

# 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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<sup>4</sup> 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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