sorts: peer review. If DA-RT is intended to enforce quality standards, it is taking on a role that peer review has played for quite some time.<sup>1</sup> As Dvora Yanow and I asked in an examination of the origins of DA-RT (2016, 11):

[W]hat, precisely, is wrong with continuing to rely on peer review for policing epistemic-community standards? While the peer review process is not without problems or critics, when it functions well, it draws on [reviewers'] expertise. Informing this expertise are evaluative standards that are to some extent codified in methods texts,

but practitioners also draw on expert knowledge that is often known tacitly (Polanyi 1966; Flyvbjerg 2001; Yanow 2015, 277–85; cf. Yashar 2016).

A search of the founders' arguments for DA-RT (Lupia and Elman 2014; Elman and Kapiszewski 2014) as well as their rejoinder to critiques (Elman and Lupia 2016) produced no references to peer review.<sup>2</sup> Is DA-RT meant to substitute for peer review, improve peer review, or be a supplement to it? To date, the critical question concerning DA-RT's relationship to peer review remains unanswered.<sup>3</sup> Since there seems to be no associated effort to get rid of peer review, it could be that, implicitly, DA-RT is seen as a means of improving it. Alternatively, peer review may be seen as necessary (and improvable) but still insufficient. In this event, DA-RT is primarily meant to improve post-publication reading experiences and scholarly exchanges (including promoting efforts toward

1 Although some scholars have found evidence of such activity as early as the seventeenth century, the practice of peer review only became wide spread after WWII (Horbach and Halffman 2018, 2).

2 There has been some attention to peer review in political science. Referencing DA-RT in the APSA Guide to Professional Ethics (2012), Nyhan (2015a) offers three ideas for journal reform for quantitative studies. In note 7 he observes that his reform idea for "preaccepted articles" might be possible for "qualitative research," then noting that "format" is beyond his expertise (2015a, 82). Nyhan's (2015b) "checklist manifesto for peer review" does not even acknowledge the possibility of research outside a positivist, variables-based tradition, much less the inappropriateness of many items on the list to such studies.

3 The final reports of the elaborate participatory process designed by Jacobs and Büthe (2015), the Qualitative Transparency Deliberations, are now available. An examination of the 12 draft reports' treatment of "peer review" showed: five made no mention of it; six mentioned it either as sufficient—peer review is working fine—or as being harmed by DA-RT through various means, such as discouraging access to participants or archives. Only the draft document by Schwedler et al. gave guidelines related to peer review. There is a logical connection between DA-RT and peer review but, as the draft reports made clear, peer review had not been a focus initially, which is consistent with the founders' failure to explicitly discuss that connection.

reproducibility and replication<sup>4</sup>). Let's review these two possibilities.

### Improving Peer Review?

DA-RT might be understood primarily as a means of improving peer review, of making sure that peer reviewers have still *more* access to the data used in the article and *more* information than is now the case about how the data was generated and how it figures into the author's argument/findings. As expressed in the revisions to the APSA *Guide to Professional Ethics* (2012, 10, emphasis added):

6.2 Production Transparency: Researchers providing access to data they themselves generated or collected, should offer a *full* account of the procedures used to collect or generate the data.

6.3 Analytic Transparency: Researchers making evidence-based knowledge claims should provide a *full* account of how they draw their analytic conclusions from the data, i.e., clearly explicate the links connecting data to conclusions.

These changes to the *Ethics* guide imply that something about existing scholarly practice needed fixing: the typical methods statement in a research article or methodological appendix in a book was seen as wanting, as not “a full account,” but as incomplete. The systematic evidence that standard practices have been problematic has never been offered; and, indeed, the notion that researchers had to be admonished in the *Ethics* guide to do these things seems strangely ahistorical. Since WWII, scholars have anticipated peer review, conducting research *knowing* that their particular knowledge claims would be assessed by reviewers prior to publication. As important, editors and reviewers have had the authority and power to call on authors to provide

more information on their evidence, how it was obtained, and how they used it (Isaac 2015). How, exactly, is DA-RT an improvement on these long-standing practices?

Perhaps “the problem” is that scholars have changed? In reading DA-RT proponents' arguments and listening to them at conferences, I heard one implicit theme sounded, that scholars' motivations are suspect, that they—we—need to be “incentivized” to conduct ourselves appropriately.<sup>5</sup> Because the pressures to publish have become so intense, we are not to be trusted, but require more explicit guidance than that offered by the existing peer review system. Hence the change to APSA's *Ethics Guide*, and the subsequent, successful effort to coordinate agreement among editors to explicitly incorporate DA-RT into their published guidelines.<sup>6</sup> Even if this aspersion cast on scholarly character were to be given credence, it is not clear that DA-RT is a reasonable means of improving researchers' ethical conduct and thereby the general trustworthiness of research.

I take specific issue with Büthe and Jacobs' statement in the second epigraph that “documenting every step in the research process of designing and executing a research project...surely will lead to better research” (2015, 61). First, why single out documentation of “every step” as opposed to the myriad other things that could make us better researchers: What about imagination and creativity? What about the relationship of our research questions to contemporary problems? What about improving our interviewing skills or writing ability? Second, the focus on giving a “full” or “complete” account seems to imagine the impossible: that every “step,” every inspiration, *can be known* explicitly and laid out verbatim. Büthe and Jacobs (2015, 61) themselves recognize that “publishing an article based on a case study should not require a supporting manuscript several times the length of the article itself.” Yet their admonishment to provide a “full”

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4 Reproducibility rather than replication seems to be the actual focus of DA-RT. Reproducibility in quantitative research means that a quantitative data set, meta-data, software, and associated commands are provided to others so that they can apply the statistical techniques used by the author to check whether the results turn out the same. In contrast, a bona fide replication begins at the data generation stage, running an experiment in a new lab, repeating survey questions among the same population, or trying to achieve cold fusion (Browne 1989) using the processes claimed by the researcher. The conflation of reproducibility with replication elides ontological assumptions about the stability of the phenomenon under study. The physics of matter is law-like making replication feasible, assuming the new knowledge is valid. In contrast, human activity changes by generation and with historical events including shifting cultural underpinnings. For such reasons ontological stability should be actively theorized. King (1995) is, in part, responsible for this original conflation, failing to discuss ontological assumptions essential to conceptualizing the appropriateness of replication to the social sciences.

5 Whenever I hear “incentivized,” I must admit that it makes me cringe as it is the language not of the academy but of “New Public Management” (NPM). That “innovation” in governance practices imagines that workers are not intrinsically motivated, not agentic, but, instead, *homo economicus*, responding to “incentives” dangled before them by oh-so-wise managers (deans and presidents intent on improving the national rankings of their departments and institutions). NPM is part of the larger trend toward the corporatization of the university (see, e.g., Strathern 2008, Broucker and De Wit 2015).

6 See the Workshop on Data Access and Research Transparency (DA-RT) in Political Science, 2014.

account—complete, comprehensive, all-encompassing and, my favorite synonym, exhaustive, as the effort to do so will, in fact, be exhausting—reminds me of the famous Jorge Luis Borges story in which the science of cartography becomes so exact that cartographers “struck a Map of the Empire whose size was that of the Empire, and which coincided point for point with it” (Borges, 1998, 325). As the fable recounts, succeeding generations judged the map worthless. Will scholars in twenty years’ time look back at the tumultuous Obama-Trump years and ask, “*This* was the preoccupation of the political science discipline?”

Returning to peer review, if we think about its purposes, it accomplishes two very specific things: it gives tailored advice to authors about what they must do to achieve publication in one particular journal for their particular projects, and it signals that readers (of any stripe) should have confidence in the article because relevant experts assessed the research and found it both worthy of publication and as offering trustworthy knowledge claims. On the former, the general admonitions of DA-RT hardly seem an improvement over the project-centered assessments from peer reviewers. To the latter purpose of signals to readers, I now turn.

### **Improving Readers’ Experience / Empowering Readers?**

Perhaps DA-RT is aimed at improving the post-publication experience of readers.<sup>7</sup> The implication seems to be that readers themselves want the opportunity to do their own review of the evidentiary basis of what they’re reading. And, indeed, DA-RT takes for granted that post-publication readers will be accessing the author’s evidence either through “trusted digital repositories” and/or directly via “active citation,” a technological innovation made possible by the internet which provides links to particular bits of the evidence base that support particular parts of an author’s argument. The ideal is that the author will also annotate these evidentiary sources, so that the reader can directly assess the author’s logic for why that particular evidence supports that particular part of the argument (see Figure 1, in Moravcsik 2014, 51). Of course, to some extent the longstanding use of footnotes accomplishes some of what is envisioned, but

now, this will be facilitated by instantaneous access to texts (assuming functioning links).

Who are these imagined readers? Lupia and Elman characterize the audiences for DA-RT as fourfold (2014, 22): members of a particular research community, scholars outside that immediate community, those who want to use the research as a basis for action (their example is teachers), and “public and private sector decision makers.” For each of these groups, it is the *possibility* of looking at the evidence *for themselves* that is important, in contrast to, as Lupia and Elman put it (2014, 22), knowledge “claims whose origins are... hidden.” In other words, for these four audiences, expert peer review prior to publication is insufficient. Moreover, an associated implication is that all of these audience members are somehow as or more qualified than the peer reviewers or the author him- or herself to assess the evidence!

Andrew Moravcsik is the most enthusiastic promoter of active citation, arguing that it will give scholars “greater incentives to improve their qualitative methodological skills” (2012, 35). But, more than that, in his view: “While active citation encourages more careful research, it will also empower critics. By revealing evidence at a single click, active citation will democratize the field, letting new and critical voices be heard” (2012, 36). Moravcsik goes so far as to envision a new future:

In all these ways, active citation can be understood as a way of transforming traditional hierarchies of control and publication into an open, virtual network, in which new and plural streams of evidence and interpretation can emerge—while still imposing discursive rules that require some substantial commitment from serious participants in the scholarly debate, and permit others to voice their objections (2012, 36-7).

This future is desirable because, in his view, peer review does not function well. Ironically, without citation to any systematic studies, he states that:

It is common—yet almost never remarked by referees or reviewers—that citations lack page numbers, secondary materials are cherry-picked from historical debates, journalistic conjectures are cited to establish causality, primary sources are taken out of context, or important empirical points rest on the interpretations of generation-old historians whose work has been overturned

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7 I’m setting aside reproducibility and replication as much ink has been devoted to that and relatively less to “readers.” But see note 4 on the conflation of two these concepts.

by subsequent scholarship (Moravcsik 2010, 34).

Notwithstanding a loosened link between active citation and DA-RT,<sup>8</sup> Moravcsik's argument merits further examination—not only because he offers a critique of peer review but, also, because his is a clear explication of what transparency is meant to accomplish. His 2012 article references his detailed assessment of six criticisms of active citation (2010, 33-34), and he then concludes, “For the most part, I find them without much substance—in part because other disciplines have adopted similar practices without ill consequences” (2012, 36). The disciplines that he takes as models are history<sup>9</sup> and legal scholarship. Lacking in-depth familiarity with either of these, I cannot confidently assess his rebuttal. But I am wary of his evoking democratization as a rationale for active citation, and I am troubled by his additional comment that “one can imagine some preferring that scholarly debates remain restricted to a small number of insiders, as they are today, even with the resulting costs in scholarly quality and the anti-democratic hierarchy” (2012, 36). Who are these “insiders” who have the power to restrict debate? Are these the peer reviewers chosen by editors? As important, is democratization, as he sees it, an improvement over the existing peer review system?

Let's take a hypothetical case. Imagine that a reader clicks on a link that takes her to a legal text in Spanish from the Colombian parliament. To make sense of that bit of evidence, she needs the ability to read Spanish but also to make sense of legal discourse in that context. Assuming she has those skills, she will still, in most cases, lack the in-depth knowledge of the specific information the researcher has drawn on to make sense of this particular bit of information as it plays a role in the analysis of the entire body of evidence. Should she trust her own abilities or should she trust in the peer

review process, which has vetted the author's research? Put another way, Moravcsik and DA-RT seem to imply that the researchers' (and peer reviewers') expertise is not to be trusted, whereas it assumes that any reader from any of these four audiences has the ability to make sense of such evidence without having the in-depth education of a disciplinary Ph.D. or the situational knowledge of the research setting, much less the time to devote to the topic at hand.<sup>10</sup>

Of course, it is members of Lupia and Elman's (2014) first two communities who have more of what it takes to assess this evidence, i.e., the disciplinary Ph.D., familiarity with scholarly discourse and norms of argumentation, and perhaps the requisite language skills and familiarity with legal jargon and modes of analysis. Even these two audiences, however, will lack what the author can claim: in-depth knowledge of the case and its socio-political setting, gained through time spent generating and analyzing the evidence (as Renee Cramer explicates in her contribution to this symposium). As has always been the case pre-DA-RT, the onus is on the author to make the case to peer reviewers that her knowledge claims are trustworthy; and, again, peer reviewers have or can request access to evidence they deem missing. That said, ultimately, peer reviewers must trust researchers to some extent—that researchers have acted ethically (i.e., not fabricating data) and have reported their modes of analysis in good faith. If in doubt, they can challenge the manuscript.

