

Qualitative & Multi-Method Research

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Letter from the Editors

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We are delighted to present this double issue of Qualitative and Multi-Method Research. We open with an essay by John Gerring and Lee Cojocaru on an issue with which most qualitative and multi-method researchers have surely wrestled. Based on a systematic review of journal practices, Gerring and Cojocaru argue that “arbitrary” word or page limits have profound and deleterious effects on how and where research results are communicated and on the topics we choose to study. They propose that journals thus abolish “one-size-fits-all word limits,” allowing every article to be “as long as it needs to be.”

Our first symposium unpacks the theoretical underpinnings of process tracing. While process tracing is often understood as inquiry into causal mechanisms, there is much ambiguity about what mechanisms are and how we should study them. Derek Beach, Andrew Bennett, Rosa Runhardt, and David Waldner tackle these issues, each advancing a specific understanding of mechanisms and deriving implications for qualitative strategies of causal inference.

In our second symposium, Lahra Smith, Gary Goertz, and Patrick Thaddeus Jackson discuss Frederic Schaffer's *Elucidating Social Science Concepts: An Interpretivist Guide*. Schaffer takes up their constructive criticisms to suggest further ways of advancing our understanding of concepts.

For future issues of *QMMR*, we invite proposals for articles and symposia. We especially encourage proposals of broad interest for *QMMR* scholars such as for symposia about methodological approaches or problems that are currently animating debate or shaping research practice in large segments of the discipline. We look forward to hearing from you.

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Arbitrary Limits to Scholarly Speech: Why (Short) Word Limits Should Be Abolished

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Most journals in political science and sociology set stringent word or page limits, a fact of which every author is keenly aware. By all reports, researchers expend a good deal of effort trying to work within these limits. This might involve revising successive drafts until the final version slips just under the ceiling, moving sections of a paper into online appendices, splitting up a subject into “minimal publishing units,” shopping around for a publication venue with less stringent limits, or trying to negotiate special terms with an editor.

Some researchers relinquish the goal of journal publication entirely in preference for the more relaxed format of an academic monograph. This option, however, is less and less viable as university presses trim their lists and reorient priorities toward books with a popular theme and a potential cross-over audience.

In sum, limits on the length of journal articles affect scholarly research in all sorts of ways, some more visible than others. Some researchers, we must presume, avoid projects entirely if they seem intractable in a journal format.

Our contention is that current policies that impose arbitrary word or page limits on published articles are not serving the discipline well. They contort the academic process in ways that are not conducive to scholarly research or to communication. And they waste everyone’s valuable time.

We begin by surveying the policies of top political science and sociology journals. In the second section, we lay out a proposal that we suppose will not be very controversial: journals should clearly state their policies vis-à-vis length requirements and adhere to those policies. In the third section, we lay out our more controversial proposal, that journals should abolish—or at least greatly loosen—length limits. The rest of the article elaborates and defends that proposal. We discuss (a) heterogeneity across venues (different journals offering different policies), (b) supplementary material posted online,

John Gerring is Professor of Government at The University of Texas at Austin. He can be reached at jgerring@austin.utexas.edu. Lee Cojocar is a Research Assistant with the Varieties of Democracy (V-Dem) Project, University of Oslo. He can be reached at cojocar@bu.edu. Comments and suggestions on various drafts were provided by Taylor Boas, Colin Elman, Alan Jacobs, Carl Henrik Knutsen, and Evan Lieberman. A longer version of this paper, integrating work in economics and the natural sciences, where some of these issues have been explored, will appear in an edited volume, tentatively titled “The Production of Knowledge: Enhancing Progress in Social Science.” All online appendices (A, B, and C) can be accessed at <https://utexas.app.box.com/s/fmybgvn70w78c5yn8ixwhg8ssv3z0pyv>.

(c) references (often the first aspect of an article to be cut down in order to meet a length requirement), (d) the role of length limitations in structuring the work of political science, (e) word limits in economics (where we find journal policies to be considerably more permissive), (f) the correlation between article length and impact, and (g) the ramifications of a change of journal length limit policies for journal business models.

Survey

Despite its importance, no comprehensive survey of word or page limits has ever been conducted. To remedy this omission, and to set the stage for our argument, policies and practices across top journals in political science and sociology are summarized in Tables 1 and 2. Information is drawn directly from journal web pages (instructions to authors)—supplemented, in some cases, by direct communication with editors. Journal policies are quoted verbatim in online Appendices A and B.

Comparisons across journals must be inexact inasmuch as they follow different protocols. Some count words and others pages. Some count abstracts, references, tables, figures, and footnotes while others do not (or only count some of them). Some apply limits at the submission stage and others wait for final approval.

Here, we adopt a few standard criteria in order to provide a (more or less) systematic comparison of journal policies based on stated guidelines posted on journal web pages. Length is counted with words, as this is the usual practice in political science and is more exact than pagination. Where limits are counted in pages, we list the journal policy (in pages) and then convert pagination to word counts following journal guidelines with respect to margins and font, as noted in column 2 of Table 1. We assume that online materials (generally in the form of appendices) are not considered in the word count. However, some journals exempt references and/or appendices in the word count even if they appear as part of the published article, as noted in columns 3-4. We also note whether the word count is applied at submission or later (column 5) and whether, according to the stated policy of the journal, editors are allowed some discretion in applying the rules (column 6).

Twenty of the most influential journals from each discipline are included in this survey. For gauges of impact in political science we rely on two sources: SCImago journal rank (Elsevier) and Science Watch (Thomsen Reuters).¹ Chosen journals include *American Journal of Political Science*, *American Political Science Review*, *Annual Review of Political Science*, *British Journal of Political Science*, *Comparative Political Studies*, *Conflict Management and Peace Science*, *European Journal of Political Research*, *International Security*, *International Studies Quarterly*, *Journal of Conflict Resolution*, *Journal of Peace Research*, *Journal of Politics*, *Journal of Public Administration Research & Theory*, *Party Politics*, *Political Analysis*, *Political Communication*, *Political Geography*, *Political Psychology*, *Public Opinion Quarterly*, and *World Politics*.

¹ See <http://www.scimagojr.com/journalrank.php?category=3312> and http://archive.sciencewatch.com/dr/sci/09/mar29-09_1/.

Table 1: Political Science Journals

Journal	Policies					Practices			Consistency	
	Limit	Including References	Including Appendices	At Submission	Editor's Discretion	Mean	Min	Max	Actual Max	Column 10-Column 2
1	2	3	4	5	6	7	8	9	10	11
<i>American Journal of Political Science</i>	10,000	Yes	No	Yes	Yes	10,156	7,558	14,294	14,294	4,294
<i>American Political Science Review</i>	12,000	Yes	Yes	Yes	Yes	13,361	10,990	16,593	16,593	4,593
<i>Annual Review of Political Science</i>	7,800 (24)	No	No	Yes	Yes	8,522	5,799	14,467	12,525	4,725
<i>British Journal of Political Science</i>	12,000	Yes	Yes	No	Yes	12,048	7,830	23,596	23,596	11,596
<i>Comparative Political Studies</i>	12,000	Yes	Yes	Yes	Yes	11,597	8,320	15,890	15,890	3,890
<i>Conflict Management and Peace Science</i>	10,000	Yes	No	No	Yes	10,239	4,574	13,155	13,155	3,155
<i>European Journal of Political Research</i>	8,000	Yes	No	Yes	Yes	8,789	5,916	11,039	10,127	2,127
<i>International Security</i>	20,000	Yes	No	No	Yes	17,123	10,803	20,215	20,215	215
<i>International Studies Quarterly</i>	12,000	Yes	Yes	Yes	Yes	11,934	8,440	15,840	15,840	3,840
<i>Journal of Conflict Resolution</i>	11,000	Yes	Yes	Yes	Yes	10,570	6,870	14,201	14,201	3,201
<i>Journal of Peace Research</i>	10,000	Yes	Yes	Yes	No	9,382	7,445	10,417	10,417	417
<i>Journal of Politics</i>	10,250 (35)	Yes	No	Yes	Yes	9,948	6,917	17,754	12,720	2,470
<i>J. of Public Admin. Research & Theory**</i>	12,000	Yes	No	Yes	Yes	11,610	5,298	16,091	15,284	3,284
<i>Party Politics</i>	8,000	Yes	Yes	Yes	Yes	8,103	4,553	14,788	14,788	6,788
<i>Political Analysis</i>	8,750 (30)	Yes	No	No	Yes	8,570	5,766	15,371	14,706	5,956
<i>Political Communication</i>	8,750 (30)	Yes	No	No	Yes	8,684	5,039	15,890	14,654	5,904
<i>Political Geography</i>	11,000	Yes	Yes	Yes	Yes	10,821	9,372	14,891	14,891	3,891
<i>Political Psychology</i>	9,000	Yes	Yes	Yes	Yes	9,253	7,776	13,014	13,014	4,014
<i>Public Opinion Quarterly</i>	6,500	No	No	Yes	Yes	8,520	4,160	17,775	15,451	8,951
<i>World Politics</i>	12,500	Yes	No	Yes	Yes	13,324	11,282	18,531	18,531	6,031
<i>Mean or Distribution (Yes/No)</i>	10,578	17/3	9/11	15/5	19/1	10,628	7,235	15,691	15,045	4,467

Journal = top 20 political science journals by impact (<http://www.scimagojr.com/journalrank.php?category=3312>; http://archive.sciencewatch.com/dr/sci/09/mar29-09_1/). **Limit** = ceiling on number of words (pages) allowed in research articles, as specified on journal web pages. Where the limit is specified in pages we list the projected word limit based on the specified page limit with double-spaced lines and standard 12-point font. **At submission** = limits apply at submission. **Editor's discretion** = longer versions may be accepted with editor's approval. For further clarification of journal policies see online Appendix A. **Practices** = mean, minimum, maximum word counts across all research articles and including all published material (footnotes, references, appendices, et al.). **Actual maximum** = maximum word count, including or excluding references and appendices as specified by the journal's policies. All journals observed in the calendar year 2015 except those marked with a double asterisk (**), which are observed in 2014.

For a gauge of impact in sociology we rely on the Google Scholar (scholar.google.com) H5 index. Chosen journals include *American Journal of Sociology*, *American Sociological Review*, *Annual Review of Sociology*, *Antipode*, *British Journal of Criminology*, *British Journal of Sociology*, *Criminology*, *Demography*, *Ethnic and Racial Studies*, *European Sociological Review*, *Journal of Ethnic and Migration Studies*, *Journal of European Social Policy*, *Journal of Marriage and Family*, *Journal of Population Economics*, *Population and Development Review*, *Qualitative Research*, *Social Science Research*, *Social Forces*, *Sociology*, *Theory*, and *Culture & Society*.

All political science journals impose space limits, as shown in Table 1.² The tightest limit—6,500 words (not including references or appendices)—is adopted by *Public Opinion Quarterly*. The most capacious limit—20,000 words—is allowed by *International Security*. Most hover between 8,000 and 12,000 words, with a mean of just over 10,500. All journals except the

² We are aware of one influential journal—the newly founded *Quarterly Journal of Political Science*—which does not impose a length limit, perhaps on the model of economics (see discussion below). This journal did not meet our threshold of a “top” journal according to the chosen sources of journal rankings, however.