### **Turtles all the Way Down: Trust in Scholars**

Part of what seems to motivate DA-RT is decreasing trust in researchers. They are expected under DA-RT to somehow lay bare every decision and every insight for inspection—almost seeming to imply that, from this

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8 Understanding how active citation relates to DA-RT requires examination of two versions of the statement endorsed by those editors who committed to implementing it at their journals. The first document is “Data Access and Research Transparency (DA-RT): A Joint Statement by Political Science Journal Editors” available at [http://media.wix.com/ugd/fa8393\\_da017d3fed824cf587932534c860ea25.pdf](http://media.wix.com/ugd/fa8393_da017d3fed824cf587932534c860ea25.pdf) (accessed February 18, 2019) from a workshop held at the University of Michigan. That active citation was a part of the discussion at the workshop is clear on its first page, which states: “The materials that an author might provide to show her analytic procedures, for example, could include program code, pre-analysis plans, activated citations, and so on.” The sentence ends with an endnote to Moravcsik's 2010 article on active citation, published in a *PS* symposium that was one of the first efforts of those promoting DA-RT, including the 2014 article by Colin Elman and Diana Kapiszewski. The second version of the statement, what is now being called “JETS,” for the Journal Editors' Transparency Statement, is available at <https://www.dartstatement.org/2014-journal-editors-statement-jets> (accessed February 18, 2019). This version no longer includes the reference to the location of the Workshop at the University of Michigan, the sponsorship by APSA, nor the four-paragraph background section that included the quotation above. The removal of that section would seem to imply the downplaying of active citation as part of the DA-RT program, although the requirement remains that authors clearly delineate “the analytical procedures upon which their published claims rely, and where possible ... provide access to all relevant analytical materials.”

9 Moravcsik's own use of historical sources has been challenged forcefully by Lieshout, Segers, and van der Vleuten (2004).

10 Readers of a certain age may be able to recall the naïve enthusiasm with which some initially greeted the internet—as if it would usher a new period of peace and understanding. Instead, the internet has produced rumor mongering that has, at times, produced physical violence, e.g., in Pizzagate (Fisher et al. 2016). What unintended consequences might the democratization of peer review produce?

information, the reader should be able to (re)conduct the research! But who among one's readers will do so? And why would we expect them to? In contrast, what peer review ideally produces (though not inevitably) is a peer, or a set of them, capable of reviewing a specific manuscript. When the process works as intended, he or she can be expected to read a manuscript at least once with care, perhaps going back to particular parts or checking his or her own comprehension in the preparation of the review. This assumes that the peer reviewer has been selected by editors based on theoretical and methodological expertise and knowledge of the general and specific existing literatures engaged by the researcher—that is the ideal and the norm that most editors strive for.

At its logical extreme, echoing the third epigraph of Previtali and Fagiani above, DA-RT implies a “deskilling” of the researcher who, after all, has invested years in obtaining the PhD. As Lupia and Elman claim in the opening epigraph, the reason to believe a scientist's claim is not because she has a PhD. Instead, it is her ability to fully reveal all of what she has done to accomplish the

research which will somehow render her both ethical and open to all readers. Under DA-RT, her years of investment in the PhD and her specific project become erased, her embodied effort denigrated by an absurd notion that any reader has the skill to make sense of her evidence. This web-enabled “democratization” may sound tempting: “Why shouldn't anyone be able to examine the evidence for themselves?” Yet there is a clear tension between that impulse and the roles many academics fashion for themselves as experts in their research areas. Whither expertise in an “open, virtual network?” It's a question DA-RT proponents should be asking.

### Acknowledgements

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# Data, Transparency, and Political Theory

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In the various conversations, deliberations, postings, panels, and forums in which I have participated, I confess that I often have felt like Admiral James Stockdale, who ran for Vice President on Ross Perot's independent ticket in 1992. In the vice-presidential debate with Al Gore and Dan Quayle, he began his opening remarks with the questions: "Who am I? And why am I here?" Less metaphorically, the question I must confront is: What is a political theorist doing in these deliberations over Data Access and Research Transparency (DA-RT)? What does DA-RT have to do with political theory?

We may recall that Admiral Stockdale's question, profound in its existential implications, and elegant in its practical simplicity, was responded to with laughter and turned into a joke, as Stockdale was read as a nice, but confused, old man. He was considered irrelevant to the election. Political theory, similarly, has been sidelined, dismissed, as irrelevant to this discussion, indeed, perhaps to the entire discipline, because we don't deal with "data." We may be nice colleagues, and some colleagues may begrudgingly acknowledge that perhaps political theory needs to be taught to our students, but we are often not seen as serious participants in the conversation called political science, and that has extended to the DA-RT debates.

Yet just as Stockdale was an accomplished military man with a stellar record of public service—so relevant to politics now more than ever—so is political theory central to the discipline of political science. This essay is in part a call to my fellow theorists, and to fellow travelers in other subfields, that they, too, need to be concerned about political theory's place in the conversation. But I also want to raise some larger issues about DA-RT that I hope speak to the entire discipline.

I grant that many political theorists themselves—indeed, perhaps the vast majority of us—seem to believe that DA-RT is completely irrelevant to our field. After all, though some of us use data in our arguments, the vast majority do not. Many of the journals that we prefer to publish in, such as *Political Theory*, *Constellations*, *Political Research Quarterly*, and *The International Journal of Political Theory*, have not signed on to DA-RT either. So what is there to talk about?

Accordingly, I am a reluctant participant in this debate. Like other theorists, I find it tempting to see the stress

on data as irrelevant to my work. Transparency may be a problem for quantitative political scientists, because their arguments depend on data that are claimed to constitute "facts" which are offered as "proof." But as Sandra Harding argued over thirty years ago in her feminist critique of science (Harding 1986), the potential bias and interpretative framework of the researcher are often hidden behind the data, as if controlling for variables and methodological rigor distances the investigator from those interpretations. By contrast, the goal in writing a political theory paper is to display to the reader how we achieved the interpretation that we are advocating. Political theory is about making an argument, persuading readers, and generally doing so through arguments made in the paper itself and by quoting political theory texts, whether primary (e.g. Rousseau's *Discourse on the Origins of Inequality*) or secondary (e.g. Judith Shklar's *Men and Citizens: A Study of Rousseau's Social Theory*). The entire text of the article displays the workings of our minds as we do the work, and the argument will succeed or fail based on our ability to persuade the reader of the validity of our thinking. As for data access, the texts that we read are available to anyone with an Amazon account or a library card. If we use obscure archival material, much of that is increasingly available online.

Living in a democracy, we tend to think of transparency as the highest good. It is the primary safeguard against corruption and abuse of power. But the stress on transparency as an unalloyed good in political science writ large is possible only from the perspective of a particular subset. In particular, the myriad issues and dangers posed by human subjects and vulnerable populations, which many others have identified, have been inadequately addressed by the journals that have signed on to DA-RT. That is a serious concern for the future of the profession, and other contributors to this forum have outlined the grave implications of this problem for other subfields in the discipline better than I can. But theorists concerned with ideals of privacy, trust, and justice cannot turn away from these issues. It is our job, in many ways, to be the ethical watchdogs of the profession. The insufficient attention to human subjects issues on the part of the JETS statement that many journals have adopted smacks of an effort to push all

qualitative researchers—not just theorists—out of the field by creating inappropriate standards, and by framing the conversation in a way that forces us to “just say no” rather than to engage in a real conversation about how political science can be improved across the board.

The recent Qualitative Transparency Deliberation (QTD) effort led by Tim Büthe and Alan Jacobs of the Qualitative and Multi-Methods Research (QMMR) section notably stressed the importance of protecting human subjects, and raised many important issues pertaining to qualitative research of different kinds. Political theory was a very minor part of the debate, perhaps for the same reasons I have mentioned concerning the lack of participatory interest on the part of many theorists. If we don't participate, our issues won't be discussed.<sup>1</sup> Significantly, the QTD process did not yield uniform rules or standards across subfields, as noted in a post to *PS Now* by then-APSA President Kathleen Thelen and then-President-Elect Rogers Smith (Thelen and Smith 2018). That this post appeared the same day that the APSA announced that it “has joined with Cambridge University Press, the Qualitative Data Repository at Syracuse University (QDR) and the software non-profit Hypothesis in developing and demonstrating a new approach to transparency in qualitative and multi-method research” should, however, raise concerns (American Political Science Association 2018). It seems to reinforce the idea that many have accepted DA-RT as a given for the discipline of political science, giving way to earnest discussions of *how* to make it work for the different subfields. This point, also raised several years ago by Jeff Isaac (2015) in his excellent editorial essay in *Perspectives on Politics*, obscures the broader conversation that has been going on for the past several years but seems to be ignored by those determining the standards for journal publication.

This last concern taps into my larger worry, which is ultimately what keeps me from simply dropping out of this issue altogether: namely, that the DA-RT effort signals a further shift in the discipline to further privilege already-privileged quantitative work, after which theory will be pushed further and further to the margins of the field. Yes, we theorists are already marginalized, but we could be eliminated altogether. This has happened at some universities already, Pennsylvania State University being the most notorious, with a much publicized (though

ultimately futile) mobilization effort by Foundations of Political Theory to protest the discontinuation of political theory as a graduate field<sup>2</sup>. Other universities—even if we cannot mention them in print without further hurting the theorists who remain at these institutions—have converted the lines of retiring political theorists to lines in other subfields, effectively gutting, or at least severely reducing, the political theory subfield in their departments. It is difficult not to believe that many political scientists would be happy to achieve this result at many other research universities.

In other words, a strategic question is involved here concerning the future place of political theory in the discipline of political science. It is tempting to say “to heck with political science; we will publish in other journals,” and many political theorists, as well as qualitative comparativists, have decided to do just that. But that is a dangerous strategy, as political theory positions continue to shrink throughout the United States and abroad. Withdrawing from this battle simply concedes more and more ground until there is too little left. Publication in non-political science journals reinforces the prejudice that we are not part of the discipline, further reducing the numbers of faculty lines. The reduction of future faculty positions is then offered as a reason to accept fewer graduate students in political theory. Without students to take our courses, we become even less important to the discipline, thereby justifying a further reduction. Pretty soon, we could simply disappear.

What to do? One might think that a reasonable response could be for us to take up the notion of active citation that Andrew Moravcsik has proposed.<sup>3</sup> As Peregrine Schwartz-Shea notes in this symposium, active citation was part of the original discussions over implementing DA-RT principles but is no longer part of the JETS statement (this issue, page number).<sup>4</sup> Would this be a way for political theory to stay “relevant” to the field? It could certainly be a useful tool for posting archival data already online. One might also think that since theorists often deal with texts that are historical in nature, and since many canonical texts are in the public domain and on the web, it would be very easy to provide internet links to these texts to provide fuller citation of passages drawn on in our arguments. But this is not as simple as it seems. Many online versions do not provide definitive or in some cases accurate renditions of texts,

1 The reports from the various working groups participating in these deliberations can be found here: <https://www.qualtd.net/>

2 Penn State no longer lists political theory as an “area of study” for graduate studies. <https://polisci.la.psu.edu/graduate/programs-of-study> accessed October 5, 2019.

3 See e.g., Moravcsik 2010.

4 Available at: <http://www.dartstatement.org/2014-journal-editors-statement-jets>

even those originally written in English: punctuation, italicization, capitalization, at times even the spelling of words in their twentieth-century incarnations rather than their original spelling present inaccuracies. Phrases and even entire sentences can differ from edition to edition. The differences among translations in different editions of texts originally in Greek, French, Italian, German, Arabic and so forth are significant, and different authors rely on different translations (including their own). When particular editions of various texts come to be accepted as “authoritative,” as often is the case, they are almost always copyright protected and not online. A policy of active citation would then make it even more difficult than it already is for political theory to appear in the discipline-wide journals, and may have a chilling effect on submissions.

And anyhow, why go to all these lengths when footnotes have accomplished these goals in the past? A major reason is that political science journals have increasingly moved to “scientific notation” (in-text reference to author, year, and page number) as the preferred citation methods. Endnotes are increasingly discouraged; if it is important enough to mention, the thinking goes, then it is important enough to put in the text, “up or out.” Given decreasing limits on article length, that rule usually results in “out.” Even journals that permit endnotes with substantive content seek to limit them in length and number. This restriction has had a significant negative effect on the political theory field, curtailing our capacity to present our arguments in depth in the text and in further depth in the footnotes. For readers who are specialists in the field or for another reason want to have that greater depth easily accessible on the page itself, open to quick scanning for possible relevance, footnotes have long served a useful function.<sup>5</sup> Gerring and Cojocaru note that footnotes serve the interests and values of “academic honesty,” evaluation of the originality or derivativeness of an argument, as well as knowledge production, noting that “the intellectual history of a subject is located in the citations” (Gerring and Cojocaru 2016, 8). So resistance to active citation and a move to bring back what are now being called “meaty footnotes” could be valuable in changing the conversation about what we are up to in performing academic research and publishing in journals: that is,

how we seek to communicate with the wider academic audience, our readers. So why not just “bring back the footnote”?

“Scientific notation,” ironically, hurts the discipline as a whole. It makes it easier to permit relaxing the demands of specific citation with page numbers. This, some have complained, permits laxity: “Rawls 1971” as a citation could cover a lot of territory, and makes it difficult to trace the author’s line of reasoning. If you summarize an entire article or book, it is often appropriate not to provide a page number, but the increasing frequency of the no-page-number option, some have complained, may have permitted us to draw on it too often.

Political theory certainly did not generate that problem. Rather, it is a problem that has been created *for* political theory by the same mind-set that lies behind DA-RT: viz. the idea that political science is a “science” and must strive increasingly to emulate the methods and look of the “hard” sciences. And yet, notice that in *scientific* journals, only author and year are cited unless a direct quote is involved. This mind-set produces a double-bind for all subfields that are not strictly quantitative: in being forced to adapt to a method of citation that is not appropriate to our subfield (in-text references with virtually no explanatory footnotes), we end up adopting a practice that in turn creates the foundation for us to be criticized as inferior scholars and not “real” political scientists.

One can always get around these problems by considering political theory as an “exception” to the “norms” of political science as defined by the principles of DA-RT, which are designed primarily with quantitative data in mind and are being bent and twisted to apply to other forms of data. But I would argue against that approach. Rather, following the QTD report, editors have a responsibility to recognize and acknowledge, *publicly*, that they understand that political science research has many different sorts of norms that are appropriate to their respective subfields, and that an author’s work should be evaluated by and in terms of those respective norms by individuals in those subfields. Political theorists need to engage with the discipline’s journals that publish across the subfields to remind everyone that we are and should be part of the conversation. The future of the field is not something that we should be leaving to others to determine for us.

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5 I have made some of these comments in the QMMR blog posts over Qualitative Transparency Deliberations <https://www.qualtd.net/viewtopic.php?t=101>. Thanks to Marcus Kreuzer for reminding us of Anthony Grafton’s work on this topic (1997). See also Gerring and Cojocaru (2016), who note that in the interest of reducing overall article length, citations are often the first line of attack, and that decisions over which citations to cut often reflect biases of gender, seniority, and field prominence (7).

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# Debating the Value of DA-RT for Qualitative Research

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## Unexplored Advantages of DA-RT for Qualitative Research

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### Introduction

Discussion regarding the introduction and expansion of data access and research transparency (DA-RT) standards in political science has aroused a lively debate (e.g. Büthe et al. 2015). Scholars of various methodological orientations—qualitative researchers, theorists and even some experimentalists—have raised several concerns about the desirability or difficulties of implementing these standards (Fujii 2016; Isaac 2015; Pachirat 2015). Yet, the argument for making qualitative research more accessible and transparent has already been presented in several excellent pieces (see, e.g., Büthe et al. 2015; Büthe and Jacobs 2015; Elman, Kapiszewski, and Vinuela 2010; Elman and Kapiszewski 2014; Gleditsch and Kern 2016; Lupia and Elman 2014; Moravcsik 2014). We have also supported the introduction of these standards, extending the logic of preregistration to qualitative analysis (Piñeiro and Rosenblatt 2016, Piñeiro, Pérez, and Rosenblatt 2016). In this brief note, we add to the literature cited above by highlighting a different perspective on the assessment of the introduction and expansion of DA-RT practices in the discipline, especially in qualitative research with a focus on preregistration. It is important at the outset to stress that our claims are only valid for—and can thus be only applied to—positivist qualitative

research, i.e. research that seeks to make descriptive and causal claims regarding a research problem. The interpretivist tradition in political science and other traditions in the social sciences and humanities follow other epistemological rules (Sil, Castro, and Calasanti 2016). Our discussion is not meant to suggest that one tradition is superior to others.

We have conducted various studies in which we have preregistered the research designs and analysis and we have also made efforts to facilitate data access and replication to the greatest possible degree, what Büthe and Jacobs (2015) dubbed as “replication-in-thought” (57). We have participated in several studies that combined natural experiment research designs with field and survey experiments, using administrative, survey, and interview data. We will describe our experience studying the reproduction of activism in Uruguay’s Broad Front party to illustrate the role of DA-RT in improving how we design, conduct and analyze our research. Ours was an in-depth case study undertaken to test descriptive and explanatory hypotheses about the origin of the Broad Front as a mass-organic leftist party and the reproduction of its activism. We used different techniques (process tracing and survey experiment) and different data collection tools (in-depth interviews, archival research from press and party documents, and survey research). In this note, we will first argue that preregistration helped us in the design of our experimental and qualitative research. While one of the main arguments in favor of DA-RT is that it makes analysis more transparent, we will argue that it also improves qualitative theory building. Second, we will describe how preregistration helped us plan our fieldwork and how it allowed us to avoid potential setbacks in the field. Third, we will describe how preregistration guided us in our assessment of what evidence we needed to test our descriptive and causal

hypotheses and, especially, how to avoid confirmatory bias in the selection of evidence.

## Improving Design

One of the greatest challenges in conducting qualitative research is to develop a systematic and parsimonious theory and, more critically, to develop a clear-cut connection between theory and working hypotheses. In our experience, preregistration helped us build a more systematic theory and a clearer relationship between theory building and hypothesis-generation from the very early stages of our research.<sup>1</sup>

Preregistration enabled us to prepare a good set of hypotheses. In a previous paper (c.f. Piñeiro and Rosenblatt 2016) we stated that: “The PAP-Q [pre-analysis plan-qualitative] is premised on the idea that a great proportion of qualitative research work (and much of its virtue) lies in its inductive character. Nonetheless, this inductive nature does not preclude the development of theoretical claims, and does not entail that everything be learned or done in the field... The PAP-Q seeks to establish a formal beginning to the iterative alternation between empirical work and theory...” (788). Our commitment to preregister the design for our analysis of the reproduction of activism in Uruguay’s Broad Front generated a clear milestone that motivated us to develop a clear body of hypotheses. While proceeding in this manner may have delayed the beginning of our fieldwork (see below), it forced us to think thoroughly and rigorously about our theory and our working hypotheses.

Preregistration also helps one select and explicate the analytical strategies one intends to use and the type of evidence necessary to test one’s working hypotheses. This reduces the temptation to—consciously or unconsciously—omit some information or to avoid seeking certain kinds of information or evidence. Preregistration, however, does not preclude the possibility that, for example, during fieldwork new evidence might come to light and help one test the hypotheses or force one to reformulate the original working hypotheses. All research is a process and qualitative research is an iterative process alternating between theory and evidence (Elman and Lupia 2016; Mahoney and Rueschemeyer 2003). The crucial issue here is that, with preregistration, the researcher improves the transparency of this iterative

process. Once the researcher has committed to seek a given type of evidence, he or she must present the results of that search and analyze the data in a manner consistent with the previously formulated plan. Conducting this systematic process after preregistration facilitates research transparency, reduces the researcher’s moral hazard, and allows one to assess the evidence included and discarded in the course of the research.

Clearly establishing the role of each piece of evidence (causal process observation, CPO), how the data will be analyzed and weighted (in the case of process tracing), and what sources will be used to obtain such evidence helps the researcher avoid an ad hoc analysis that is tied to the evidence collected. For example, Bennett and Checkel (2015) present a list of good practices for process tracing. Preregistration helps promote good practices, such as determining the relevance of the evidence collected in light of the potential biases of evidentiary sources, and deciding when to stop the data collection and analysis. This, in turn, helps one to develop clear codification rules *ex ante* that will facilitate replication of the analysis; for example, the researcher identifies in advance the outcomes he or she expects to obtain (or not) from in-depth interviews with different interviewees (e.g. what type of answers the researcher expects from a given set of questions). When combined with data access, this practice improves transparency in qualitative research. For example, it helps one to check whether all relevant questions were included for each interviewee.

## Improving Fieldwork

As every methods textbook suggests, a good design leads to good fieldwork. Preregistration, a key trait of DA-RT, is an important tool for improving the quality of our research designs. Thus, a good pre-analysis plan improves the quality and the efficiency of our fieldwork. This is even more crucial for qualitative research, where fieldwork typically offers a one-shot opportunity and is time consuming. Prior to undertaking fieldwork, a qualitative researcher must develop a clear sense of the questions to ask and also identify the kinds of evidence they might need. Both steps help elucidate the data necessary to test specific hypotheses and therefore help ensure that the fieldwork undertaken is efficient. In the case of our analysis of the Broad Front in Uruguay,

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1 Preregistration, via a Pre-Analysis Plan (PAP) is: “...a document that formalizes and declares the design and analysis plan for your study. It is written before the analysis is conducted and is generally registered on a third-party website. The objectives of the PAP are to improve research design choices, increase research transparency, and allow other scholars to replicate your analysis.” (Source: EGAP, available at <http://egap.org/methods-guides/10-things-pre-analysis-plans>, last accessed September 26, 2019). While this is a relatively common practice in experimental research, it is less frequent in observational studies (and even less so in qualitative research).

preregistration guided us to focus on the relevant evidence and sources, that is, to list the documents needed, the people we wanted to interview, and the kind of questions we needed to ask to test our hypotheses (Piñeiro, Pérez, and Rosenblatt 2016). For example, we identified the type of evidence needed to support the claim that the grassroots activist structure of the Broad Front never oligarchized; we planned to observe this through in-depth interviews with party leaders, review of party documents, online survey, observation of party activities, the party's administrative records, and review of press articles. We succeeded at collecting CPOs from these sources. Additionally, we reported what evidence we were not able to find or build from existing sources. For example, we were not able to calculate the exact turnover of grassroots' delegates to the FA national directorate (Plenario Nacional). By systematically reporting both the evidence found and the evidence not found, we were able to assess the CPOs as a whole and get a clear picture of the Broad Front. This also helps readers to assess the relevance of the evidence presented.

Pre-registration provides an opportunity to pause and forces the researcher to think thoroughly before executing the fieldwork. This does not mean that preregistered research designs are immune to error in design and implementation. However, it does encourage more thoughtful and conscious research. In his classic work, Sartori (1970) urged researchers to seek better conceptual and theoretical foundations for their work. Preregistration promotes this practice and prevents researchers from initiating projects without having a clear conceptualization and solid theoretical foundations. In the case of our Broad Front study, we preregistered the necessary and sufficient evidence to test our hypotheses. This helped us calibrate our in-depth interviews and the logic of archival research. Also, it guided our design of the survey questionnaire in terms of relevant outcomes.

Some scholars might claim that DA-RT in general, and preregistration in particular, is a straitjacket that precludes the possibility of being open to new findings and/or dampens creativity promoted by the induction process. Yet, in our experience, preregistration helped order the creative process of developing a systematic theory and planning fieldwork. It also guided our process of induction. We outlined the main nodes of the process we wanted to trace to describe and explain the production and reproduction of the Broad Front as a party with a grassroots structure where activists regularly engage with the party. For our analysis, we selected sources that we thought could—or, in the case of party

documents, should—signal such party structure. To reiterate, preregistration in qualitative research conceives of the possibility of updating one's theory (Piñeiro and Rosenblatt 2016); preregistration simply makes more transparent the iteration between theory and evidence. Also, if the researcher knows beforehand what to expect from his or her fieldwork, the researcher can be more open to unexpected results and can delve more deeply into these new findings.

One of the main challenges of qualitative research is to decide when to stop (Bennett and Checkel 2015). The conventional practice is to stop when the iteration between theory and evidence ceases to generate new insights, similar to the saturation criterion in in-depth interviews. However, there always remains doubt as to whether other pieces of evidence might change one's theory. A way to avoid this problem is to commit to search for a given piece of evidence as a function of one's expectation. Thus, beyond post-hoc saturation, there is a clear preestablished endpoint. In the event that the researcher fails to find sufficient evidence, he or she can easily report the reasons for not fulfilling the stated commitment and the scope of the evidence collected in relation to the preregistered expectation.