Table 2: Sociology Journals

Journal	Policies					Practices			Consistency	
	Limit	Including References	Including Appendices	At Submission	Editor's Discretion	Mean	Min	Max	Actual Max	Column 10 - Column 2
1	2	3	4	5	6	7	8	9	10	11
<i>American Journal of Sociology</i>	[none]					19,718	12,457	28,482	-	-
<i>American Sociological Review</i>	15,000	Yes	No	Yes	Yes	13,761	10,073	18,972	18,972	3,972
<i>Annual Review of Sociology</i>	7,800 (24)	No	No	Yes	Yes	12,395	9,455	17,634	11,729	3,929
<i>Antipode</i>	9,500	Yes	No	Yes	Yes	9,982	7,923	12,597	12,597	3,097
<i>British Journal of Criminology</i>	10,000	Yes	Yes	Yes	Yes	10,080	7,511	12,160	12,160	2,160
<i>British Journal of Criminology</i>	8,000	Yes	No	Yes	Yes	9,423	8,018	11,810	11,810	3,810
<i>Demography</i>	[none]					13,125	9,296	19,063	-	-
<i>Ethnic and Racial Studies</i>	8,000	No	No	Yes	Yes	11,036	4,258	15,612	12,034	4,034
<i>Ethnic and Racial Studies</i>	8,000	Yes	No	Yes	No	7,626	6,272	8,247	8,247	247
<i>European Sociological Review</i>	7,000	Yes	No	Yes	No	6,694	5,278	9,254	7,392	392
<i>Journal of Ethnic and Migration Studies</i>	9,000	Yes	No	Yes	Yes	9,606	7,189	12,589	10,467	1,467
<i>Journal of European Social Policy</i>	[none]					8,741	3,635	12,331	-	-
<i>Journal of Marriage and Family</i>	10,250 (35)	Yes	Yes	Yes	Yes	9,853	4,127	14,534	14,534	2,034
<i>Journal of Population Economics</i>	[none]					12,356	5,975	18,214	-	-
<i>Population and Development Review</i>	[none]					9,547	6,026	15,907	-	-
<i>Qualitative Research</i>	8,000	Yes	Yes	Yes	Yes	8,747	6,742	12,946	12,946	4,946
<i>Social Science Research</i>	[none]					13,376	4,364	19,921	-	-
<i>Social Forces</i>	10,000	Yes	No	Yes	Yes	10,160	7,107	12,899	12,793	2,793
<i>Sociology</i>	8,000	Yes	Yes	Yes	No	8,166	7,431	8,504	8,504	504
<i>Theory, Culture & Society</i>	8,000	Yes	Yes	Yes	Yes	9,613	5,531	14,203	14,203	6,203
<i>Mean or Distribution (Yes/No)</i>	9,039	12/2	5/9	14/0	11/3	10,700	6,933	14,794	12,028	2,828

Journal = top 20 sociology journals by impact: https://scholar.google.com/citations?view_op=top_venues&hl=en&vq=soc_sociology. **Limit** = ceiling on number of words (pages) allowed in research articles, as specified on journal web pages. Where the limit is specified in pages we list the projected word limit based on the specified page limit with double-spaced lines and standard 12-point font. **At submission** = limits apply at submission. **Editor's discretion** = longer versions may be accepted with editor's approval. For further clarification of journal policies see online Appendix B. **Practices** = mean, minimum, maximum word counts across all research articles and including all published material (footnotes, references, appendices, et al.). **Actual maximum** = maximum word count, including or excluding references and appendices as specified by the journal's policies. All journals observed in the calendar year 2015.

Journal of Peace Research allow editorial discretion in the application of word limits.

In sociology, journal practices are somewhat more relaxed. Six journals—*American Journal of Sociology*, *Criminology*, *Journal of European Social Policy*, *Journal of Population Economics*, *Population and Development Review*, *Social Science Research*—impose no formal limits. Among those that impose limits, the range extends from about 8,000 to 15,000,

with a mean of about 9,000. Most journals allow editorial discretion in the policing of these limits.

The second section of Tables 1-2 focuses on journal practices, i.e., how these length limits are administered. We report mean, minimum, and maximum word counts of all articles published in 2015 (or where unavailable, in 2014), as noted in columns 7-9. Here, we include only regular, full-length articles, as defined by the journal. For example, if a journal has a separate

section for research notes, methodology notes, or reviews, these publications are excluded. To determine mean length, we record page lengths for all articles published in a year, calculate the mean (in pages), locate an article with that (approximate) length, place the contents of that article (all aspects of the article—text, abstract, footnotes, references, appendices, tables, figures—so long as it appears in the journal itself rather than in an online appendix) into a Word document, and record the number of words. To calculate minimum and maximum length we use page length to identify the longest and shortest articles and then place the contents of those articles (all aspects, as published) into a Word file to record the number of words.

We find that the mean length of articles is close to the stated word limit for most journals in political science and sociology (~10,000), and there is considerable spread from minimum (~7,000) to maximum (~15,000). Recorded word counts in practice are remarkably similar across the two disciplines.

The final section of Tables 1 and 2 focuses on consistency between policies and practices for those journals with an official limitation on length. Column 10 records the “actual maximum,” the highest word count of any article published by that journal within the year, including only those elements of an article that are considered relevant to calculating length according to the journal’s policies. For example, if the journal excludes references from the limit, the actual maximum does so as well. Column 11 compares the actual maximum with the official word limit, subtracting one from the other. Results are explored in the following section.

First Proposal: Clarity and Consistency

In comparing policies with practices, we find strong correspondence between the stated limits and the mean length of articles. Comparing columns 1 and 7, only a few journals—notably *International Security* and *Public Opinion Quarterly* in political science and *Annual Review of Sociology* and *Demography* in sociology—have mean lengths that greatly surpass their official word limits, and this could be partly accounted for by our method of counting, which includes all article content (even that which is not included in a journal’s assessment of word limits).

However, when comparing maximum (actual) lengths with stated limits we find considerable divergence, at least for certain journals, as shown in the final column of Tables 1 and 2. The average difference is nearly 5,000 words in political science. That is, across the twenty top political science journals, the longest article published in a year (usually, 2015) in that journal surpassed these journals’ formal limits by an average of just under 5,000 words. One journal, the *British Journal of Political Science*, published an article that is more than 11,000 words over the stated limit. And only one journal, the *Journal of Peace Research*, appears to strictly abide by their word limits (not coincidentally, it is only journal that does not allow editorial discretion). Differences between stated policies and practices are noticeable in sociology as well, though not as glaring.

To be sure, most journals allow editorial discretion in the

application of length limits. In this sense, they are not violating their own policies. However, length limits are described on journal web pages as if they were strictly applied. Authors without experience with a specific journal—or prior correspondence with the editor—would have no way of knowing that they might publish an article of 15,000 words in a journal with a 10,000 word limit.

This inconsistency between de jure and de facto policies is problematic in several respects. Authors are unsure about how to craft their work in order to meet the journal’s guidelines. They do not know whether the word limit will be observed and, if not, how much leeway might be allowed. Likewise, senior faculty, who have greater experience, and perhaps know the editors personally, can muster inside information to successfully walk this tightrope.

Our first proposal will surprise no one. If wide discretion in word limits is allowed then this policy should be clearly stated on the journal’s web page. Authors should not be required to second-guess this important issue. Our analysis suggests that most journal word limits in political science should be understood as *targets*, not ceilings. Note that the mean number of words in published articles aligns closely with journal word limits, with considerable dispersion about the mean. A simple change of terminology would solve this problem. Editors could change word (or page) *limit* to word (or page) *target* and disable web pages (e.g., for the *American Political Science Review*) that automatically disqualify submissions that violate the target.

While a great deal of effort has gone into enhancing the transparency of journal content (i.e., articles) in recent years, e.g., via the DA*RT initiative, it is equally important that journal policies be transparent. This seems like an easy reform.

Second Proposal: No More (Tight) Limits

Our second, more controversial, proposal is that journals should abolish arbitrary, one-size-fits-all word limits, or greatly expand those limits. The argument for this proposal may be concisely stated. *An article, like a book or any other written product, should be as long as it needs to be—no longer, and no shorter.*

Some articles are *over-written*. There is only one basic point and it is repeated ad infinitum. Or there is a set of empirical tests that so closely resemble each other as to be redundant; they belong in an appendix or perhaps are entirely unnecessary. Nonetheless, the author feels compelled to fill up the allocated space.

Articles in top natural science journals (e.g., *Nature*, *Science*) are typically much shorter than those that appear in social science journals. While we do not think this format generally serves social science well, we should be mindful that some points can be made with brevity, and this should not take away from their importance or their impact. In political science and sociology, short papers are often relegated to “research notes,” simply because of their brevity. As a consequence of this classification, they are not taken very seriously and do not count for very much (re: promotion and tenure). This sort

of classification by size seems just as arbitrary as the exclusion of longer papers that surpass word limits.

Some articles are *under-written*. The author has a very large and complex argument to make, or an extended set of (non-redundant) empirical exercises, many contexts to explore, or many styles of evidence to incorporate. However, under the rigid word limits assigned by the journal, all that appears in the main text is the outline of a story, from which one can glean little about the truth of the author's argument. Here, word limits constitute a Procrustean bed.

To clarify, our argument is not for longer journal articles. Our argument is for the removal of arbitrary space constraints that have nothing to do with the content of a submission. Length should be adapted to the paper under review. Some topics can be dispensed with in 2,000 words. Others may require 20,000, or even 30,000. As such, length should be a minor feature of the review process, along with other stylistic concerns (not to mention content). Journals do not mandate that authors present 3 tables and 1 figure. This would be patently absurd. We should not mandate that they present 10,000 words.

Thus, we are not making an argument for endless babble. Some authors need to be restrained from diarrhea of the keyboard. Other authors are terse to the point of obscurantism, and need to be drawn out ("please give a few examples of what you are talking about"). But one argument about length that does not seem admissible, if we are concerned with such things as truth and its dissemination, is that an article fit within an arbitrary (short) word limit. Journals cannot possibly reduce academic research to a formula because articles are not all alike.

We are reminded of the first question we always get from students after distributing a writing assignment. "How many pages?," they ask. Most students are concerned with the minimal number of pages they will need to generate in order to pass the assignment. A few are concerned with the maximum. To both concerns we reply with a set of bounds intended to be advisory—e.g., "10-20 pages"—followed by the admonition not to get caught up in the number of pages but rather with the quality of the work they are producing. The number of pages or words is the *least* important aspect of your paper, we tell them. Unfortunately, we are not following this advice in academic publishing.

Heterogeneity across Venues

A few political science journals, which did not make it onto our list in Table 1, look favorably upon longer submissions. This includes *International Security* (20,000 words), *Studies in American Political Development* (no official limit), and the *Quarterly Journal of Political Science* (no official limit). There may be others of which we are not aware. By the same token, some journals have even tighter space restrictions than those listed in Table 1. For example, the newly founded *Journal of Experimental Political Science* requests papers of "approximately 2,500 words."

Evidently, there is some degree of heterogeneity across journals, and even more so in sociology, as noted in Table 2.

This heterogeneity may increase over time, if divergence rather than convergence is the overall trend within the discipline. Authors can thus shop around for an appropriate forum for their paper, as, to some extent, they do now. Supply and demand would then intersect. This seems like it might offer a happy resolution of our problem, with flexibility provided across journals (rather than across articles within the same journal).

This model of diversity fits the consumer-driven model of the commercial publishing business. Readers looking for a discursive treatment of a contemporary subject can turn to the *New York Review of Books* or the *New Yorker*. Readers looking for the quick-and-dirty might turn to a newspaper, a blog, or a publication known for terseness such as the *Economist*. Fiction readers may look for long books, short books, or short stories. They are free to choose. By all accounts, length is an important consideration in consumer choice in the commercial marketplace.

Likewise, in the world of social science the choice to read a journal article rather than a book is, at least to some extent, a choice about length. So, one might argue that journal heterogeneity in length requirements is merely a continuation of a spectrum that stretches from academic monographs to paragraph-sized blogs, or even Tweets.

Unfortunately, journal specialization by length is inappropriate for academic journals. The reason, in brief, is that journals do not have overlapping purviews and functions. Because mass market publications like *NYRB*, *New Yorker*, the *Economist*, and book publishers cater to the same sort of readers and cover (pretty much) the same sorts of things, readers may choose the format they wish—short, medium, or long. This does not obviate the tradeoff—conciseness versus depth—but it means that readers can make choices based on their priorities.

However, journals do not offer multiple options. Indeed, they are in the business of avoiding redundancy. Un-original content is excluded from consideration. Moreover, journals tend to specialize in a particular field or subfield. There is no space in the academic journal market for two journals focused on the same topic—one of which publishes long articles and the other of which publishes short articles.

Only general-interest journals (e.g., the *American Political Science Review* or the *American Journal of Sociology*) have overlapping purviews. Here, one might envision a division of labor in which some specialize in long articles and others in short articles. This would be productive in all respects except one: differentiation by space allotment would interfere with an important function of top journals—differentiation by quality. Insofar as scholars wish to maintain a clear ranking of journals (and, all protests to the contrary, it seems that they do) space-constraints should not obstruct that goal.

To conclude, heterogeneity across journals does not solve the problem. Indeed, this scenario seems about as defensible as a scenario in which some journals publish authors whose names begin with consonants and others publish authors whose names begin with vowels. Publication decisions should

hinge on matters of topicality and quality, not size.

Online Supplementary Material

In recent years, the practice of posting supplementary material online has become more common, and readers may wonder if this solves the problem we are posing. Unfortunately, while online appendices are surely an improvement over the pre-WWW era, they are not ideal.

Appendices often contain information that is vital to the review process. Sometimes, they appear at the insistence of reviewers or editors. This suggests that anyone seeking to make sense of the argument of a paper would need to access the appendix (and that it should remain in stable form, post-publication). Yet, if the appendix is posted separately those who read or cite an article will feel under no compunction to read it. Such material is not part of the formal record, occupying a nebulous zone. A citation to “Sullivan (1998)” does not imply “and online appendices.” Online material is sometimes hard to locate and in any case usually ignored. For this reason, online appendices sometimes serve as a place to stow away evidence that does not fit neatly with the author’s main argument. Note also that if the online appendix is under the author’s control it is susceptible to post-publication manipulation.

For all these reasons it seems essential that appendices be published along with the main text of an article. Moreover, decisions about what material to place within the main text and what to place in appendices should be driven by matters other than arbitrary space constraints. There is nothing sillier than moving text from one place to another simply to get under a 10,000-word limit. (“I put it in the Appendix because I ran out of space in the text.”) This sort of shenanigan damages the stylistic coherence of an article, not to mention the time it imposes on the author, editor, and reviewers (who must check up on such things). Note also that when an appendix appears online the distinction between main text and appendix is highly consequential—something that editors need to scrutinize closely. By contrast, if an appendix is easily accessible and part of the published version of an article, this decision is not so fundamental.

The same general point applies to other decisions that are often made under pressure from arbitrary word limits, e.g., whether to cite additional work, to address counterarguments, to provide examples, or to provide clarification of a theory or method. Authors face many decisions about content and composition, and each deserves careful consideration. Writing social science is not a paint-by-numbers exercise. In searching for the right resolution of these questions one consideration that does not seem relevant is an arbitrary word limit. And one must not lose sight of the time required to re-shuffle words and ideas until the proper quantity is obtained. Researchers’ time is valuable and should not be wasted in a trivial quest for magic word counts.

References

A few journals (e.g., the *Annual Review of Political Science* and *Public Opinion Quarterly*) do not include references in

their wordcount. But most do (see Table 1). Because references are of little concern to most authors and reviewers (unless it is their work that is being cited, naturally), and because references consume a lot of words (for each citation there is usually a two-line reference), they are usually the first to be sacrificed when an author has to shorten a piece to satisfy a length limitation. For this reason, it is worth pondering the value of references.

Recent work by Patrick Dunleavy³ suggests that citations to the literature on a subject are essential for providing a basis for evaluation, showing how the present study fits in with an existing body of work. If that body of work is not fully represented, cumulation is impeded. A study must be understood within a context, and that context is provided by the citations. If past findings on a subject are not cited, cumulation is impossible.⁴

Second, anyone attempting to come to grips with a new area of study must be able to follow a trail of citations in order to piece together who has done what on a given subject. The intellectual history of a subject is located in the citations.

Third, we must consider the problem of academic honesty. We are acutely aware of the problem of plagiarism, when someone’s ideas (uncited) are stolen. A problem that receives less attention—but, arguably, is much more prevalent—is when prior studies of a subject are not cited, or only briefly cited, leaving readers unaware of how novel—or derivative—the author’s theory and findings really are.

Fourth, we might want to consider whether dropped citations are chosen in a biased fashion. Studies suggest that citations are often biased toward prestige journals⁵ and toward authors who are well established, senior, male,⁶ or at top universities and departments located in the United States and Europe.⁷ Commonsense suggests that these biases may be exacerbated in situations where space is in short supply. Here, authors are likely to favor the most prominent writer or work on a subject—the “obligatory” reference.

Finally, we should consider the role of citations in measuring impact. Nowadays, citation counts are critical for the evaluation of scholarship at all levels. An article’s impact is understood by the number of citations it receives. Journal impact is measured by the number of citations all the articles published in that journal receive. Author impact is measured by the number of citations all their publications receive. And the impact of fields and disciplines is understood according to how many citations they receive. It follows that when articles are incompletely referenced our ability to properly assess impact—of articles, journals, authors, subfields, or disciplines (at large)—

³ Dunleavy 2014. See also Bastow, Dunleavy and Tinkler 2014.

⁴ Gans 1992.

⁵ Callaham, Wears and Weber 2002; Nosek and Bar-Anan 2012: 219.

⁶ Larivière et al. 2013; Maliniak, Powers, and Walter 2013.

⁷ Basu 2006. We regard these selection factors as elements of potential bias since none of them—with the possible exception of journal ranking—is directly indicative of the quality and relevance of the cited work.

is impaired. We may be able to trace the impact of “obligatory” references, but we cannot trace the impact of other work that may have affected the development of thinking on a subject.

Right-sizing the Discipline

The most serious cost imposed by word limits is not the author’s time. Nor is it the published articles that are too long or too short, those that make use of online appendices to get around arbitrary word limits, those that omit important citations, or those that are stylistically flawed because the text is playing limbo with the journal’s word count. These are fairly trivial costs. The most serious cost arises from the way in which the word count protocol structures the work of social science.

We shall assume that, in our highly professionalized discipline, researchers are sensitive to incentives. Since the main incentive is to publish, and since journals are increasingly the most prestigious outlets for publication (surpassing books, at least for most subfields), we must consider what sort of research this regime encourages, and discourages. Substance is inevitably structured by form. And when the form is rigidly fixed, the substance must accommodate itself.

Smart academics choose topics and research designs that fit the space-constrained format of the journals they wish to publish in. Since all journals impose word limits, and there is not a great deal of variation in these limits—leaving aside a few journals, as noted above—shopping around does not afford much leeway.

Under the circumstances, success in the business of academic publishing involves finding bite-sized topics that can be dispatched with 8 to 12,000 words. Qualitative work is at a disadvantage since evidence drawn from archival, ethnographic, or interview-based research normally requires a good deal of verbiage to adequately convey the nuances of the argument, e.g., the many bits and pieces of evidence that, together, contribute to a causal inference. Multi-method work is at an even more severe disadvantage since it must practice two trades—two separate research designs—in order to fulfill its mission. Work that embraces a large theoretical framework, with many empirical implications, is at a disadvantage. Work that applies a theory to multiple contexts is at a disadvantage. Historical work, which often involves both qualitative and quantitative evidence, is at a disadvantage. Research designs that fall far from the experimental ideal, and therefore involve a great deal of supporting argumentation and robustness tests, are at a disadvantage.⁸

Insofar as scholars are rational they will pause before undertaking such ventures, or will divide them up into separate pieces—“minimal publishing units”—that fit the space-constrained format of journal publication at the cost of redundancy (since the evidence for a large argument is divided up

⁸ We recognize that experimental research may also involve a good deal of supporting argumentation and robustness tests. But we assume that the burden carried by this sort of theoretical and empirical work is even greater when the data is observational, thus requiring more space for elaboration and demonstration.

across multiple publications). But our biggest concern should be about articles that never get written, or, if written (in a fit of vainglory), never get published.

Economics

At this point, it may be appropriate to consider our field in relation to our social science cousins on the “hard” (naturalist) end of the spectrum. In Table 3, we survey the space limitation policies of 20 top journals in economics.

For estimations of scholarly impact we rely on SCImago.⁹ Chosen journals include *American Economic Journal (AEJ): Applied Economics*, *AEJ: Economic Policy*, *AEJ: Macroeconomics*, *AEJ: Microeconomics*, *American Economic Review*, *Annual Review of Economics*, *Brookings Papers on Economic Activity*, *Econometrica*, *Economic Journal*, *Journal of Economic Literature*, *Journal of European Economic Association*, *Journal of Finance*, *Journal of Management*, *Journal of Marketing*, *Journal of Political Economy*, *Quarterly Journal of Economics*, *Review of Economic Studies*, *Review of Economics and Statistics*, *Review of Financial Economics*, and *Review of Financial Studies*.

Table 3 reveals that economics journals have a considerably more relaxed set of policies with respect to article length than political science and sociology journals. This is signaled by the calculation of length in pages rather than words, for most journals. Six journals have no official limit on article length. Among the remainder, the average limit is just over 15,000 words. Only one journal, *Economic Journal*, has a tight limit—in this case, 7,500 words. However, we find that the average length of an article in that journal is well over 12,000 words and one article published in 2015 included over 21,000 words. So this does not constitute much of an exception from the industry norm of overall permissiveness with respect to article length.

As with political science and sociology journals, practices often depart from policies. The actual maximum length is 7,000+ over the stated limit. This suggests that in economics, as in other fields, limits are not strictly applied. And this, in turn, suggests a problem of transparency.

Impact

Thus far, the gist of our argument is that by removing an arbitrary component of the publication process—article length—we will improve efficiency (spending less time worrying about limits and strategizing about how to get around them) and also arrive at higher-quality articles. Can the latter proposition be tested?

In one sort of hypothetical experiment, article length would be arbitrarily assigned. Conceivably, one might enlist a journal that takes a relaxed attitude toward word limits. Submissions that surpass a given threshold (e.g., 15,000 words) and pass the review process (in that form) would then be randomized into a control group (no change) and a treatment group (subjected to a word limit of 10,000 words). Compliance (not to mention ethics) would be difficult. Authors would need to com-

⁹ See www.scimagojr.com/journalrank.php?area=2000

Table 3: Economics Journals

Journal	Policies					Practices			Consistency	
	Limit	Including References	Including Appendices	At Submission	Editor's Discretion	Mean	Min	Max	Actual Max	Column 10-Column 2
1	2	3	4	5	6	7	8	9	10	11
<i>AEJ: Applied Economics</i>	15,750 (50)	Yes	No	Yes	Yes	12,989	5,581	20,125	17,293	1,543
<i>AEJ: Economic Policy</i>	15,750 (50)	Yes	No	Yes	Yes	14,923	8,978	25,318	20,026	4,276
<i>AEJ: Macroeconomics</i>	15,750 (50)	Yes	No	Yes	Yes	16,267	8,877	28,263	27,000	11,250
<i>AEJ: Microeconomics</i>	15,750 (50)	Yes	No	Yes	Yes	16,595	5,218	26,791	22,499	6,749
<i>American Economic Review</i>	15,750 (50)	Yes	No	Yes	Yes	17,183	8,305	24,099	23,545	7,795
<i>Annual Review of Economics</i>	[none]					13,836	7,897	19,560		
<i>Brookings Papers on Economic Activity</i> **	[none]					18,084	15,723	21,846		
<i>Econometrica</i>	17,000	Yes	No	Yes	Yes	15,998	6,589	25,139	21,042	4,042
<i>Economic Journal</i>	7,500	Yes	Yes	Yes	No	12,697	4,157	21,894	21,894	14,394
<i>Journal of Economic Literature</i>	25,000 (100)†	Yes	Yes	No	Yes	15,492	4,625	35,347	35,347	10,347
<i>Journal of European Economic Association</i>	[none]					13,690	4,633	28,919		
<i>Journal of Finance</i>	22,000 (60*)	Yes	No	Yes	Yes	14,834	7,253	23,650	23,650	1,650
<i>Journal of Management</i>	15,750 (50)	Yes	No	Yes	Yes	13,178	8,670	19,661	19,661	3,911
<i>Journal of Marketing</i>	15,750 (50)	Yes	No	Yes	No	14,247	6,490	18,718	18,718	2,968
<i>Journal of Political Economy</i> **	12,000 (40)	Yes	Yes	No	Yes	17,872	8,649	28,892	28,892	16,892
<i>Quarterly Journal of Economics</i>	15,750 (50)†	Yes	Yes	No	Yes	15,587	11,403	23,893	23,893	8,143
<i>Review of Economic Studies</i>	[none]					17,104	8,661	23,514		
<i>Review of Economics and Statistics</i>	12,500 (45)†	Yes	No	No	Yes	13,053	6,753	21,609	20,114	7,614
<i>Review of Financial Economics</i>	[none]					13,972	7,324	19,787		
<i>Review of Financial Studies</i>	[none]					14,936	9,218	24,878		
<i>Mean or Distribution (Yes/No)</i>	15,386	14/0	4/10	10/4	12/2	15,127	7,750	24,095	23,112	7,255

†=There is no official limit but the editors have communicated (to us) the unofficial limit.