### **Improving Qualitative Analysis**

Qualitative analysis is complex and requires researchers to make a large number of decisions. These decisions are often obscured behind parsimonious theories, narrative, and analysis. Even in lengthier academic works, such as books, which are better suited for the presentation of qualitative research, authors rarely detail the process of data collection and data analysis. Therefore, replication or the simple analysis of each decision becomes impossible. It is difficult, therefore, to evaluate analytical mistakes and identify better approaches to the same research problem. Also, in the peer review process, while a referee may prefer to focus on the paper, access to a detailed preregistered analysis plan and a full appendix with all research materials improves the assessment of the research. The credibility of a research project is enhanced when there exists the possibility of replicating the findings (King 1995). More generally, it is critical for the cumulation of knowledge to explicate analytical decisions (Elman and Kapiszewski 2014; Elman and Lupia 2016; Lupia and Elman 2014).

In the case of qualitative analysis, explicitly answering questions—e.g. why was a given person interviewed? Why was a particular set of documents reviewed? What documents were left out? What is the bias of the

sources reviewed and what is the relevance of such bias for the study's conclusions? What are the limits of the sources?—is important for assessing the quality of the evidence and its analysis. Causal claims in qualitative research depend on two criteria: finding evidence to support the researcher's working hypotheses and finding evidence to reject rival hypotheses (Bennett and Checkel 2015; Zaks 2017). The former is the obvious role of empirical research: an investigation needs to collect evidence to support its theoretical claims, and it has to present such evidence following disciplinary standards (Elman and Kapiszewski 2014). The latter is critical for qualitative research, especially in cases where the researcher was not able to observe or collect conclusive evidence for the main hypotheses. In such cases, rejecting rival hypotheses helps in the construction of a plausible causal argument regarding a given process. How rival hypotheses are themselves rejected depends upon the transparency of the analysis. Preregistration forces the researcher to acknowledge alternative hypotheses early on, making the manipulation of rival claims much more difficult.<sup>2</sup>

## Conclusion

Research transparency is costly. Yet at stake is a shared standard of transparency, the value of which outweighs the costs (Elman and Lupia 2016). At stake is the reliability of work that is usually funded with public resources, and that produces a public good which might eventually guide public policy decisions. If social scientists wish greater respect from the larger science community, they must adapt and be willing to fully disclose the nature of their research process. Elman and Lupia (2016) state that: "The process-dependence of knowledge generation has a transparency corollary: if there are stable practices for properly conducting investigation and analysis, and if the legitimacy of a knowledge claim depends on those practices being followed, then the less you can see of the process, the less access you have to the context from which the knowledge claim has been derived. This corollary determines the nature of openness" (44). Fortunately, recently several scholars have built the tools and necessary infrastructure to improve data access and research transparency in qualitative social sciences (see

e.g., Elman and Kapiszewski 2018; Moravcsik 2010, 2014).

Comparativists, interpretativists, and experimentalists tend to raise different concerns about the implementation of DA-RT standards (Fujii 2016; Htun 2016; Pachirat 2015). For example, some emphasize the need to prioritize the safety of sensitive informants or administrative information over the transparency of research; the intellectual property of ideas and data is also at stake. The answer to this concern and trade-off (and to other issues) is to exercise common sense, an answer already included in the guidelines.<sup>3</sup> For example, research ethics, e.g., the content of informed consent agreements and confidentiality agreements in general, places bounds on the degree of transparency in DA-RT. As stated above, this logic of preregistration is only valid for an epistemology that seeks to describe and explain a certain research problem. Moreover, as Elman and Lupia (2016) and Bütthe and Jacobs (2015) state, there is no "one size fits all" standard, but there is a shared principle.

Research transparency is not different from transparency in politics and public administration. Modern democracies require transparency, and politicians and bureaucrats are subjected to the standards set forth by, for example, freedom of information laws. Politicians and civil servants usually complain that these standards in fact raise the costs of government and are time consuming, affecting policy and administration. This concern is also raised by scholars, who usually see transparency standards as unwarranted requirements in a line of work where the administrative and procedural burden is heavy. We agree with this concern and it is evident that these standards involve more work, more costs, and more time. Yet, DA-RT also brings many positive externalities and, as Elman and Lupia write: "... DA-RT is based on the broad and epistemically neutral consensus that the content of empirical social inquiry depends on the processes that produce it. Offering others access to these processes makes conclusions of social inquiry more understandable, more evaluable, and more usable" (2016, 45).

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<sup>2</sup> Preregistration limits the chances of manipulation at the beginning of the fieldwork stage, yet it does not rule out the possibility of ex-ante manipulation. This is particularly relevant for observational data studies, as is the case in qualitative research. Therefore, transparency in observational studies relies heavily on the researcher's principles.

<sup>3</sup> <https://www.dartstatement.org/2012-apsa-ethics-guide-changes>

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# The Penumbra of DA-RT: Transparency, Opacity, Normativity: A Response to Pérez Bentancur, Piñeiro Rodríguez, and Rosenblatt

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In the last few years a debate has been taking place on the best way to improve standards of research transparency in political science (Elman and Kapiszweski 2014; Büthe and Jacobs 2015; Isaac 2015; Pachirat 2015; Lupia and Elman 2016; Sil, Castro, and Calasanti 2016; Hall 2016; Fujii 2016; Htun 2016; Schwartz-Shea and Yanow 2016). Since 2009 efforts conducive to increasing openness, data access, and research transparency have been promoted under the name of DA-RT (Data Access and Research Transparency), and in 2012 such efforts crystallized in APSA's *Guide to Professional Ethics in Political Science*. Influenced by DA-RT's initiative, Section 6 of the Guide now reads: "Researchers have an ethical obligation to facilitate the evaluation of their evidence-based knowledge claims through data access, production transparency, and analytic transparency so that their work can be tested or replicated" (APSA 2012, 9-10).

But perhaps the first DA-RT initiative that caught the attention of political scientists more broadly was the controversial implementation of the Journal Editors' Transparency Statement (JETS) by several journals in the discipline in 2016. As is well-known, Jeffrey Isaac, then editor of *Perspectives of Politics*—one of APSA's flagship journals—refused to implement such standards. He wrote a piece defending relevant and problem-driven political science against what he saw as a resurgent neo-positivism in the discipline (Isaac 2015). As evidenced by this symposium, the debate on DA-RT seems far from over. I thank the QMMR editors for the opportunity to take part in this exchange and professors Pérez Bentancur, Piñeiro Rodríguez and Rosenblatt (hereafter PPR) for their challenging piece.

Like several proponents of DA-RT (Lupia and Elman, 2014, 2016), PPR favor the implementation of standards for improving openness and research

transparency in political science. In addition to endorsing DA-RT's initiative in general, PPR make novel arguments on the "unexplored advantages" of DA-RT for qualitative research. According to the authors, the adoption of DA-RT's guidelines, in particular the pre-registration of research designs, is useful not only for increasing transparency in research, but also for improving the quality of research as such. According to PPR, pre-registration is advantageous in the following, still unexplored, ways: 1) it improves research design; 2) it improves fieldwork; and 3) it improves qualitative analysis.

In the course of endorsing DA-RT and highlighting its added value, PPR make clear that the scope of their argument should be limited to "positivist qualitative research," that is to say, according to their definition, to "research that seeks to make descriptive and causal claims regarding a research problem," and make clear that "other traditions in the social sciences and humanities follow other epistemological rules" (this issue, 2). Such caution in delimiting the scope of their argument contrasts with less cautious statements in their piece regarding the normative implications of adopting DA-RT. According to PPR, DA-RT "is about ethics" and "[r]esearch transparency is not different from transparency in politics and public administration" (this issue, 10).

I will respond to their arguments in turn. First, I will focus on what I see as an implicit parceling of the discipline between research communities that can reach scientific status and research communities that cannot. In particular, I will take issue with PPR's definition of positivist qualitative research as consisting of descriptive and causal claims regarding a research problem. Second, I will tackle the issue of the difference between replication and replication-in-thought. I will suggest that the replication-in-thought argument can easily be

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reduced to absurdity and that we should either reject the idea or reformulate it in a way that is more sensitive to non-positivist approaches in political science. Finally, I will discuss the analogy between research transparency in political science and transparency in democracy and public administration. I will suggest that the analogy does not hold and will invite PPR to reflect upon the penumbræ in democracy's reality and aspirations and on alternative forms of democratic visibilization—i.e. of disadvantaged groups. I will conclude with a few thoughts on how to move forward.

### Who is the Scientist in the Room?

First it may be good to state the obvious: nobody I can think of is against increased transparency in political science research. This is in part why the emphasis DA-RT places on transparency remains puzzling. From my perspective the question is, rather, how to achieve such transparency and, more fundamentally, whether transparency as understood by DA-RT should be the highest value for which a research community needs to strive (more on this latter issue below).

On the issue of how to achieve transparency, DA-RT supporters oscillate between delimiting the scope of their standards to specific ways of doing political science and generalizing such standards for the discipline as a whole. On the one hand, PPR argue (this issue, 2) that interpretivists and other approaches in the social sciences and the humanities “follow other epistemological rules... and our discussion is not meant to suggest that one tradition is superior to others.” On the other, they contend that DA-RT offers “no ‘one size fits all’ standard, but *there is a shared principle*” (this issue, XX; emphasis added). PPR add, quoting Elman and Lupia, that “DA-RT is based on the *broad and epistemically neutral consensus* that the content of empirical social inquiry depends on the processes that produce it” (this issue, 10; emphasis added). Hence the reader is moved to ask: what do PPR do when they make cautious intra-disciplinary distinctions and, at the same time, appeal to an encompassing disciplinary consensus?

First, they invest in and invigorate intra-disciplinary distinctions and delimitations, for example, between positivist and non-positivist qualitative research. PPR do not discuss at length the difference between positivist and non-positivist approaches to qualitative research or include citations that justify such classification. Nor do they, for example, make distinctions between positivist, neo-positivist, and post-positivist approaches. Can all of these approaches be included under the umbrella

term of “positivism”? To be fair, PPR do not have to elaborate on these distinctions, but by proposing a clean and unsubstantiated break between “two cultures” *within* qualitative studies, they ultimately highlight the fact that positivism is precisely what is shared between quantitative research and a certain kind of qualitative one—the kind of qualitative research that they practice and endorse.<sup>2</sup>

Thus, while making the pluralistic gesture of denying any superiority to one type of research over the other, PPR claim that positivist qualitative research “seeks to make descriptive and casual claims regarding a research problem” (this issue, 2) and that DA-RT's logic “is only valid for an epistemology that seeks to describe and explain a certain research problem” (this issue, 9-10). In the absence of further clarification, the reader is left with the impression that unless political scientists engage in a kind of positivist (either qualitative or quantitative) research, they are precluded from making descriptive and causal claims. This argument is highly problematic and deserves further commentary on two fronts: a) the definition of descriptive claims and b) the definition of causal claims.

It is not quite clear what the authors mean here by “descriptive claims.” One may assume, for the sake of the argument, that descriptive claims are the opposite of “normative claims,” namely, that the former are concerned with what is and the latter with what ought to be. If we accept such standard distinctions between descriptive and normative claims, it is reasonable to assert that, unless social scientists engage in a kind of explicit normative theorizing, as political scientists doing normative and critical political theory often do, one is caught in the realm of, so to speak, descriptive claiming—and proudly so. But if this is the case, what do PPR mean when they say that “descriptive claims” are circumscribed to positivist qualitative research? Are they suggesting that non-positivist qualitative studies cannot make such claims? If this is true, what do PPR think that non-positivist qualitative studies actually do?

Similar concerns are raised by PPR's understanding of causal claims. PPR seem to assume that, within qualitative studies, only political scientists within the positivist camp can make such claims. This is a highly controversial statement, even among positivist qualitative researchers. For example, Gary Goertz, an expert on conceptual analysis within qualitative research, calls himself a positivist and argues that interpretive social scientists can make, and often do make, causal claims

2 For an alternative cartography of qualitative research in political science that distinguishes between different types of epistemologies and approaches to causation, see Koivu and Damman (2014).