Journal = top 20 economics journals by impact (www.scimagojr.com/journalrank.php?area=2000). **Limit** = ceiling on number of words (pages) allowed in research articles, as specified on journal web pages. Where the limit is specified in pages we list the projected word count based on the specified page limit (in parentheses) with double-spaced lines or 1.5 spaced lines (*) and standard 12-point font. **At submission** = limits apply at submission. **Editor's discretion** = longer versions may be accepted with editor's approval. For further clarification of journal policies see online Appendix C. **Practices** = mean, minimum, maximum word counts across all research articles and including all published material (footnotes, references, appendices, et al.). **Actual maximum** = maximum word count, including or excluding references and appendices as specified by the journal's policies. All journals observed in the calendar year 2015 except those marked with a double asterisk (**), which are observed in 2014.

ply with the imposed limits (they could withdraw their submission and resubmit to another journal, complain to the editorial board) and reviewers would also need to be brought on board. Results could then be compared by standard metrics of influence such as citations—though some confounding might result as the nature of the experiment became known throughout

a discipline and authors posted “full” versions on their web sites.

Natural experiments can also be imagined. For example, one might regard length limits as an instrument for actual length (columns 1 and 7 are indeed highly correlated). Citation counts for articles could then be regressed against the instrumented

values for article length. However, this research design cannot disentangle journal fixed effects (some journals are more cited than others, even among the top twenty journals in Table 1).

Even so, we may learn something from the simple expedient of comparing articles—published in the same journal—that are shorter and longer. Because we are interested in relative length within the same journal, it is sufficient to rely on page counts (as listed in citations or on the journal’s table of contents) rather than word counts.

As a measure of the scholarly impact of journal articles, we rely on citation counts tallied by Web of Science, transformed by the natural logarithm (to accommodate a right-skewed distribution). To eliminate variations based on year of publication we focus on a single year located in the past (so that the article has time to be digested by the academic community) but not the distant past (since we wish to generalize about contemporary policies and contemporary academic work). Balancing these goals, we focus on articles published in 2005.

Citations may be influenced by the journal so we can only reliably compare articles published by the same journal. Fortunately, a good deal of variation can be found in most economics journals, and in one political science journal, as revealed by the range (minimum/maximum) of actual word counts in Tables 1 and 2. Our analysis therefore focuses on those journals (excluding sociology) with the greatest range (in 2015), provided they were published in 2005 (excluding journals founded after that date). This includes *British Journal of Political Science*, *American Economic Review*, *Brookings Papers on Economic Activity*, *Econometrica*, *Economic Journal*, *Journal of Economic Literature*, *Journal of European Economic Association*, *Journal of Finance*, *Journal of Management*, *Journal of Marketing*, *Journal of Political Economy*, *Quarterly Journal of Economics*, *Review of Economic Studies*, *Review of Economics and Statistics*, *Review of Financial Economics*, and *Review of Financial Studies*.

Note that our selection criterion allows us to focus on journals that do not make a fetish of length, and thus follow policies that are closer to those that we advocate. The regression analysis takes the following form: $Y = X + Z + \varepsilon$, where Y is citation count, X is article length, Z is a vector of journal fixed effects, and ε is the error term. Estimation is by ordinary least squares with standard errors clustered by journal.

This resulting model, presented in Table 4, suggests that there is a robust relationship between length and citations. Indeed, the relationship appears to exist in every journal in our sample: when regression analyses are conducted for each journal, seriatim, we find a positive—though not always statistically significant—relationship between length and impact.

A plot of marginal effects is displayed in Figure 1. We preserve the logged scale of citation count on the Y axis; however, tick marks on the Y axis correspond to raw (unlogged) values in order to render the exercise more natural.

It is tempting to focus on the—apparently huge—impact of article length on citations as one approaches the right end of the X axis. However, this is not where most of our data falls, as suggested by the wide confidence bounds in Figure 1. The

mean number of pages in our sample is about 25, with a standard deviation of about 12, so generalizations near the center of the distribution are apt to be most meaningful.

Consider an increase in article length from 25 to 35 (a little less than one standard deviation), which translates into an increase of about 6,000 words.¹⁰ This hypothetical change is associated with a substantial increase in citations, from (roughly) 35 to 55.

The meaning of this estimate may be debated. Let us assume for a moment that a rational selection bias is at work, namely more important articles are granted greater space in a journal’s pages. Articles deemed less significant are granted less space, as a product of the considered judgments of authors, reviewers, and editors. In this circumstance, it should be no surprise that longer articles garner more attention, as measured by citation counts.

Of course, we cannot rule out the possibility that researchers are influenced by length in their estimation of an article’s importance. Length may be regarded (implicitly) as a proxy for significance, and hence may influence citation counts. Even so, to the extent that such norms exist, they reinforce our basic point that, in the considered opinion of the scholarly community, length is correlated with importance.

Now let us consider the extent to which this analysis might be regarded as exemplifying a *causal* effect. We certainly cannot assume that articles analyzed in this sample would have been better (i.e., more impactful) if they were longer. But it does seem reasonable to propose that the longer articles in our sample would have been worse had they been shortened. Not all articles justify an expansive domain. But those that do would presumably suffer if the domain were arbitrarily constrained. In this loose and unidirectional sense, we may regard the estimate contained in Table 4 as causal.

Business Costs

We have argued that length limits should be abolished, or at least considerably relaxed. A consequence of this change in policy is that many articles would increase in length. (A few might decrease, as we have suggested, if quality rather than quantity becomes the principal metric of evaluation.) Assuming that the number of articles published over the course of a year remains the same, the number of words and accompanying features such as tables and figures will grow. This imposes additional costs on academic editors and publishers, whose resources are already stretched thin.

One cost is associated with proofreading and typesetting additional pages. We assume that this cost is fairly minimal. (One can envision a scenario in which long appendices are submitted in “copy-ready” form, as is the case now with online material.)

A more substantial cost is associated with printing and mailing the “hard copy” version of the journal. Note that under

¹⁰ We derive this word-count estimate by drawing one normalized (full-text) page from each journal in our 2005 sample, counting the words on those pages, and calculating the mean across those 16 journals.

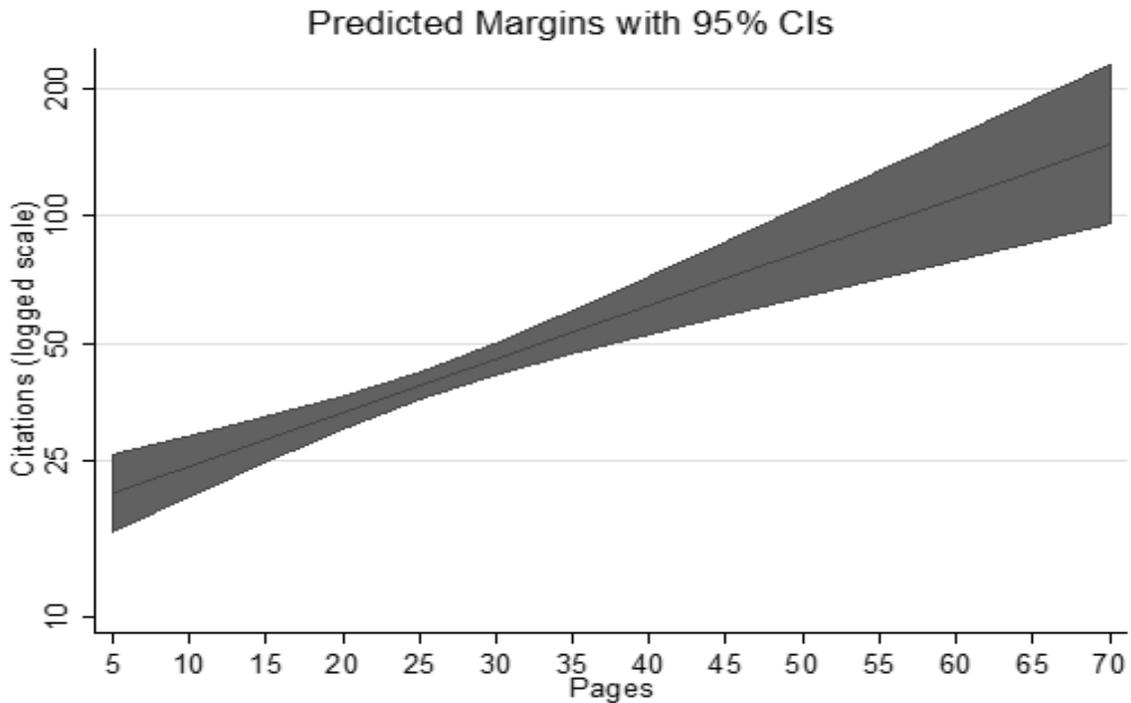
Table 4: Impact

	1
Length (pages)	0.028*** (.005)
Journal fixed effects	✓
<i>Journals (N)</i>	16
<i>Observations (N)</i>	675
<i>R²</i>	0.3050

Ordinary least squares regression of article citations (Web of Science), logged, on article length (pages), including journal fixed effects and clustered standard errors. *p<.10 ** p<.05 ***p<.01 Journals: 0=BJPS, 1=JEEA, 2=JEL, 3=AER, 4=Brookings, 5=Econometrica, 6=Econ J, 7=Finance, 8=Management, 9=Marketing, 10=Pol Econ, 11=QJE, 12=Rw Econ Studies, 13=Rw Econ & Stats, 14=Rw Financial Studies, 15=Rw Financial Econ.

	Obs	Mean	Std. Dev.	Min	Max
Length (pages)	675	25.42	11.71	3.00	71.00
Cites (ln)	675	3.68	1.16	0.00	6.97

Figure 1: Impact, Marginal Effects



Marginal effect of length (pages) on impact (citations, logged), based on benchmark model in Table 4, with 95% confidence intervals.

the current business model most journals are sold to individuals and institutions (primarily university libraries) that receive a paper copy, which may then be bound prior to shelving (yet another cost, though one that libraries must bear). In economics, many journals charge a publication fee, which no doubt helps to support production costs, and may account for the greater latitude granted to authors by journals in that discipline.

However, the hard copy format seems increasingly anachronistic in an age when most journal output is accessed online and when many journals are adopting online-only publication formats. If this is the wave of the future, there may be good reasons to hasten its arrival. Our proposal presumes that this is possible, and desirable.

Conclusions

The expansive policies adopted by many top economics journals dovetail with a move within the field to prize quality over quantity. Economists lay their claim to fame on a small number of high-impact publications rather than a larger number of less-cited ones. H-index scores matter more than the length of a CV. This may have something to do with the not-so-secret desire of every economist: to obtain a Nobel prize by the end of their career.

While no such holy grail exists for political science and sociology, it may still be possible to adjust incentives so that the time-consuming search for fundamental discoveries and/or comprehensive analyses of a large topic is facilitated. One small but important step in this direction involves loosening the noose around authors' necks so they can focus on the task at hand, rather than the space they must fill.