(Goertz 2016, 48). Like Goertz, the interpretivist political scientist Fredric C. Schaffer asserts that non-positivist qualitative researchers do engage in causal claims with the caveat that they have a different understanding of causation. In Schaffer's words (2016, 53):

...many interpretivists do seek to explain and are interested in causes. As I see it, what distinguishes interpretivists is how they think about explanation and *how they conceive of causes*. In contrast to many positivists, who tend to think that explanation should be built up from generalizable causal laws or mechanisms, interpretivists are more likely to work up context-specific explanations. *Causal accounts can be so embedded in these context-specific interpretivist explanations that they are not recognizable as causal to someone looking for a discussion of laws or mechanisms.*

Thus, it seems to me that PPR's analysis may profit from a more nuanced understanding of the grey areas that complicate what they otherwise see as a clear-cut frontier dividing positivist from non-positivist approaches to qualitative research in political science. More nuance in their assessment of what they conceive as a positivist approach, as well as on the distinction between positivist and non-positivist approaches in political science, could make their analysis sharper and more convincing.

From Replication to Replication-in-Thought to Replication-of-Thought?

Commentators have noted that, when DA-RT supporters move from arguments designed for research communities that value replication and generalized explanation to research communities that do not, their arguments lose traction (Pachirat 2015, 29). In PPR's piece, this becomes evident when they drop the emphasis on replication as such to focus on the importance of pre-registration for what they call, following Büthe and Jacobs, "replication-in-thought." I will suggest that the "replication-in-thought" argument is vulnerable to a *reductio ad absurdum* and that, rather surprisingly, political theorists have been doing a similar type of "replication" for several centuries—perhaps since the times of Aristotle.

DA-RT supporters value replication because, in the terms of Gary King (1995, 444), "empirical political scientists need access to the body of data necessary to replicate existing studies to understand, evaluate, and especially build on this work." Thus, the first step

towards implementing replication is the generation of a replication data set. In the context of qualitative studies a data set involves a "detailed description of decision rules followed, interviews conducted, and information collected. Transcripts of interviews, photographs, or audio tapes can readily be digitized and included in a replication data set" (King 1995, 446). Moreover, data sets should be readily available to other researchers: "(o)nce a replication data set has been created, it should be made publicly available and reference to it made in the original publication (usually in the first footnote)" (King 1995, 446).

Whereas King was still committed to replication with a capital R, Büthe and Jacobs advocate for a different, qualified form of replication.<sup>3</sup> In their concluding essay for a symposium on research transparency, they concede: "Replication so far has not featured nearly as prominently in discussions of transparency for qualitative as for quantitative research," and yet argue that many of the participants in the symposium endorse a standard "that we might call enabling 'replication-in-thought': the provision of sufficient information to allow readers to trace the reasoning and analytic steps leading from observation to conclusions" (Büthe and Jacobs 2015, 57). Thus, "replication-in-thought involves the reader asking questions such as: Could I in principle imagine employing the same procedures and getting the same results? ... Replication-in-thought also allows a reader to assess how the researcher's choices or starting assumptions might have shaped her conclusions" (Büthe and Jacobs 2015, 57).

Described in this way, replication-in-thought does not seem to lead in every possible scenario to an actual replication (with a capital R) of results by using a replication data set. It may be that the feedback and criticism provided by colleagues in workshops and conferences in the discipline, as well as by a rigorous double (or triple) blind peer-review process on the way towards publication, can help "trace" the reasoning, from observation to conclusion, that is necessary to judge good from bad scholarship. Book reviews after publication can add additional stages of such "tracing," by eliciting praise or scaffolding critique. In this context, Pachirat (2015, 30) seems right in suggesting how pointless it is to ask an ethnographer to "post to a repository the fieldnotes, diaries, and other personal records written or recorded

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3 According to my reading there is a shift of emphasis from King's original definition of replication, based on the provision of a data set (what I call replication with capital R), and Büthe and Jacobs's claim that a more widely accepted understanding of replication can be conceived as the tracing of reasoning and analytical steps that lead from observation to conclusions. The latter definition is, or so I argue, softer than King's and is designed to be more appealing to scholars doing qualitative work.

in the course of their fieldwork.” He then reduces the argument to absurdity, speculating that DA-RT can well turn into VA-RT (Visual and Audio Recording Technology), which social scientists could wear 24 hours a day and be “digitally livestreamed to an online data repository and time-stamped against all fieldwork references in the finished ethnography” (Pachirat 2015, 30).

Reducing arguments to absurdity is one possible way of examining an argument critically—a peculiar type of replication-in-thought. By following the argument to its ultimate, often unforeseeable, consequences, one can test whether the argument is sound, internally coherent, and even desirable. In this replicating spirit, one can conduct a thought experiment and suggest the following: the best way to replicate results is not replication-in-thought but replication-of-thought. In other words, why settle on a limited dataset if we could replicate the exact thought and reasoning processes followed by researchers when making decisions about collecting, processing, and interpreting data? If only neurologists could produce a device that could allow us to read another’s train of thoughts in detail, we, as social scientists, would be able to spot inconsistencies, detect alternative avenues not followed, as well as track invalid arguments, achieving greater (full) transparency in the discipline. Paradoxically, without much help from neurologists and cognitive scientists thus far, political theorists have been exercising such replication-of-thought by reading carefully and often mercilessly the work of colleagues, in order to spot inconsistencies, contradictions, shifts of emphasis, and conclusions that do not follow from premises. It would be ironic to conclude that DA-RT is actually inviting us to do something that many political scientists with a rich theoretical background have been doing pretty seriously for quite some time.

### **What You See is Not What You Get: Transparency, Opacity, Democracy**

The final, and more substantial, issue I would like to raise is about PPR’s understanding of democracy and its relation to political science research. PPR’s arguments seem cautious when establishing distinctions between positivist and non-positivist camps within the discipline. They are less cautious when making normative claims about transparency and democracy. According to PPR, transparency in research is analogous to transparency in democracy and public administration. Therefore, increasing transparency in research is tantamount to making laws of freedom of information binding to state

authorities and public administrators. But is transparency valuable for democracy in the same way that it could be valuable to scholarly research? Is transparency such an important value for democracy after all?

In the majority opinion of the well-known 1965 case, *Griswold v. Connecticut*, Justice William O. Douglas introduces the term “penumbra” (from the Latin *paene*, almost, and *umbra*, shadow) to justify the right to marital privacy regarding consultation on the use of contraceptives. As is well-known, the case involved the arrest and conviction of C. Lee Buxton, a gynecologist from Yale, and Estelle Griswold, the head of Planned Parenthood in Connecticut, on the grounds that they violated state statutes that prohibited the use of any drug or device for the purposes of contraception. Douglas suggested that privacy rights could “emanate,” be inferred or extended, from rights not clearly stated neither in the Constitution nor in the Bill of Rights, but that could nonetheless be construed by exercising a kind of “penumbral reasoning.” Rights could be found in the “penumbrae” of other constitutional protections. In a different context, political scientist James Scott focused on how subordinated groups assent to established authorities in public life while expressing dissent in other, less obviously visible, areas of social life (Scott 1990). Scott named these alternative ways of dissent “hidden transcripts” and suggested that a good social scientist should be able to identify and read such transcripts in order to grasp power relations at work in a specific political and social reality. In both cases, the co-implication of what is visible (in law, in power relations) and what remains opaque, hidden, or even secret, is problematized in ways that help us think about how rights can be expanded and oppression can be contested. Put simply, penumbrae have much to do with how democracy works and is (at least potentially) augmented. If Douglas and Scott are right, then transparency—or the total absence of shadow—is less central to democracy than one may have thought; at times it may even be irrelevant.

Moreover, it could be argued that, paradoxically, a certain understanding of democratic transparency can in fact *fail* to make relevant political agendas *more visible*. For example, one could ask whether DA-RT’s take on research transparency can help make disadvantaged groups in political science such as women, racial and ethnic minorities, the LGBTI community, and people with disabilities more visible.<sup>4</sup> Put differently: What kind of political science is required to *increase the visibility* of these groups? Why is the discussion on research

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4 See, for example, Teele and Thelen (2017), Hanchard (2018), Ravecca (2019), and Arneil and Hirschmann (2017).

transparency limited to the visibility and availability of data and not about the visibility of real, concrete, relevant, and pressing real-life causes and agendas in society and within the political science research community as a whole? These are big questions that I cannot answer here, nor do I expect PPR to answer them—but it would be helpful if they could take them into account in their assessment of what democracy (and research) renders visible and what could both continue to make invisible.

### Concluding Remarks

Commentators of DA-RT have argued that the degree of emphasis and concern with standards for research transparency is a phenomenon that political science shares only with economics (Sil, Castro, and Calasanti 2016). Sociology, anthropology, and history do not seem to be as troubled by their standards of transparency in research. Does this mean that these disciplines are lagging behind more “modern” and “professionalized” ones? Or could it be, on the contrary, that political science shows a recurrent inferiority complex regarding economics? Why is it that developments in economics have such traction in specific research communities within the vast discipline of political science? Are other disciplines simply more self-confident about their own standards of research and scholarly production?

I ask these questions because they are important and because PPR worry that “if social scientists desire a better regard among the larger science community, they must adapt and be willing to fully disclose the nature of their research process” (this issue, 9). I believe that PPR’s worry is key because it reveals disciplinary anxieties regarding the scientific status of political science— anxieties that are increasingly shared across several (though not all)

sub-fields in the discipline. However, it would be helpful to remember that classic, groundbreaking figures in the field of political science, such as Thomas Hobbes, John Locke, and John Stuart Mill, among others,<sup>5</sup> were not just adapting their research to more stringent and demanding “scientific standards;” they were *setting the standards of science itself* as they made major contributions to the realm of political thought and science widely understood. I wonder when and how political scientists began to perceive that their scholarship was in need of an upgrade, or adaptation, to more rigorous scientific standards imposed from outside and above, instead of conceiving themselves as co-creators of such standards for the scientific community as a whole.

Finally, although PPR do not push the issue of replication with a capital R further, they invest in pre-registration as a way of improving not only research transparency but also qualitative research as a whole. Although PPR make their point clear, it remains to be shown that pre-registration is in fact the cause of a better research design, better fieldwork, and better qualitative analysis. It may very well be that what is needed to improve research design, fieldwork, and qualitative analysis is simply planning in advance, thinking hard about the best way to observe and gather data on a given phenomenon, being clear about rival hypotheses, and being open and aware about the many ways in which theory and evidence, as PPR claim, take part in an “iterative process” (this issue, 4). If this is the case, then pre-registration could simply be a by-product of, well, a good researcher doing her or his work thoroughly, professionally, and in advance.

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5 On Hobbes’s relation to the natural sciences, see Shapin and Schaffer (1985); on Locke’s contribution to modern empirical and experimental science, see Michael Ben-Cham (2004) and Matt Priselac (2017); on Mill’s influence on inductive oriented scientific investigation, see Raguin (2014); on Alexis de Tocqueville’s uncanny talent for predicting future political events, see Boesche (1983).

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# A Response to Rossello

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We have carefully read Rossello's reaction to our paper, and we appreciate his taking the time to provide helpful comments and critiques. In this short reaction paper, we explain some of our ideas in greater detail. We will first discuss our argument regarding positivism and interpretivism and the role of causal and descriptive inference in qualitative research. We then will discuss Rossello's critique of the idea of "replication-in-thought." We suggest there is a trade-off between transparency and the ability to problematize the situation of disadvantaged groups. We conclude with a general assessment of Rossello's perspective and the debate concerning DA-RT.

Rossello criticizes our distinction between positivist and non-positivist qualitative research. It is not our intention to debate the location of the boundary between positivist and interpretivist research. This has been extensively discussed elsewhere (e.g. Almond 1988; George and Bennett 2005; Goertz and Mahoney 2012; Koivu and Damman 2015). For example, when presenting the tools for conducting case study research, George and Bennett claim: "The term 'qualitative methods' is sometimes used to encompass both case studies carried out with a relatively positive view of the philosophy of science and those implemented with a postmodern or interpretative view" (George and Bennett 2005, 18). In the same spirit, our only purpose in making this distinction is to set the scope conditions for the ideas we advance. Thus, we recognize that significant differences exist within the positivist qualitative tradition (Koivu and Damman 2015), and that interpretivists and positivists share some common ground.

In a similar vein, regarding the role of descriptive inference and causality, we agree that interpretivism also makes descriptions and causal claims. Nevertheless, interpretivists are more oriented toward understanding a case in its context (Koivu and Damman 2015). They construct an interpretation of a phenomenon based on

the way actors understand their reality. Moreover, the researcher herself plays a role in the construction of meaning. Thus, for an interpretivist, it makes no sense to pre-register research that is not theoretically oriented and that is completely context dependent.

Rossello also criticizes the idea of "replication-in-thought" (c.f. Büthe and Jacobs 2015) and offers a critique similar to that raised by Pachirat (2015). He claims (this issue, 9) that DA-RT "...is actually inviting us to do something that many political scientists with a rich theoretical background have been doing pretty seriously for quite some time." However, against his point, the logic of "replication-in-thought" is not about the logical consistency of the argument and the relationship between the argument and its empirical grounds. "Replication-in-thought" refers instead to the possibility of knowing the process of data collection and, in the case of qualitative research, the iteration between theory-building and evidence. Replication is only possible in the qualitative setting if there is information about what evidence was sought, what was not sought (or could have been sought), and what was used and not used to generate descriptive and causal inferences. The process facilitates research transparency because it reveals, to readers and to the research community, the decisions made during the iterative process that led to a set of given conclusions.

Rossello finally states that transparency is not necessarily a desirable goal of research or of democracy. Moreover, he states that a "...certain understanding of democratic transparency can in fact *fail* to make relevant political agendas *more visible*" (this issue, 10). The problem with this point is that there is no trade-off between transparency and raising awareness of the conditions of disadvantaged groups. In democracy, transparency is necessary for accountability. For example, it helps citizens determine whether allocated public funds are expended in accordance with the original goals of the public policy. Transparency reduces the discretion available to those

in power—in this case, politicians vis-à-vis citizens. Researchers, like politicians, are in a position of privilege and hold power. Research transparency limits that power and holds researchers accountable vis-à-vis those who fund their research—beyond the self-control exerted by the academic community. Transparency in democracy is not oriented towards calling attention to disadvantaged groups. The point is whether progress in terms of transparency affects the visibility of disadvantaged groups. Is there a trade-off? The two examples cited by Rossello do not show such a trade-off. Instead, they illustrate a different problem, namely, the forms of domination of disadvantaged groups. As far as we know, there are neither theoretical nor empirical grounds to support the existence of such a trade-off.

Finally, our argument does not ascribe different scientific status to different research traditions. Nor do we aim to promote a standard that suffocates

researcher creativity and the generation of knowledge. We simply suggest a tool to improve the practice of a given tradition in political science. Yet, as with every proposal, discussion of the tool's merits should address the potential trade-offs that it might imply in practice. Does it differentially affect scholars in the North versus the South? Does it necessarily produce a bias against particular research agendas? Does it negatively affect a given group of scholars? Does it negatively affect the chances of publication or the academic career of a given group, e.g. women? These are all crucial questions for any scientific community. Unfortunately, the different debates about how to conduct research in the social sciences at times seem concerned more with dismissing different ways of producing knowledge than with devising ways to improve the practice of research and the democratization of scientific knowledge generation.

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# Articles

## The Reshaping of the Quantitative-Qualitative Divide: A Case of QCA

Dawid Tatarczyk

### Introduction

Researchers have long argued that quantitative and qualitative methods are distinct and, therefore, serve different analytical purposes. Quantitative methods tend to be associated with large-N analysis and systematic theory testing, while qualitative methods are believed to provide thick accounts of one or few cases. However, in recent years, a pair of interconnected processes has reshaped the quantitative-qualitative divide in political science. Not only is our discipline in the middle of a mixed methods boom, but that trend is also being bolstered by innovations to the very methods used in mixed methods scholarship. These two processes raise pragmatic questions about what counts as mixed methods research, and even more fundamentally about its best practices. Therefore, political scientists are prompted to reconsider the qualitative-quantitative divide, especially as the meaning of these two labels continues to evolve. Doubtlessly, sound methodological advice depends on a proper understanding of what separates these two research traditions.

Using Qualitative Comparative Analysis (QCA) as an example of a research tool that has evolved methodologically, this article shows that a number of previous understandings of the quantitative-qualitative divide are no longer salient. This essay extends the conceptualization of “two cultures” (Goertz and Mahoney 2012) and emphasizes that, since both cultures build on the same foundation of formal logic, they are not as distinct as previously assumed. On the other hand, despite a common foundation in formal logic, both quantitative and qualitative methods make use of specific, albeit different, types of mathematics. These mathematical superstructures are hugely important, because they determine which two methods can be

combined for the purposes of mixed methods research (see, e.g., Humphreys and Jacobs 2015).

This article concludes by underscoring the importance of training institutes, focusing on QCA meetings worldwide (2005-2018), to demonstrate the point that the line separating quantitative and qualitative methods is not clear-cut and continues to evolve. QCA became more popular after the Comparative Methods for Systematic Cross-Case Analysis (COMPASSS) network was established in 2003, but its rise has been facilitated by its epistemological overlap with statistical approaches. Today, the method is regularly taught at key institutes around the world, and, in 2018, even the quantitatively oriented Inter-University Consortium for Political and Social Research (ICPSR) institute incorporated QCA into its curriculum.

### New Boom, Old Questions

A new wave of mixed methods scholarship is sweeping political science. Indeed, from 2000 to 2013, “the number of references to multi-method research grew from 14% of the references to OLS regression, to 26%, while references to mixed-method research grew far more impressively, from 8% to 47% of the number of search results for OLS regression” (Seawright 2016, 3). The early phases of the mixed methods boom were underpinned by the implicit understanding that two methods were better than one. Using numerous tools within a single research design was meant to make inferences more robust. This approach was widely referred to as triangulation, and corroboration was its goal (see, e.g., Jick 1979). Seawright (2016) raised a number of objections to triangulation and proposed integration as an alternative way of strengthening mixed methods analysis. With integrative mixed methods, one

method produces a final inference while other methods are meant to test and refine the analysis producing that inference. Nevertheless, a common complaint among scholars practicing mixed methods research is that many potential practitioners lack the methodological training and knowledge to do it properly (see, e.g., Humphreys and Jacobs 2015, 655).

QCA is arguably the most formalized of what is usually understood as a qualitative method. Its development is evident both in methodological terms, as the method continues to incorporate more sophisticated algorithms, and in the way the method is practiced and taught. Consider that Charles Ragin developed QCA as a case-centered alternative to statistical inference, and yet these days the method is regularly taught by a number of key institutes around the world, including the quantitatively oriented ICPSR.

QCA was created to synthesize the best aspects of both quantitative and qualitative approaches. Over the years, the method has evolved from crisp-set analysis (Ragin 1987) to fuzzy-set analysis (Ragin 2000, 2008), and to enhanced standard analysis (Schneider and Wagemann 2012) and beyond. As QCA became more formalized, it also began to attract more attention from methodologists who were critical of the approach. For instance, Krogslund, Choi, and Poertner (2015) argue that QCA's findings are highly contingent upon the parameters selected by the user. Similarly, Braumoeller (2015) complains that QCA is highly susceptible to producing results that exhibit Type I error (i.e., false positive). QCA's methodological merits and drawbacks were also the topic of many symposia in high-profile journals. In 2013, *Political Research Quarterly* published a mini-symposium titled "QCA, 25 Years after 'The Comparative Method'" and *The Qualitative & Multi-Method Research Newsletter* released in the spring of 2014 was also almost exclusively focused on set-theoretic approaches. Likewise, in May 2016, *Comparative Political Studies* published a special issue titled "Debating Set Theoretic Comparative Methods." Both methodological criticism and debates in highly respected research journals have made QCA more well-known while helping the method's practitioners address some of its technical shortcomings. However, a key point of contention in these debates is the question of contrast between QCA and standard statistical methods (see, e.g., Grofman and Schneider 2009; Schneider and Wagemann 2012, 86-90; Thiem, Baumgartner and Bol 2016). The key question is whether QCA is more of a qualitative or quantitative method. It thus becomes necessary to

re-examine how political scientists have conceptualized these two research traditions.

## **The Quantitative and Qualitative Research Traditions in Political Science**

When the study of politics was still in its incipient stages, qualitative research served as the dominant methodological paradigm. Munck (2007) shows that comparative social science has a particularly long-standing tradition of qualitative work, and Adcock (2007, 184) reminds us that the same can be said about political science in the post-World War II era in general. In the 1960s, the behavioral revolution re-shaped American political science. As more scholars began using statistical inference in search of systematic patterns and greater generalizability, qualitative approaches began to lose their appeal. Around that time, Lijphart (1971, 683-5) underscored that there exists a hierarchy of research methods. Experiments produce the most valid inferences, followed by statistical approaches. Qualitative methods come in last, and the author portrays them as the least reliable in producing robust findings. The supposed problem with comparative case studies, and qualitative methods in general, is that they analyze too few cases to permit proper control (Lijphart 1971, 684).

Researchers have long argued that a quantitative template can and should be applied to qualitative research. That indeed was the primary goal of King, Keohane, and Verba (KKV). Their 1994 book proved to be hugely influential, achieving a canonical status in the discipline. The impact of the publication was so great that Mahoney (2010, 122) speaks of a post-KKV era in the social science methodology. Brady and Collier's (1994) edited volume was published as a reaction to the challenges posed by KKV. While the book attempted to underscore the distinctiveness of qualitative methods, with some authors going as far as to accuse KKV of methodological imperialism, the volume, nonetheless, emphasized the importance of shared standards. Hence, the underlying aim of the volume was to underscore the distinctiveness of qualitative methods, while at the same time bringing them more in line with the quantitative research tradition.

Only in recent years has the distinctiveness of qualitative methods been described and then defended on its own terms. George and Bennett (2005) showed why even a single case study could be important from both methodological and substantive points of view. And, while both Sartori (1970) and later Collier and Mahon (1993) wrote about the importance of proper

concept formation, this critical element of social science research was formalized only more recently by Goertz (2006). Qualitative methodologists continue to assert their place in political science (see, e.g., Cyr 2019; Goertz 2017; Kapiszewski, MacLean and Read 2015). The insistence that qualitative methods are valuable, albeit different from statistical methods, has led Goertz and Mahoney (2012) to conclude that qualitative and quantitative methods belong to different cultures. However, what exactly differentiates these cultures is still a matter of considerable debate. Not only are scholars oscillating between narrower and more encompassing conceptualizations of the two research traditions, these definitions are also continuing to evolve. In light of methodological developments associated with QCA and other methods (e.g., process tracing), few of these definitions give us clear guidance on how a particular approach should be classified.

Some scholars advocate a clear-cut categorization. King, Keohane and Verba (1994) insist that:

Quantitative research uses numbers and statistical methods. It tends to be based on numerical measurements of specific aspects of phenomena; it abstracts from particular instances to seek general description or to test causal hypotheses; it seeks measurements and analyses that are easily replicable by other researchers. Qualitative research, in contrast, covers a wide range of approaches, but by definition, none of these approaches relies on numerical measurements. Such work has tended to focus on one or small number of cases, to use intensive interviews or depth analysis of historical materials, to be discursive in method, and to be concerned with a rounded or comprehensive account of some event or unit (3-4).

King, Keohane, and Verba's distinction rests upon the questionable assumption that qualitative methods do not deal with numbers, but the practice of calibration in QCA (i.e., the process by which set membership scores are assigned to cases) shows that, in fact, the opposite is true.

Other definitions are equally encompassing, but provide a more fine-grained analysis of what separates both research traditions. Collier and Elman (2010, 781) argue in favor of disaggregating the qualitative-quantitative distinction in terms of four criteria: level of measurement, large-N versus small-N, use of statistical and mathematical tools, and whether the analysis builds on a dense knowledge of one or few cases or on the thin

analysis of large-N studies. QCA, however, can be applied to any level of measurement and process any number of cases. Although its methodological superstructure is different from that of statistical methods, the method nonetheless builds on a sound mathematical framework. Finally, although QCA calibration should always be informed by a case study expertise, researcher's knowledge of particular cases decreases as the number of countries in the analysis goes up. This is not a problem with calibration per se, but an observation that our resources and intellectual capabilities are limited. A single person can be an expert only in so many cases. Because of this trade-off, calibration in large-N QCA analysis necessarily depends on a thinner knowledge of cases, making such analysis very similar to other cross-sectional methods that can also process an unlimited number of cases.

On the other hand, Gerring (2017) provides a much narrower understanding of qualitative methods. He classifies methods based on the type of data they can process. Thus, quantitative methods analyze observations that are comparable, while qualitative methods make use of non-comparable data, regardless of how many observations there are (Gerring 2017, 18). The proposed dichotomy is so minimalist that it actually says very little about the two research traditions and focuses instead on two types of data. However, even that distinction is not perfect because qualitative data can be translated into quantitative observations, although not the other way around. As an illustration, we can reduce qualitative data about any country to a single number in a data matrix, but once the reduction is complete the initial qualitative information will be impossible to infer. Furthermore, the author also recognizes that QCA – widely recognized as a qualitative method – escapes this clear-cut dichotomy by being able to analyze comparable observations (Gerring 2017, 19).