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Symposium: Causal Mechanisms and Process Tracing

Introduction: Mechanisms and Process Tracing

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There has always been a close association between the empirical method of process tracing and a theoretical interest in causal mechanisms. In their highly influential 2005 book on case-study research, Alexander George and Andrew Bennett refer to process tracing as “an operational procedure for attempting to identify and verify the observable within-case implications of causal mechanisms.”¹ It would probably be fair to say that most methodologists and practitioners of process tracing see the approach largely in these terms: as the search for evidence of the mechanisms that are operating within a case and that generated or contributed to the outcome of interest.

At the same time, there has been substantial ambiguity about what causal mechanisms in fact are. In a survey of social scientific and philosophy of science literatures, James Mahoney identified 24 definitions that differ from one another in crucial respects, including in their positions on: whether mechanisms are composed of intervening variables; whether mechanisms are singular or generally recurring, lawlike relations; whether rational choice is intrinsic to social mechanisms; the level of analysis to which mechanisms apply; whether mechanisms are mere analytical constructs or processes actually existing in space and time; and whether mechanisms are themselves causes or, rather, connect causes to outcomes.² It is arguably much harder to be clear about how process tracing itself should be conducted if there is confusion about the nature of one of its key objects of analysis.

In this *QMMR* symposium, four leading qualitative methodologists—Derek Beach, Rosa Runhardt, David Waldner, and Andrew Bennett—advance arguments in response to two questions:

1. How should we conceptualize causal mechanisms for the purposes of empirical social inquiry?
2. How, given this conceptualization, should we undertake process tracing as an approach to causal inference?

In the remainder of this essay, I provide a brief roadmap to the debate that unfolds and point to a set of important questions that the discussion raises. I begin by juxtaposing the arguments advanced by Beach, Runhardt, and Waldner, turning

then to Bennett’s contribution, which critically reflects on two of the other contributions.³

To the first question, the authors advance substantially different, if partly overlapping, understandings of causal mechanisms. Beach defines mechanisms as theoretical systems that characterize *entities engaging in activities*.⁴ The key theoretical work in characterizing a mechanism, in Beach’s view, lies in establishing *productive continuity*⁵ across steps: that is, in stating as precisely as possible how each activity transmits causal force from one entity to another. A key advantage of this understanding of mechanisms, Beach contends, is that it focuses our attention on *how* causal effects were generated and outcomes emerged. Beach sharply contrasts this conceptualization with views of mechanisms as either intervening variables or linked counterfactual causal relations, pointing both to explanatory gaps in and empirical problems with these alternatives.

Waldner, in his contribution, similarly understands mechanisms as generative of outcomes and highlights productive continuity as a desirable feature of causal explanations.⁶ Waldner also rejects a rather common view of mechanisms as intervening variables but departs sharply from Beach’s understanding of mechanisms as systems of entities and activities. In Waldner’s view, mechanisms should be understood strictly as those components of our causal explanations that display a particular property: *invariance*. While variables constitute those parts of a causal chain that can be “wiggled,” Waldner holds mechanisms to be those relations or features of phenomena that do not change (and at least under a certain range of changes in context). In an explanation of the forward movement of an automobile, oxygen and fuel in the car’s engine are variables that can be manipulated; combustion, on the other hand, is an invariant process that always occurs under the right conditions and thus constitutes a mechanism. Waldner sees this conception of mechanisms as more fundamental than, but compatible with, a view of mechanisms as counterfactual causes: if mechanisms produce effects, then they are also causes in a counterfactual sense.

Like Beach and Waldner, Runhardt also draws on the concept of productive continuity: mechanistic explanation, she argues, requires a gapless account of how initial conditions lead to outcomes.⁷ Unlike Waldner, however, Runhardt understands mechanisms as chains of causation of the form, $A \rightarrow B \rightarrow C$. More substantively, Runhardt parts ways with Waldner in arguing that relations of invariance, which may be common in the natural world, will be a much less prominent

³ See Beach 2016, Runhardt 2016, Waldner 2016 and Bennett 2016 in this symposium.

⁴ Beach 2016.

⁵ The concept is drawn from Machamer, Darden, and Craver 2000.

⁶ Waldner 2016.

⁷ Runhardt 2016.

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¹ George and Bennett 2005, 138.

² See Table 1 in Mahoney 2001.

feature of social mechanisms. As compared to biological causal processes, for instance, causes in the social world are far harder to conceptualize in ways that lend themselves to lawlike statements about effects; moreover, many chains of social interaction occur only once. At the same time, unlike Beach, Runhardt sees productive continuity as fully consistent with a counterfactual view of mechanisms—a view with direct implications for her methodological advice.

These three authors' differing understandings of mechanisms, in turn, inform their divergent arguments about how process tracing should proceed. Beach, viewing mechanisms as linked chains of entities and activities, contends that process tracing should involve the search for evidence about whether entities in fact engaged in the activities posited in a given theory of mechanism. Runhardt, in contrast, holds that observations of entities engaging in actions consistent with a theory are insufficient to establish a *causal* connection between steps in a process. Rather, drawing on James Woodward's interventionist account of causation,⁸ she argues that analysts should seek more direct evidence of causal effects in the form of empirical support for counterfactual claims (a strategy that Beach explicitly argues against). Given the scarcity of regularities in the social world, Runhardt argues, suitable cross-case comparisons for establishing counterfactuals will usually be unavailable. Instead, she contends, analysts should focus their efforts on examining "theoretical interventions" by collecting within-case evidence that is diagnostic of what would have happened had the cause been manipulated.

Waldner advocates an explanatory strategy grounded in his understanding of mechanisms as invariant relations.⁹ In particular, he calls on process tracers to empirically construct case-level event histories and to map those histories onto causal graphs crafted from knowledge of invariant mechanistic properties of the world. There is, thus, a similarity between the kinds of data implicated in Waldner- and Beach-style process tracing: both involve a search for evidence of how things unfolded within a case (rather than, as in Runhardt's approach, evidence of what *would* have happened under counterfactual conditions). Yet these two versions of process tracing put evidence of how things happened to very different uses. In Beach's approach, evidence of entities and activities is used to *test* causal theories as explanations of an outcome. In the "scientific solution" that Waldner outlines in his essay, in contrast, we use prior theoretical knowledge of invariant relations as a foundation for drawing case-level causal inferences from the data. Rather than testing general theories, process tracing in this approach *rests* on general claims about the world.

In the symposium's final contribution, Bennett takes a step back to assess the state of the debate over mechanisms and process tracing.¹⁰ He reflects, first, on how his own thinking about the meaning of causal mechanisms has evolved since his early methodological work with George. While George and

Bennett originally drew on Wesley Salmon's work¹¹ in characterizing mechanisms as processes through which energy, information, or matter are transferred between entities, Bennett has since moved toward a Woodwardian understanding of causation as defined by invariant counterfactual relations, a view that he sees as aligned with Waldner's.

Bennett then turns to an assessment of arguments that Waldner (in this symposium and elsewhere¹²) and Runhardt have advanced about the logic of process tracing. His engagement with Waldner focuses on the "completeness" standard that Waldner has elaborated for judging the adequacy of the causal theories and empirical accounts employed in process tracing. While Bennett concurs on the value of completeness, he calls into question Waldner's claim that the standard provides qualitative researchers with "a much-needed stopping rule" in regard to when sufficient evidence has been collected. In particular, Bennett argues that the standard offers limited guidance insofar as it will rarely be possible to establish comprehensive accounts of invariant social mechanisms. For instance, for many social mechanisms the full set of scope conditions and background assumptions may be impossible to specify, leaving it unclear when an explanation invoking such mechanisms is "complete." (Waldner responds to Bennett's critiques toward the end of his own essay.)

Turning to Runhardt's contribution, Bennett accepts the importance of counterfactual-oriented evidence for evaluating case-level explanations. Yet he sees far wider use of counterfactuals in current process tracing practice than Runhardt acknowledges. Bennett judges the counterfactual evidence in Runhardt's key illustration—Kristen Bakke's study of Chechen insurgency tactics¹³—to be substantially stronger than Runhardt does. He also points to important uses of counterfactual reasoning beyond the establishment of causal effects; counterfactuals can also be used, for instance, to uncover biases in researchers' mechanistic reasoning and to test scholars' degrees of subjective confidence in causal claims.

The essays in this symposium do not resolve the questions of how we ought to define mechanisms or how we should identify their operation within cases. The contributions help advance our understanding of qualitative causal inquiry, however, by (i) crystallizing a set of distinct conceptualizations of causal mechanisms and (ii) deriving from each conceptualization an empirical approach to within-case causal inference. While researchers may continue to understand mechanisms in differing ways, the essays in this collection can help us think more clearly about how to choose a research design conditional on our understanding of causality.

The authors' arguments, moreover, raise a number of important questions about qualitative causal inference that are in need of further examination. To suggest a few: To the extent that counterfactuals are central to process tracing, what counts as valid evidence to support a counterfactual? How far can within-case evidence, as compared to cross-case evidence,

⁸ Woodward 2003.

⁹ Waldner 2016.

¹⁰ Bennett 2016.

¹¹ George and Bennett (2005) draw on Salmon (1989).

¹² Waldner 2015.

¹³ Bakke 2013.

take us in establishing counterfactual claims? If process tracing hinges on knowledge of invariant mechanistic processes, from where can we draw these prior beliefs? Can the tracing of entities and activities in individual cases serve cumulatively to build this kind of background knowledge? Or does Waldner's theory-driven approach imply that process tracing's inferences must ultimately rest on findings derived from other methods, such as randomized experiments? Must we choose between *using* mechanistic theories (a la Waldner) and *testing* theories (a la Beach)? Or can we do both at the same time, starting out with partial or uncertain knowledge of mechanisms and using process tracing to refine and update these beliefs? Further, the study of mechanisms can be put to different purposes. We might be interested in uncovering mechanisms as a means to identifying unknown causal effects, for instance, or as an explanation of how known causal effects occur. How might our inferential goals have implications for the methods through which mechanisms should be examined?

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What Are We Actually Tracing? Process Tracing and the Benefits of Conceptualizing Causal Mechanisms as Systems

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1. Introduction

Most scholars now agree that process tracing, as a distinct social science method, involves tracing causal mechanisms using in-depth case studies. Studying causal mechanisms shifts the analytical attention from causes and outcomes to the causal process that links causes and outcomes together. However, while they are widely used, statements about causal mechanisms are also the least understood type of causal claim in the social sciences.¹ This short essay discusses two competing approaches to studying causal mechanisms (a counterfactual-based and systems understanding of mechanisms), arguing that adopting a systems understanding of mechanisms results in research designs that produce actual evidence of the process we are attempting to trace instead of hypothetical 'what ifs' or weak comparisons.²

Before we proceed, it is important to note that some scholars use the term causal mechanism to refer to a series of events, or a narrative story, prior to the occurrence of an outcome.³ Describing a series of events can provide a plausible descriptive narrative about *what* happened, but it does not shed light on the causal question of *why* things happened. Other scholars like Abell do go a bit further than just tracing events. Abell contends that when studying causal mechanisms, we need to go beyond tracing events to develop narrative structures with action linkages that build on *subjective* counterfactuals, where we ask actors who participated in a process whether things could have been different at critical junctures of a process.⁴ While Abell's suggestion does point us in the direction of moving beyond just tracing events, he then reduces the scope of research questions that we can study to only those that can be assessed by asking actors themselves whether things could have been different. The two approaches to causal mechanisms discussed in this paper go beyond tracing temporal sequences or subjective counterfactuals in their attempts to explain more explicitly why things happen.

2. Mechanisms as a series of 'mini'-counterfactuals and problems with the masking of causal logics

Within the social sciences, the most widespread understand-

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¹ E.g. Brady 2008; Gerring 2010; Hedström and Ylikoski 2010; Waldner 2014.

² Runhardt 2015; Woodward 2003.

³ For examples of this understanding, see Abell 2004; Mahoney 2012, 571; Suganami 1996, 164–168.