Each of these distinctions between qualitative and quantitative methods is challenged by QCA. When King, Keohane, and Verba (1994) published their book, QCA was still in its incipient stages, although even then crisp-set QCA would invalidate their unambiguous dichotomy. Collier and Elman (2010) offered a more nuanced conceptualization of the two research traditions, and yet their emphasis on four criteria does not allow us to place QCA neatly in either camp. Finally, Gerring (2017) created a minimalist definition of quantitative and qualitative methods, but his argument tells us more

about the data these methods analyze, rather than the methods themselves.

Goertz and Mahoney's discussion of the two cultures, by contrast, is extremely useful, I suggest, because it sheds new insight on the difficult problem of what exactly separates qualitative and quantitative traditions: "We even suggest that the two traditions are best understood as drawing on alternative mathematical foundations: quantitative researchers grounded in inferential statistics (i.e., probability and statistical theory), whereas qualitative research is (often implicitly) rooted in logic and set theory" (2012, 2). In making this comment, Goertz and Mahoney (correctly, I think) point out that qualitative and quantitative methods are dissimilar because they make use of different types of mathematics. They do not, however, extend their definition to its logical conclusion. One implication of Goertz and Mahoney's treatment of qualitative and quantitative traditions is that these traditions are closer to each other than the authors are willing to recognize. As the prominent philosopher and logician Bertrand Russell put it, "all pure mathematics follows from purely logical premises and uses only concepts definable in logical terms" (Russell 1995, 57). The essence of Russell's argument is that every rule of arithmetic can be expressed in the language of formal logic. We might even say that logic provides the structure for all branches or superstructures of mathematics. Therefore, both research cultures are built on the basis of formal logic and make use of particular branches of mathematics only later on. When Goertz and Mahoney insist on thinking about these cultures as distinct, they undervalue the extent of overlap that exists between them.

On the other hand, Goertz and Mahoney's conceptualization of qualitative methods is too broad. Not every qualitative approach is committed to making use of formal logic, and thus to a systematic analysis of social phenomena. Gensler writes that "logic is the analysis and appraisal of arguments" (2002, 1). On such a reading, logic is a tool that we use to understand the world around us. Social scientists might not be able to provide definitive answers, but their commitment to logic as a mode of rational thinking orients them towards a neo-positivist tradition. Here is how another methodologist explains this view: "they [social scientists] intend to study human action in a systematic, rigorous, evidence-based, falsifiable, replicable, generalizable, nonsubjective, transparent, skeptical, rational, frequently casual, and cumulative fashion" (Gerring 2011, 2). Advocates of deconstruction, postmodernism, post-structuralism and

other related approaches would likely reject some or all of these goals. That is also why they have been subjected to fiercer methodological criticism (see, e.g., Elster 2015; Sokal and Bricmont 1998). Goertz and Mahoney (2012, 4) do acknowledge that the qualitative camp has many divisions, but by not extending their definition fully, the authors group under one label of qualitative methods a great variety of approaches that are radically different, if not outright contradicting, in their methodological orientation. For example, the epistemological and ontological gulf between QCA and post-modernism is much wider than the gap between QCA and regression analysis. Epistemological differences and similarities matter (see also, Koivu and Damman 2015), and, in this case, they help explain why QCA overlaps with statistical methods and why the former approach is a part of ICPSR's curriculum.

Acknowledging that some qualitative methods build on the structure of formal logic while others do not may seem trivial. It is, in fact, crucial in terms of today's concern over mixed methods best practices. The final implication of Goertz and Mahoney's conceptualization of two cultures is that mathematical superstructures determine whether two methods are compatible with each other. This suggestion is already acted upon in the practice of mixed methods research. Consider the Bayesian Integration of Quantitative and Qualitative data (BIQQ). Humphreys and Jacobs (2015) present BIQQ as a new mixed methods approach that combines elements of correlation-based techniques and process tracing. This innovation, however, is only possible "because the inferential strategies of both qualitative and quantitative analyses can be described in Bayesian terms," and from there "it is a short step to combine the two forms of inference within a single analytic framework" (Humphreys and Jacobs 2015, 671). Basically, mathematical superstructures of both approaches are similar enough to allow for their fusion. In other cases too, the merging of the Bayesian approach with statistics (Kennedy 2008, 213-26) or with process tracing (Bennett 2014) is built upon and presupposes the already existing mathematical commonality.

### **Bridging the Quantitative-Qualitative Gap while Remaining Distinct**

Although QCA tends to be presented as a qualitative-based alternative to statistical approaches (see, e.g., Ragin 2008; Schneider and Wagemann 2012), the method does in fact build on the same logical structure as regression analysis. It is thus worth asking if QCA can serve as

a bridge between statistical methods and more case-oriented approaches. Generally speaking, both QCA and OLS regression aspire to generalizable cross-sectional findings, while sharing a common challenge of separating association from causation. Moreover, both need to be supplemented by process-tracing analysis to examine causal mechanisms in particular cases. Furthermore, Paine focused on technical parallels to demonstrate that truth table analysis is similar to that of multiplicative interaction terms because “regression can be used to convey the same information about necessary/sufficient condition hypotheses as a truth table” (2016, 4). I extend his analysis by adding a meta-commentary that embeds the discussion of multiplicative interaction terms within the broader context of heteroskedasticity. The latter serves as a fruitful point of comparison because both methods deal with this phenomenon differently, and this contrast allows us to see the degree of epistemological overlap between them as well as the limits of methodological fusion.

In OLS regression, the homoskedasticity criterion assumes that the variance of the error term is constant across observations (Lewis-Beck 1980, 26). Thus, if educational attainment has a positive impact on future earnings, we would expect this finding to hold for all observations in our sample. Consider, however, that an investment banker with an MBA degree from a prestigious university might make significantly more money than a social worker with a similar level of formal education. In this hypothetical example, the variance of the error is larger at higher levels of educational attainment. In short, heteroskedasticity is present because highly educated people have considerably different income levels.

The presence of heteroskedasticity poses a serious problem for standard OLS models since it makes regression estimators inefficient. That is why statistical software programs are equipped with diagnostics tests meant to recognize the presence of heteroskedasticity (e.g., the Breusch-Pagan test) and with remedies to mitigate its impact (e.g., the bootstrap technique). These mechanical solutions are useful if the origins of heteroskedasticity are an artifact of incomplete data or flawed survey instruments. For instance, if economic crises are more dangerous to survival of dictatorships than democracies, then scholars interested in the validity of modernization theory might face a challenge in that quality country-level data is often contingent on the level of economic development because non-democratic regimes will have an incentive to hide their poor economic performance (Przeworski and Limongi 1993, 62). Moreover, highly-

developed countries collect and report data on economic performance much better than authoritarian regimes since democracies derive legitimacy from more than their economic performance, and therefore imprecise financial data about the economic performance of poor and/or non-democratic regimes would lead to biased OLS estimators (Przeworski and Limongi 1993, 62-64).

But, in some cases, heteroskedasticity might simply be a fact of the world around us. As in the hypothetical example above that links educational attainment with increased earnings, it simply might be true that education does, in fact, have a different effect on the wages of highly trained people because the type of occupation they pursue moderates its effects. When scholars expect that an independent variable will not have the same effect on different groups they have turned to using interaction terms. This, in turn, relaxes the unit homogeneity assumption. An interaction effect among independent variables occurs when a change in the value of one independent variable affects the impact of another independent variable on the outcome. To illustrate this point, consider the following example: “Changes in the amount of sunlight a plant is exposed to make little difference if the plant does not receive any water, but makes substantial difference if it does: water (or its absence) moderates the impact of sunlight on plant growth; and the converse is true as well, of course: the amount of sunlight that a plant receives moderates the impact of water on plant growth” (Braumoeller 2014, 42). Interaction terms are very useful because they allow us to analyze politics in a more realistic way. Yet, this technique is not easy to use in the context of standard regression analysis. Brambor, Clark, and Golder (2005) found that only 10% of articles published in the top three political science journals between 1998-2002 applied and interpreted interaction terms correctly. Moreover, analysis of three or more interaction terms, while technically possible, would be very difficult to carry out and interpret.

QCA differs from regression analysis in that it does not treat heteroskedasticity as a problem to be solved. To illustrate this point, Ragin examines data on electoral districts voting in Great Britain and observes that, when the percentage of the population employed in manufacturing sector was low, the degree to which people vote along class lines could be either high or low. Yet, when the percentage of the population employed in manufacturing was high, the level of class-based voting was always high (2000, xiii). From the perspective of statistical analysis, such findings exhibit severe

heteroskedasticity. Furthermore, a conventional theory of voting behavior would suggest that in manufacturing areas class-based voting is high, but this theory does not claim that having a high level of manufacturing is the only way to generate high level of class-based voting (Ragin 2000, xiv). Thus, Ragin's findings do not contradict the overall theoretical argument and, in fact, we have learned something important. Namely, that class-based voting can also be present in areas where people are not primarily employed in manufacturing sector. Ultimately, then, QCA practitioners recognize and even embrace a scenario in which alternative factors can produce the same outcome (i.e., equifinality), while statistical analysis aims to identify the most powerful predictor for explaining the variance in the dependent variable, and thus are driven by the assumption of unifinality (Schneider and Wagemann 2012, 86).

OLS regression and QCA might overlap in the way they deal with heteroskedasticity, but the two approaches remain distinct. Ragin writes that interaction terms "have all the appearance of testing for combined causes, but they are really generic, omnibus tests for non-additivity, not tests for the exact causal combination specified in the multiplicative term" (Ragin 2013, 1). This contrasts with QCA and its set-theoretical superstructure, which allows it to focus on combinations of conditions rather than non-additivity. Indeed, "the basic idea behind truth table analysis is to find the combinations of conditions that yield highly consistent membership in the outcome set and the focus, then, is on what it takes to meet or surpass a threshold value" (Ragin 2013, 1). In OLS regression, multiplicative interaction terms are a way to analyze politics in a more complex way, while also addressing the presence of heteroskedasticity, but this strategy requires that certain assumptions underpinning standard regression analysis be relaxed. QCA, by contrast, was designed to discover causal paths that lead to specific outcomes, and the method's focus is on combinations of conditions required to reach a specific outcome threshold. That is also why QCA can easily deal with three-way and higher order terms (Axel, Rihoux, and Ragin 2014, 118).

On the one hand, Paine's (2015) argument that QCA and OLS regression are related because truth table analysis and multiplicative interaction terms allow researchers to carry out similar analyses rests on the implicit assumption that their methodological structures are similar enough to allow for such convergence. On the other hand, such insistence overlooks the fact that these methods make use of distinct mathematical

superstructures (probability calculus/matrix algebra vs. set-theory), which, in turn, makes them approach the issue of heteroskedasticity differently. These observations have significant implications for the practice of mixed methods research, as well as for the potential to bridge the quantitative-qualitative gap. Compatibility among methods is, for the most part, determined by their mathematical superstructures. Where epistemological overlap does not exist, the gap between quantitative and qualitative methods will persist. Hall (2003) reminds us about the importance of achieving a fit between the character of the world as it actually is and the choice of research method in order to increase the validity of empirical inferences. It becomes apparent that his recommendation to align ontology and methodology also extends to the practice of mixed methods scholarship. As we will see next, the existing epistemological overlap between QCA and statistical methods is also reflected in the way the former approach is taught. These days even highly quantitative training institutes (e.g., ICPSR) have incorporated QCA into its curriculum.

### **Methodological Institutes**

This section focuses on methodological institutes to show that QCA is now a mainstream method. Over the last decade, the QCA approach has been transformed from a method on the fringes to being taught at all major methodological institutes worldwide. Such institutionalization is most likely aided by the fact that QCA shares important epistemological similarities with mainstream statistical approaches. Mixed methods research is certainly on the rise, but the relationship between quantitative and qualitative methods continues to be asymmetrical. For example, a survey of the top ten political science and sociology journals from 2001 to 2010 reveals that both fields favor quantitative methods, as 73% of the articles utilize some form of statistical research methods (Goertz and Mahoney 2012, 227). Qualitative methods are used in 31% of the articles, of which only 1% used QCA (Goertz and Mahoney 2012, 228). To be sure, there are exceptions to this rule in some pockets of our discipline. Moravcsik (2010, 29) notes that over 90% of IR scholars employ some form of qualitative analysis. However, when it comes to methodological training, the asymmetry becomes especially acute. Emmons and Moravcsik (2016) find that only sixty percent of the top political science departments offer

any dedicated graduate training in qualitative methods, and that percentage is declining over time.