⁴ Abell 2004, 295–296.

ing of mechanisms is to view them as a form of counterfactual, often described as intervening variables.⁵ The clearest statement of this position is found in Woodward's work, where he contends that if a given part of a mechanism did not exist, the next part of the mechanism would also not have existed, thereby enabling us to infer that the preceding part was causally linked to the outcome because its absence produced a difference.⁶ Mechanisms are viewed as one or a series of 'mini'-counterfactual claims that are at a lower level than the overall causal claim of a relationship.⁷ Therefore, in Woodward's understanding, all mechanisms are sets of counterfactual claims about links in a process. If one part does not work, the rest of the mechanism does not function.

The theoretical logic of the process is masked

The counterfactual-based understanding has several theoretical and methodological disadvantages. First, there is the theoretical problem that the logical causal arguments linking parts of mechanisms together are masked in the counterfactual understanding. When the parts of mechanisms are understood as lower-level counterfactuals, the links between them are typically depicted as causal arrows linking one part to the next without making explicit what it is about one part that links it in a causal sense to the next part. For example, Rosato describes a causal mechanism linking democracy and peace as: Democracy → accountability group constraint Peace.⁸ Yet we are not told anything more about what it is about accountability that actually is linked to group constraint, meaning that the causal logic behind the mechanism is not made explicit: it is "masked" or "black-boxed" such that we aren't invited to see the causal logic inside the mechanism. The masking of causal links is not problematic in the experimental tradition because it is the actual counterfactual manipulation that enables us to infer causation; we know there is a causal effect because we can measure the difference that the manipulation makes irrespective of whether we actually know anything about how the effect is produced. But this theoretical masking of causal mechanisms becomes a critical weakness when studying causal mechanisms using case studies because we cannot assess whether the causal logic linking parts together actually makes sense, or whether there is empirical evidence of the causal links working in a case.

Methodological problems

In addition to masking the causal logic of mechanisms, theorizing mechanisms as a series of counterfactuals forces us to investigate empirically whether the presence or absence of

each part of a mechanism makes a difference in whether or not the process functions to make causal inferences.⁹ To assess difference-making means that we have to conduct a form of experimental manipulation for each part of the mechanism, investigating the difference that the presence or absence of a part of a mechanism makes for the overall outcome, other things held equal. This can be achieved in principle either through an actual experiment with human manipulation of the research subject, or by implementing a weaker design with a natural or logical experiment.¹⁰

What then are the disadvantages of utilizing experiments to study mechanisms? Actual controlled experiments are great for providing evidence of difference-making (whether the presence or absence of a part makes a difference in the function of the mechanism) through the manipulation of the presence or absence of a treatment across cases, enabling strong inferences about causal effects. But to assess *parts of a mechanism* using actual experiments would involve disaggregating a mechanism into a series of parts and then deploying mini-experiments for each counterfactual claim in our mechanism! There are few social science research questions for which it is relevant to engage in a set of sequential mini-experiments in a meaningful fashion. For example, what kind of experiment could validly model each part of a mechanism separately whereby lobbyists influence policy-makers? Could we create a research situation where we investigate first in an experimental fashion (i.e. holding all other things equal) whether information provided by a lobbyist and not provided in a control case makes the hypothesized difference in policy-maker attitudes toward the lobbyist, followed by another controlled experiment where we investigate whether this change of attitude actually produces the hypothesized difference in policy-maker behavior?

If we take the natural experiment route, this requires that we find two real-world cases that are similar in all respects except for the presence and absence of a particular part of a causal mechanism. If we unpack a mechanism into multiple parts (i.e. a series of intervening variables in the counterfactual understanding), a natural experiment would involve trying to find two completely comparable cases for each part: one where the part is present and one in which all other factors are present but the part of the mechanism being assessed. We would then have to repeat this natural experiment for each part of the mechanism! But could this even be achieved? A prominent advocate of counterfactuals admits that "...controlled comparison and matching face the difficulty of finding real-world cases that are identical in all respects but one..."¹¹ But unless we can substantiate that 'all other things are equal' except for the presence or absence of a single part of a mechanism, we cannot make a causal inference that the absence of a part made a difference.

Using logical hypotheticals to assess difference-making is a counterfactual-based method where we compare an existing real-world case with a hypothetical "what if" case where all

⁵ Gerring 2010; Imai et al 2011; Weller and Barnes 2014.

⁶ Woodward 2003, 350–358; also Steel 2008; Runhardt 2015.

⁷ Note that level here does not refer to theoretical levels of analysis, such as macro- versus microlevel theories. Instead, we are talking about the disaggregation of a causal process into smaller constituent parts. The process itself that is disaggregated can be a macrolevel theory (e.g. economic development → democratization) or a microlevel theory of small group decision-making.

⁸ Rosato 2003, 585–586.

⁹ Woodward 2003; Runhardt 2015.

¹⁰ Runhardt 2015.

¹¹ Levy 2015, 390.

other things are present except the part of the mechanism whose difference-making we want to assess. The logical argument is then made that if a particular cause had not occurred, the outcome would not have occurred. In effect, a logical counterfactual comparison attempts to approximate a most-similar systems test. To best approximate a most-similar systems test, the most important criterion is the ‘minimal-rewrite’ rule: hypothetical changes should involve only the most minor changes possible to enable us to assess whether the change can logically produce a major change in an outcome.¹² But a minimal re-write is a non-achievable ideal in most circumstances, as the absence of all but trivial conditions could have significant knock-on effects for other causal conditions, potentially shifting a number of other conditions in our logical counterfactual situation.¹³ And logical hypotheticals face the critical challenge of being merely “what ifs” without any actual empirical evidence. Despite many attempts to build a methodology for logical counterfactuals,¹⁴ there are no objective empirical truth conditions for assessing a non-existent but possible alternative world.¹⁵ Single-case counterfactual comparisons involve comparing an existing real world case with a hypothetical counterfactual case.

At a more fundamental level, even if we could engage in meaningful assessment of the difference that parts of mechanisms make through some form of experiment, assessing the difference that an individual part makes assumes that there is not *redundancy* embedded in the mechanism. In biological mechanisms there is (luckily) redundancy of key parts of the mechanism, meaning that if we remove one part to see what happens, we still find that the mechanism worked to produce the outcome because another previously unknown part with similar capacities became activated instead.¹⁶ There is no reason we should not expect similar redundancy in key parts of important social mechanisms. But based on the counterfactual logic, if we found that removing a part had no real effect on the outcome, we would disconfirm the part as being causally relevant, which would be a flawed inference if redundancy exists. In the case of redundancy, the correct inference would be that the part was only one of multiple ways that causal forces could be transferred through the mechanism. Using evidence of difference-making gained from counterfactual-based experimental manipulation, would not enable us to detect redundancy.

An additional problem is that isolating the difference that individual parts make results in a form of atomistic analysis of the workings of mechanisms. Amongst scholars who view

mechanisms as systems,¹⁷ mechanisms are viewed in a more holistic fashion, and their theories attempt to capture the complex interrelationship between parts, where the effects of individual parts often only manifest themselves fully together with the effects of other parts. This holistic view is seen in Cartwright’s work, where she writes that, “There are any number of systems whose principles cannot be changed one at a time without either destroying the system or changing it into a system of a different kind.”¹⁸

Even if we could utilize experiments, they would still not tell us *how* a process works.¹⁹ An experiment provides us with no direct evidence of how one part is actually linked to the next; all we know is that the absence of a part results in breakdown. In the words of Bogen, “How can it make any difference to any of this whether certain things that did not happen would have or might have resulted if other things that did not actually happen had happened?”²⁰

3. Mechanisms as systems: theoretical and methodological considerations

Proponents of the systems understanding of causal mechanisms contend that “...a mechanism explanation for some happening that perplexes us is explanatory precisely in virtue of its capacity to enable us to understand how the parts of some system actually conspire to produce that happening.”²¹ The focus in the systems understanding is the dynamic, interactive influence of causes upon outcomes, and in particular how causal forces are transmitted through a series of interlocking parts of a causal mechanism to produce an outcome in actual cases. In the systems understanding, mechanisms are typically described as being comprised of a series of parts composed of *entities engaging in activities*.²²

Consider a mechanism as a simple machine. Entities are the toothed wheels in the machine. Entities engage in activities, which are the movements of the toothed wheels. Activities produce change that transmits causal forces through each part of a mechanism.²³ The most important aspect of theorizing mechanisms in the systems understanding is therefore capturing explicitly what is *inside causal arrows*, making explicit the causal logic whereby the activities of one part of a mechanism link it to the next part, attempting to formulate an overall mechanism with “productive continuity” between the cause (or set of causes) and an outcome.²⁴ When there is productive continuity in our theorized causal mechanism, there are no large logical holes in the causal story linking C and O together.²⁵ If a

¹² Tetlock and Belkin 1996.

¹³ Bennett (2016) in this symposium discusses a “Soviet growth” counterfactual. However, a scenario in which perestroika policies actually worked would most likely not be an “other-things-equal” comparison (minimal-rewrite) because other major changes would probably have had to occur for it to actually work (e.g., dramatically increased trade and loans from Western countries).

¹⁴ Goertz and Levy 2007; Tetlock and Belkin 1996; Lebow 2000; Levy 2015; Fearon 1991.

¹⁵ Beach and Pedersen 2016.

¹⁶ Illari and Russo 2014, 158.

¹⁷ For examples, see Machamer 2004; Waskan 2011.

¹⁸ Cartwright 2007, 239.

¹⁹ Machamer 2004; Waskan 2011; Russo and Williamson 2007; Illari 2011; Dowe 2011.

²⁰ Bogen 2005, 415.

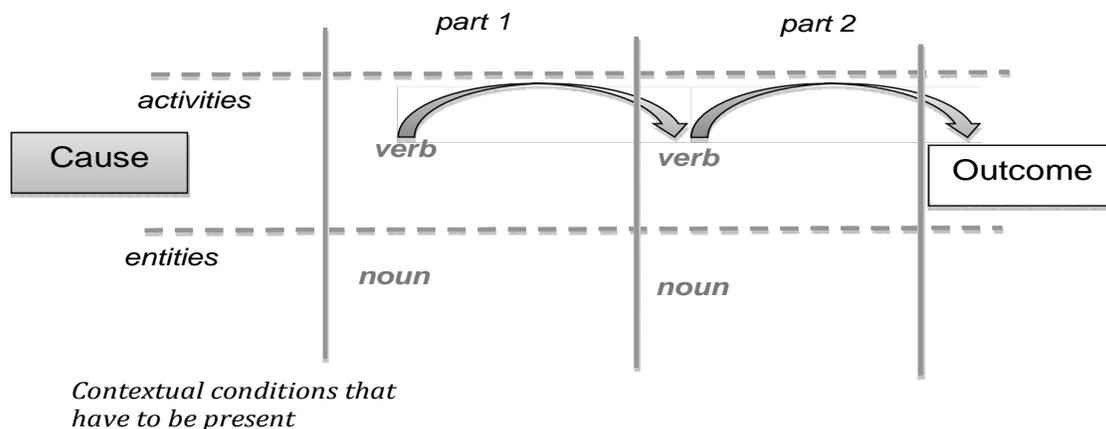
²¹ Waskan 2011, 393.

²² Machamer, Darden, and Craver 2000; Machamer 2004; Rohlfing 2012, 35–36.

²³ Machamer, Darden, and Craver 2000; Machamer 2004.

²⁴ Machamer, Darden and Craver 2000.

²⁵ Machamer, Darden, and Craver 2000, 3; Darden 2002, 283.

Figure 1: A simple template for a two-part causal mechanism

If a mechanism is represented schematically by $C \rightarrow [E1 \rightarrow E2 \rightarrow E3] \rightarrow O$, then the continuity lies in our description of what the arrows represent, defining very explicitly what activities actually transfer causal forces from one part of a mechanism to the next. A missing arrow or the inability to specify an activity connecting one part to the next leaves an explanatory gap in the productive continuity of the mechanism.²⁶

What the entities and activities are precisely will be dependent on the type of causal explanation we are working with, along with the level at which the mechanism works and the time span of its operation. A theory of a mechanism as a system can be depicted as in Figure 1, where each part of the mechanism between a cause and an outcome is detailed in terms of the entities engaging in activities, focusing in as much detail as possible on how the activities of one part of the mechanism produce the next part of the mechanism. The entities are defined as nouns, whereas the activities are depicted as verbs. As seen in Figure 1, the contextual conditions for the proper operation of a given mechanism should also be made explicit.

Theoretical advantages of unpacking mechanisms as systems

There are not many examples of good theorized mechanisms as systems in the social sciences. Most commonly, theorized mechanisms unpack the causal process slightly, but key parts of the causal story typically remain masked. For example, in Ziblatt's 2009 article about the sources of electoral fraud in new democracies, he claims to be studying a "capture" causal mechanism that links landholding inequality to electoral fraud in Imperial Germany.²⁷ However, the actual theorized causal mechanism is never fleshed out in enough detail to allow us to either evaluate the underlying theory (the causal logic) or to assess empirically whether there is evidence that confirms the "capture" mechanism. The closest we get to a theoretical de-

scription of the causal mechanism is where he writes (italics are the author's), "They [the landed elites] exert influence *indirectly* via the capture of rural public officials such as mayors, county commissioners, police officials, and election officials, who in turn are the actors that interfere with free and fair elections. In its most acute form, capture occurs as socioeconomic interests infiltrate the state by using their *own* personnel to staff the state."²⁸

While telling us something about the process—that it goes through local officials and in extreme instances might occur through direct staffing—Ziblatt does not detail a causal mechanism that exhibits productive continuity. In particular, he sheds little theoretical light on the causal process whereby landed elites are able to capture local officials. For instance, what types of power resources do landed elites deploy to capture officials? Does capture occur through the use of material resources, such as controlling sources of revenue, or by deploying discursive or social resources? Do landed elites have to actively intervene to capture officials, or do officials anticipate what local officials want? When and why should local officials be responsive? And once captured, what is the process whereby local officials actually engage in electoral fraud? What types of actions do they use? Removal of voters from electoral rolls, pressuring poll officers, and so on?

In this example, the reader is left in the dark about the causal logic linking the parts of the process together. Indeed, very different causal logics could be working in Ziblatt's theorized mechanism depending on whether the "capture" is theorized to work through material or social resources or through active intervention or anticipation. And at the empirical level, because the links are not made explicit, Ziblatt's analysis produces small empirical vignettes that insinuate the existence of an underlying mechanism without providing mechanistic evidence that would enable causal inferences to be made. Indeed, how can we claim we have evidence of a process when we are not told what the process is that is being studied?

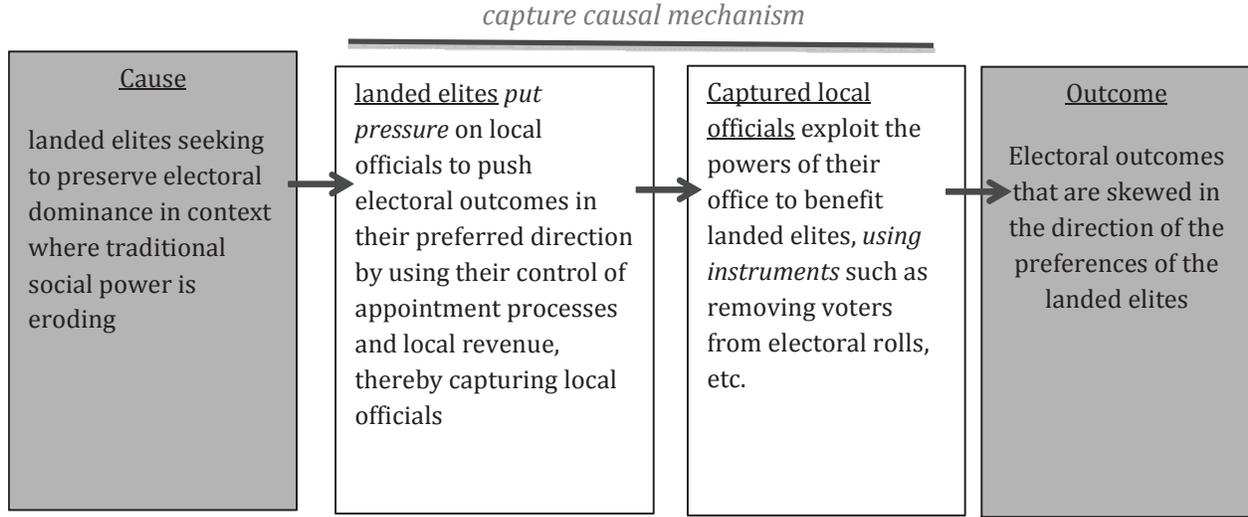
Working within the systems understanding of mechanisms, we would want to expose the causal logic between each part, making more explicit in particular the *activities* that are

²⁶ Machamer, Darden, and Craver 2000, 3. While it can always be argued that one could always unpack the parts of the mechanism even more, thereby heading down into the infinite regress problem, there is a clear pragmatism in the writings of Machamer and others that suggests that we can stop when the causal logic is explicated in a "good enough" fashion to give us a working understanding of the key working parts of the process.

²⁷ Ziblatt 2009, 12–18.

²⁸ Ziblatt 2009, 14.

**Figure 2: A “capture” mechanism linking landed elites with electoral fraud
(adapted from Ziblatt 2009)**



theorized to transfer causal forces from one part to another. For instance, Figure 2 illustrates what the capture mechanism of Ziblatt’s theory could look like if we fleshed out the causal logic linking each part to the next.

The core difference between the capture mechanism used by Ziblatt in his article and the capture mechanism theorized in Figure 2 is that my formulation more explicitly details the causal logic of each part of the causal mechanism, in particular which activities form the causal logic linking the parts together. The subsequent empirical analysis would attempt to assess whether the observable manifestations of the activities of entities for each part were present in a case, enabling us to get closer to actually tracing the empirical fingerprints left by the operation of the parts of a hypothesized causal mechanism.

Early in our research we might not know exactly how one part is linked to the next, thereby resulting in a lack of productive continuity in our explanation. However, this does not mean that we should give up our search for an “unpacked” causal mechanism that exhibits productive continuity. Instead, we can engage in further theoretical work, engaging in a far-ranging search for inspiration from existing theories,²⁹ or engaging in a more inductive, exploratory empirical analysis of a case that attempts to shed light on how particular causal links work.

Methodological advantages (and drawbacks) of understanding mechanisms as systems

In the systems understanding, of causal mechanisms, what we want to trace empirically is whether a process (mechanism) actually played out as we had theoretically expected it to in a given case. Here we are more concerned with what actually took place in the empirical record and develop theories about processes on this basis instead of based on counterfactuals of what “might have” happened if things had been different.³⁰

²⁹ Here formal modelling can be one helpful tool to “game through” potential pathways of mechanisms.

³⁰ Waskan 2011: 394; Gross 2009; Groff 2011; Machamer 2004.

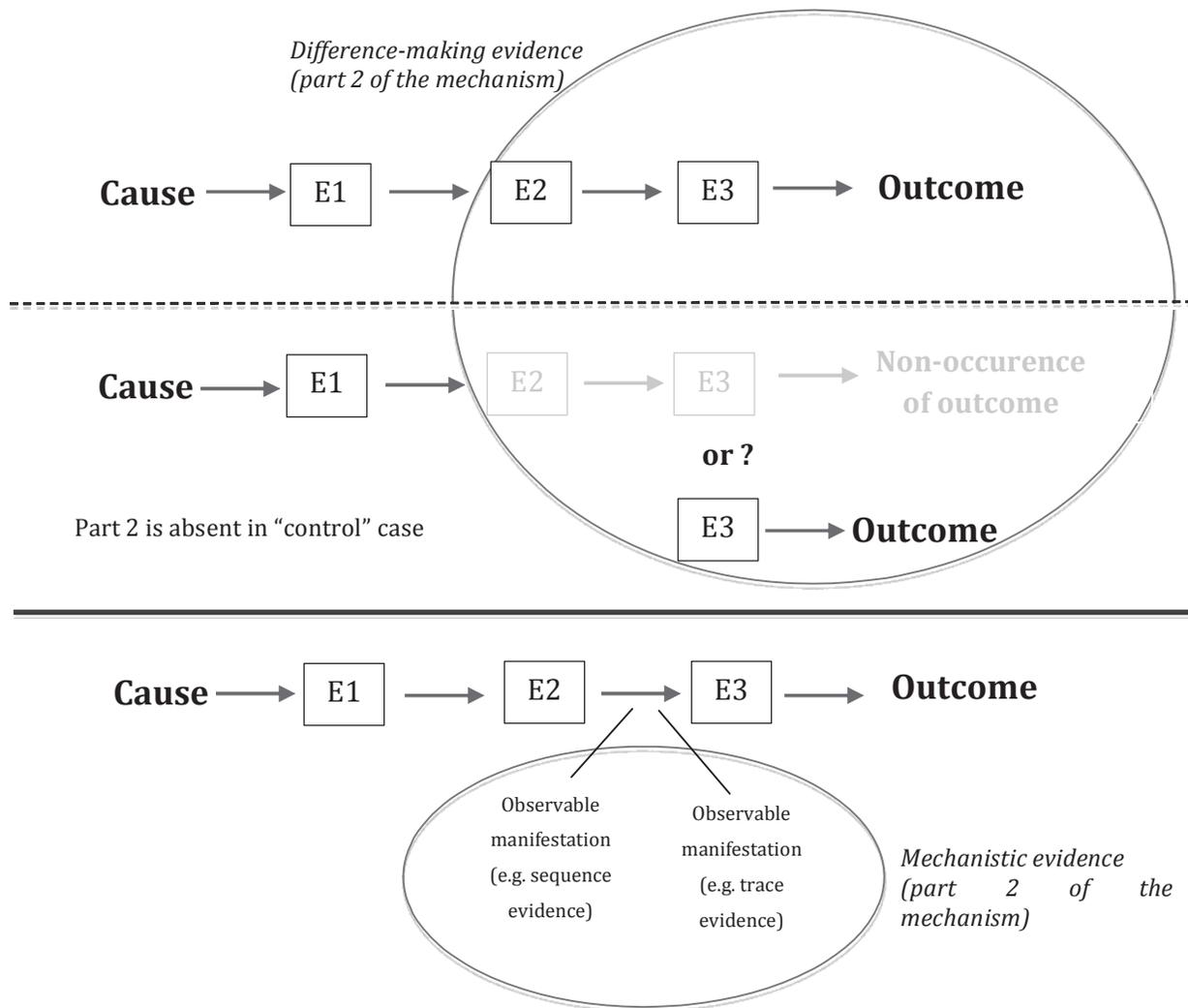
Instead of engaging in forms of mini-experiments for all the parts of the causal mechanism to assess how a causal process played out, we can assess the observable evidence left by the actual activities of entities in a real world case. The distinction between the type of evidence used to study mechanisms in the counterfactual- and systems understanding can be seen in Figure 3, where the top example is of evidence of difference-making used in a counterfactual, and the bottom example is of mechanistic evidence used in the systems understanding.

The crucial distinction is in the *object of the evidence*.³¹ In the systems understanding, mechanistic evidence is used to trace the observable manifestations left by the activities of entities for each part of a mechanism, with the goal of causal explanation, to gain a better understanding of how causal processes actually work. In contrast, in the counterfactual-based understanding, these activities are masked when parts are theorized to be manipulable counterfactuals because they remain masked, depicted merely as causal arrows without telling us what the causal link actually is. The counterfactual-based account in effect transforms the within-case study of causal processes into a cross-case analysis of patterns of variation at a lower level of aggregation, a level at which we lose focus on the process between the cause and outcome.³² The result of transforming a within-case tracing of causal processes into a cross-case assessment of difference-making is that we gain little information about how the process actually played out in a case. But is studying the process between a cause and an outcome not the reason we want to trace a causal mechanism in the first place? This distinction can be seen in Figure 3. The top half illustrates that evidence of difference-making for parts of a mechanism would actually be evidence of the difference between the two “cases” (treatment and control). Evidence of difference-making between the case and the counterfactual control case is the comparison of what happens when part 2

³¹ Illari 2011.