Social scientists have long recognized the importance of institutional infrastructures for the development and promotion of various methodological approaches. Seawright (2016, 4) acknowledges that the launching of *The Journal of Mixed Methods Research* has been crucial for the spread of mixed methods research. Similarly, Goertz and Mahoney (2012, 5) emphasize that political science methodologists have organized themselves into either the Section on Political Methodology to represent quantitative methods, or the newer Section on Qualitative and Multi-Method Research. Methodological institutes matter as well (Collier and Elman 2010). Consider that between 1984 and 1997 only 39 articles using QCA appeared in peer-review journals (Axel, Rihoux, and Ragin 2014, 121). However, Axel, Rihoux, and Ragin point out that political scientists started to use the method more frequently once the COMPASSS network was created in 2003; the network aims to stimulate the discussion of best QCA practices, offers an informal peer-review section, and develops courses for the ECPR (European Consortium for Political Research) (2014, 125).

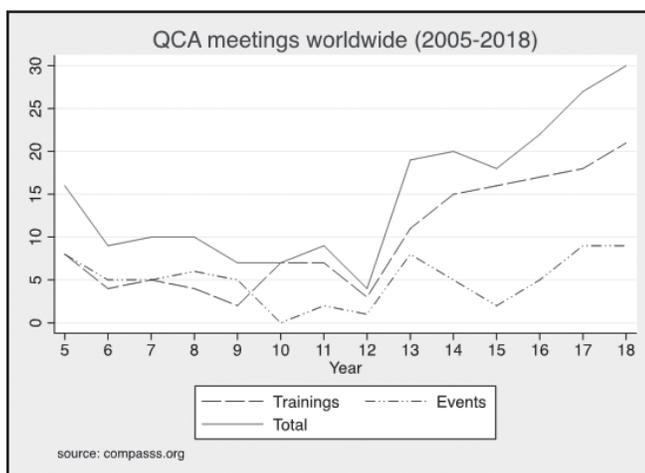
QCA has been a part of the IQMR's (Institute for Qualitative and Multi-Method Research) curriculum since the institute's inception in 2002 and, according to COMPASSS newsletters, the method was taught at ECPR already in 2005. Although the COMPASSS network makes a distinction between QCA trainings and QCA events, these two categories overlap considerably. The training category tends to describe highly institutionalized schools, such as the ECPR methods schools, while events usually indicate smaller workshops, colloquiums, and conferences.<sup>1</sup>

Graph 1 illustrates that QCA meetings worldwide gradually declined after 2005, only to increase dramatically after 2012. It is hard to explain why such a sharp change occurs. One compelling hypothesis is that the method is gaining popularity, especially since 2012. In 2013, there were 19 QCA-related meetings worldwide, and just five years later, in 2018, that number rose to 30. Between 2005 and 2018, a total of 208 QCA meetings took place (138 trainings and 70 events). In 2018, another milestone was achieved when QCA was incorporated into the ICPSR's schedule as a three- to five-day workshop (ICPSR 2018).

The discussion above is addressing the larger matter of the method's ongoing and progressing mainstreaming. QCA is no longer a periphery method, and its absorption into ICPSR's curriculum further validates this view. Currently, the approach is taught at leading methods institutes both in Europe and in America. QCA's institutionalization is, in turn, possible because of the methodological overlap it shares with mainstream statistical approaches. Consistent with the argument that formal logic underpins both quantitative and qualitative methodology, QCA is an example of a qualitative research method that has been embraced even by institutes committed to the promotion of quantitative approaches.

## Conclusion

Contemporary political science continues to be re-shaped by the increasingly popular mixed methods boom. One consequence of this development is that social scientists must reconsider what exactly separates quantitative and qualitative research, especially since the mixed methods wave presupposes the use of two or more approaches within a single research design. Methodological advice and new best mixed methods practices depend, therefore, on proper reconceptualization of the quantitative-qualitative divide. This article contributes to such a reconceptualization by extending the work of Goertz and Mahoney (2012) to argue that the gap between quantitative and qualitative research is not as large as previously assumed. In fact, this article stresses the importance of formal logic understood as an epistemological structure that unites both research traditions. Secondly, this article stresses the importance of mathematical superstructures. While both QCA and regression analysis are cross-sectional in their orientation, they make use of different types of mathematics and, therefore, serve different analytical purposes. Although both approaches are ultimately rooted in formal logic, their mathematical superstructures



Graph 1. QCA meetings worldwide

are not complementary, which means that they are suited to answer different types of questions.

Finally, the increased institutionalization of QCA demonstrates two related points. On the one hand, QCA is becoming more institutionalized and better known. By now the method is a part of major methodological institutes including the ECPR, the IQMR and the ICPSR. On the other hand, this increased incorporation

of QCA into these institutes demonstrates the point that formal logic can serve as the epistemological structure that scientific methods build on. QCA is an example of a qualitative approach that is now embraced by the quantitatively oriented ICPSR in large part because its methodological structure is similar enough to that of quantitative methods.

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# 2018 QMMR Section Awards

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## **Giovanni Sartori QMMR Book Award**

This award recognizes the best book, published in the calendar 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 Anna Grzymala-Busse (Stanford University), chair; Nicholas Weller (University of California, Riverside); and Zachariah Mampilly (Vassar College).

Winner of the 2018 Award: Alisha Holland. *Forbearance as Redistribution: The Politics of Informal Welfare*, Cambridge University Press, 2017.

Prize citation: *Forbearance as Redistribution* is a theoretically innovative, carefully argued, and meticulously documented book. Alisha Holland focuses on why states have laws that they choose not to enforce. She argues that the reason is not a lack of state capacity, but rather the decision not to enforce a law, and that this forbearance is a form of economic redistribution. Holland combines theoretically innovative arguments about the ways in which politicians deliberately choose to forego enforcing laws in order to gain votes, with a clear and skillful use of diverse kinds of evidence. She draws on a host of data including surveys, process tracing and interviews, content analysis, and experimental methods. Holland's treatment of this evidence is transparent and compelling: her case selection and research designs are clear and she documents her sources and how they were used. This is an excellent use of mixed methods, and an important contribution to the literatures on clientelism and policy discretion, electoral strategies, and economic development.

## **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 Nahomi Ichino (University of Michigan), chair; Seva Gunitsky (University of Toronto); and Carsten Schneider (Central European University).

Winner of the 2018 Award: Calla Hummel. “Disobedient Markets: Street Vendors, Enforcement, and State Intervention in Collective Action,” *Comparative Political Studies* 2017, Vol. 50(11) 1524-1555. DOI: 10.1177/0010414016679177

Prize citation: The committee is very pleased to award the 2018 Alexander George Award for the best journal article (or chapter in an edited volume that stands on its own as an article) developing and/or applying qualitative methods that was published in 2017 to Calla Hummel, for her work “Disobedient Markets: Street Vendors, Enforcement, and State Intervention in Collective Action,” published in *Comparative Political Studies*. This is an exemplary piece of research using mixed methods to advance new ideas about and empirical knowledge of a significant social phenomenon. Hummel starts with an important observation and question—informal workers make up half the global workforce, and contrary to what we might assume, given the challenges of collective action for people with limited resources, they are organized in many parts of the world. Hummel asks why is this the case. Why do informal workers organize in some places and not others? She formalizes her argument that the state provides incentives that help workers overcome barriers to collective action where it faces high enforcement costs. The core of the article is formed by her ethnographic observations of

street vendors, particularly cost-benefit calculations on whether to form an organization, in La Paz, Bolivia. Not only did she observe meetings of organizations, she was also a participant-observer attending to customers and doing other tasks with unorganized and organized vendors, including selling clothes once a week at a licensed stall. Moreover, Hummel presents results from an original survey that helps contextualize her own direct observations. The integration of all these components compellingly supports her argument to look at the role of the state to explain when informal workers organize. Bravo.

### **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 Alison Post (University of California, Berkeley), chair; Ryan Griffiths (Syracuse University); and Noah Nathan (University of Michigan).

Winner of the 2018 Award: Ana Catalano Weeks. "Why Are Gender Quota Laws Adopted by Men? The Role of Inter-and Intra-Party Competition."

Prize citation: We have unanimously chosen to award the 2018 Sage Best Paper prize to Ana Catalano Weeks for her submission, "Why Are Gender Quota Laws Adopted by Men? The Role of Inter-and Intra-Party Competition." This empirically focused paper addresses an increasingly prevalent, substantively important, and puzzling phenomenon: the adoption of gender quota laws in countries in which the vast majority of party elites and lawmakers are men. In the paper, she presents a compelling two-part argument for why male political elites would voluntarily restrict their own access to office. Parties will champion such laws when facing credible challengers from parties to the left. They will also be employed by party elites as a means of wresting control of candidate selection processes from entrenched local party elites. The empirical portion of the paper examines two paired comparisons of cases of adoption and non-adoption, highlighting the first and second mechanism respectively (Belgium and Austria, and Portugal and Italy).

The Catalano paper stood out from the rest of the pool for two reasons. First, the portion of the argument focusing on intra-party competition is compelling and

unusual. Intra-party contests for power—particularly party actors operating at different spatial scales—are often missed in cross-national quantitative and game theoretic studies. Second, we found the multi-method approach taken in the article to be both well suited to the research topic and extremely well executed. Employing process tracing in specific cases, as she does, makes sense given that the focus of the paper is strategic motivations of key participants. These motivations would be hard to uncover and code in a large-N setting. The paper also utilizes statistical matching to choose cases to compare with the main "positive" cases of quota adoption. While the approach has been advocated for a few years, Catalano's use of it is one of the first we have seen in empirical papers, and its implementation is presented with impressive clarity in the paper appendix. The paper also presents the interview data upon which it draws in an unusually transparent fashion, providing an appendix detailing whom was contacted, which interviews were accepted, and how interview data can be accessed. In all of these respects, the paper represents a model of clarity and transparency in multi-method research that we hope others will emulate.

### **David Collier Mid-Career Achievement Award**

This award 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.

Winner of the 2018 Award: Jason Seawright, Northwestern University.

Selection Committee: Melani Cammett (chair), Harvard University; Andrew Bennett, Georgetown University; Alan Jacobs, University of British Columbia; and Lauren Morris MacLean, Indiana University

Prize citation: Jason (Jay) Seawright has achieved distinction in all three areas honored by the award: publications on research methods, the innovative

application of qualitative and multi-methods techniques to substantive areas of research, and institution-building related to qualitative and multi-methods research. He has had a broad impact among scholars working across different traditions in the section and the discipline.

With regard to his work on research methods, Jay has made seminal contributions to the way that political scientists understand the contributions of qualitative and multi-methods research to descriptive and causal inference. His work in this area focuses on the distinctive value of qualitative methods to the discipline and to research designs employing multiple methods, showing what such work can and cannot contribute, and how to carry out rigorous qualitative work.

Jay has also contributed extensively to a foundational text in qualitative and multi-methods research—*Rethinking Social Inquiry*, for which he wrote a critical chapter on the distinction between dataset versus causal process observations, thereby helping to clarify for qualitative scholars the logic of within-case causal inference. His co-authored essay (with John Gerring) on “Case selection techniques in case study research” is an important reference and has been cited over 1500 times and counting. His own 2016 book—*Multi-Method Social Science: Combining Qualitative and Quantitative Tools*—is poised to become his most important work to date. In the book, he advocates for a unified approach to social science methods. The book is becoming an essential resource for anyone who engages in multi-method work and is generating much discussion, both among those who apply its approach, which is grounded in statistical theory, and others who advocate a distinct approach to qualitative and multi-method research. These are just a few examples of his published contributions to research methods in the social sciences.

Jay’s own substantive research, which largely but not exclusively centers on Latin America, has also made valuable contributions. In his 2012 book, *Party*

*System Collapse*, he explores the breakdown of political order in Peru and Venezuela, examining why voters abandoned traditional parties and why these parties could not respond adequately to the challenges they faced. Jay follows his own high methodological standards in developing and testing his arguments, resulting in a convincing and carefully argued book that takes causal complexity seriously.

Jay has a full research agenda that shows no signs of abating and continues to address important methodological and substantive issues. For example, in a current co-authored project, *Billionaires and Stealth Politics*, he and his collaborators focus on how the wealthiest Americans use their wealth to influence politics. Needless to say, this is a topic of enduring importance and one that is particularly urgent at this time.

Jay also contributes regularly and actively to institutions and programs devoted to qualitative and multi-methods research. For many years, he has taught a comprehensive course on multi-methods research at the IQMR, the APSA QMMR short course, and other methods training institutions in the US and abroad. He has been active in governance for this section, participating in multiple section committees over the years, and currently as president-elect.

In short, Jay is a unique recipient of the Collier award because he distinguishes himself in all areas celebrated by the award—the generation, application, and promotion of qualitative and multi-method research in the discipline.