³² Mayntz 2004: 244–245; Illari 2011.

Figure 3: The distinction between evidence of difference-making and mechanistic evidence.



(the second part of the causal mechanism) is present (mechanism continues) and what happens in the control case where part 2 is absent. Difference-making evidence is binary in form, in that either the process broke down (first scenario in Figure 3) or continued without interruption (second scenario). If the process breaks down and we have controlled for other potential causes of breakdown because everything else is the same between the two cases, we can conclude that part 2 has a causal effect and is necessary for the mechanism.

The bottom half of the figure illustrates mechanistic evidence for the same part of the mechanism. Mechanistic evidence would try to assess the observable manifestations left by the activities of the entities for part 2 instead of comparing the causal effect of the presence and absence of a part. When assessing mechanistic evidence, we ask ourselves what observable manifestations the theorized entities engaging in activities might leave in a case. The difficulty with using mechanistic evidence is that it is much more complex and multifaceted than the counterfactual-based difference in the presence or absence of an outcome. Instead, many different types of em-

pirical material can be used as evidence to update our confidence in the functioning of a part of a causal mechanism. Empirical material could be in the form of trace evidence of activities,³³ or *sequence evidence* in the form of expectations about a sequence of activities.³⁴ Evaluating the probative value of evidence of the workings of parts of mechanisms requires careful operationalization of the empirical fingerprints the activities of entities might leave and justifications for what finding or not finding different types of evidence tells us.³⁵

4. Conclusion: the benefits (and drawbacks) of conceptualizing mechanisms as systems

This essay has argued that a systems understanding of mechanisms has several advantages over the more common counterfactual-based understanding. First, fleshing out the causal links

³³ For example: Is there a record of a meeting between the lobbyist and politician?

³⁴ For example, in a rational decision-making process we would expect actors to collect information, evaluate it, and then take a decision that maximizes their utility.

³⁵ For more information, see Beach and Pedersen 2016.

for each part of a mechanism exposes the underlying causal logics to more detailed scrutiny, resulting in better theories, other things equal. Second, theorizing mechanisms as lower-level counterfactuals implies that we assess them empirically by analyzing the difference that their absence makes for the rest of the causal process. But studying mechanisms as a series of mini-experiments runs into many methodological problems and challenges, the most serious being that it only tells us about the causal effect of the individual parts of a mechanism, and we still do not know how a causal mechanism actually works in reality.

Unpacking mechanisms as systems, where each part is clearly theorized as an entity engaging in particular activities, has theoretical and methodological advantages, but it is by no means a panacea. Indeed, it tells us little, if anything, about the overall *causal effect* that a cause has on an outcome.³⁶ And on a practical level, collecting mechanistic evidence for each part of a multi-part mechanism is incredibly resource demanding. There is a reason that, despite knowing about causal effects since the 1950s, it was only in the 1990s that medical researchers were able to muster strong mechanistic evidence for each part of the causal mechanism linking smoking and lung cancer.³⁷ But just because it is difficult does not mean that we should not try to unpack mechanisms as systems and trace them empirically when the analytical benefits for our understanding of how things work are potentially so high.

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³⁶ Illari 2011, 150; Steel 2008, 68.

³⁷ Russo and Williamson 2007, 162.

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Tracing the Productive Continuity of Social Mechanisms

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1. Introduction

In this paper, I conceptualize the notion of the *productive continuity* of a social mechanism. The continuity of a mechanism refers to the fact that each part of that mechanism should be causally connected to another; no part of the mechanism is isolated. Moreover, each part of the mechanism should produce, i.e. cause, the next part¹. The productive continuity of mechanisms forms an important part of philosophers of science Machamer, Darden, and Craver (MDC)’s “new mechanist” conceptualization of biological mechanisms.²

Several process tracing methodologists have taken inspiration from the MDC conceptualization of mechanisms and the concept of productive continuity. Though I accept the notion of productive continuity as crucial for social mechanisms as it is for biological mechanisms, I will show that the way one corroborates the productive continuity of social mechanisms is different from the way one corroborates it for biological mechanisms.

Philosophers of causal mechanisms disagree on what exactly productive continuity entails. I will consider two common answers, *regularities* and *causal process observations*, before presenting my own, *counterfactuals*. While regularities, i.e. the recurrence of activities in other cases, are deemed necessary for biological mechanistic explanation by Machamer,

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¹ Though productive continuity is a property of causal mechanisms themselves, one can also demand the productive continuity of a *description* of a causal mechanism between some putative cause and effect of interest. If a description of a mechanism is unclear about how certain parts of the mechanism are linked to the rest, that description does not satisfactorily explain the relation between the cause and effect under study.

² Machamer, Darden, and Craver 2000.

Darden, and Craver,³ I show this is not the case for social mechanistic explanation. Having thus discounted regularities as one source of evidence for the causal productivity of a social mechanism, I then turn to a common argument in the process tracing literature: that a social mechanism can be supported by observing the implications of its components, i.e. causal process observations, in a particular case study⁴. I argue that this too is insufficient evidence for productive continuity. Finally, I show that counterfactual evidence can provide evidence of the productive continuity of a social mechanism. I make the notion of counterfactual evidence more concrete by adopting the interventionist theory of causation set out by James Woodward.⁵

2. MDC’s theory

Philosophers of science Machamer, Darden, and Craver (herein “MDC”) characterize causal mechanisms⁶ as “entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions.”⁷ Entities and activities are linked in productive continuity and the activities occur regularly, not just in one case, but in other cases too. I will discuss in detail what this regularity entails in section 3. For now, take the example of a biological causal mechanism of chemical neurotransmission. In this mechanism, “a presynaptic neuron transmits a signal to a postsynaptic neuron by releasing neurotransmitter molecules that diffuse across the synaptic cleft, bind to receptors, and so depolarize the post-synaptic cell.”⁸ Chemical neurotransmission’s entities include such things as the presynaptic and postsynaptic neuron. Entities, simply speaking, are things; they have a spatiotemporal location, and we can distinguish them in a straightforward way from other entities. Entities are interdependent with the other part of a mechanism, the activities. The activities of the chemical neurotransmission mechanism

³ cf. Bogen 2005, 2008, Machamer 2004, Woodward 2002.

⁴ The type of process tracing I am concerned with here fits best with what is called *theory-testing* process tracing (Beach and Pedersen 2013), in which one traces a causal mechanism through case-specific observable implications of this mechanism’s existence, or *top-down* process tracing (Bennett and Checkel 2015), in which one tests type-level causal hypotheses about the mechanisms producing a particular “process” with case study data.

⁵ Woodward 2003.

⁶ Note that MDC specialize in philosophy of biology, particularly molecular biology and neurobiology. MDC express the suspicion however that their analysis “is applicable to many other sciences, and maybe even to cognitive or social mechanisms” (Machamer, Darden, and Craver 2000, 2). My conclusions about social mechanisms further on in this paper will show the scope of MDC’s theory is more limited than they suspect; MDC’s new mechanist philosophy may not reach far beyond biology.

⁷ Machamer, Darden, and Craver 2000, 3. Several process tracing methodologists have taken inspiration from the MDC conceptualization of mechanisms. Beach and Pedersen, for instance, write that “[e]ach of the parts of the causal mechanism can be conceptualized as composed of entities that undertake activities” (Beach and Pedersen 2013, 29).

⁸ Machamer, Darden, and Craver 2000, 3.

include diffusion and depolarization. The properties of the entities constrain what kind of activities they can undertake, and the kinds of activities undertaken constrain what kinds of properties the entities can have. MDC see activities as types of causes (i.e. as ‘productive’); they “are producers of change; they are constitutive of the transformations that yield new states of affairs.”⁹

MDC argue that entities and activities are linked in what MDC call “productive continuity”:¹⁰

Complete descriptions of mechanisms exhibit productive continuity without gaps from the set up to termination conditions. Productive continuities are what make the connections between stages intelligible. If a mechanism is represented schematically by $A \rightarrow B \rightarrow C$, then the continuity lies in the arrows and their explication is in terms of the activities that the arrows represent. A missing arrow, namely, the inability to specify an activity, leaves an explanatory gap in the productive continuity of the mechanism.

Productive continuity is thus central to causal mechanistic explanation. If we cannot determine that one stage of the mechanism, *A*, led to the next, *B*, we do not know whether *A* is truly causally connected to *B*. However, as stated in the introduction, philosophers of causal mechanisms disagree on what exactly productive continuity entails, and this will concern me in what follows. I will now consider two common answers, *regularities* and *causal process observations*, before presenting my own, *counterfactuals*.

3. Support for the productive continuity of social mechanisms

3.1 Productive continuity supported by regularities

According to MDC, biological mechanisms only explain the link between a putative cause and effect of interest because the activities in these mechanisms are “regular,” i.e. because activities “work always or for the most part in the same way under the same conditions.”¹¹ In other words, the same activity working in some other biological system but in the same context will produce the same effect. The separate activities that together make up the mechanism of chemical neurotransmission (e.g. diffusion, depolarization) will operate in the same way in like circumstances. Chemical neurotransmission is therefore regular in the sense that it happens between many different sets of neurons in different biological systems to the same effect.

However, not every new mechanist philosopher accepts MDC’s claim that regularity is necessary for productive continuity, and thereby for mechanistic explanation more generally. James Bogen suggests that although entities and activities are indeed organized so that they are productive of changes, these changes do not need to be regular.¹² After all, Bogen argues,

there exist irregularly operating and stochastic activities and mechanisms, as well as activities and mechanisms that operate just once. Though, in Bogen’s words, MDC believe that “generalizations describing natural regularities are essential components of causal explanations,”¹³ he argues that “causal productivity and regularity are by no means the same thing.”¹⁴

Another way of interpreting Bogen’s argument is as follows. Although the existence of regularities is one source of evidence for the causal productivity of a mechanism—since such regularities provide evidence that the link between some parts of the mechanism is genuinely causal—this is not the only source of evidence for productive continuity. It would be misleading to think so, since excluding other sources of evidence means we cannot consider singly occurring activities as genuinely causal.

Let me now turn to why we ought to reject regularities as a key source of evidence for the productive continuity of *social* mechanisms. First, consider MDC’s characterization of activities in terms of recurrence, i.e. as types of causes that describe something that acts “in the same way under the same conditions.”¹⁵ A presynaptic neuron’s release of neurotransmitter molecules will take place in many such neurons in the body, and always in the same way, by virtue of the uniformity of such neurons.

However, the same demand for recurrence of the activity in other cases leads to difficulty in fields like political science. Arguably, there are no such causally homogeneous, stable entities and activities across a range of political science cases. Units in social science mechanisms are not as clearly defined as biological entities; we cannot easily rely on a spatiotemporal location or set of boundaries to distinguish social “entities” and, similarly, social activities are not as straightforwardly defined as are biological activities. There is, for instance, no reason to believe that a democratic government will always act in a similar way unless the same conditions are spelled out in excessive detail. There are more salient differences between different democratic governments’ actions than there are between the body’s different presynaptic neurons’ release of neurotransmitters.

In order to rely on regularities, one requires what Gerring has called “descriptive comparability,” the causal factors *X* and effects of interest *Y* must “mean roughly the same thing across cases.”¹⁶ Finding descriptive comparability is particularly difficult to accomplish in the social sciences, since in social science we are dealing with aggregate, social, non-individual concepts,¹⁷ what elsewhere has been described as “fuzzy” or “Ballung” concepts.¹⁸ Consider, for instance, the activity concept *making public a blue book*, one of the key activities in the causal process that Kenneth Schultz claims linked the events behind Britain’s successful use of coercive

⁹ Darden 2008, 962.

¹⁰ Machamer, Darden, and Craver 2000, 3.

¹¹ Machamer, Darden, and Craver 2000, 3.

¹² Bogen 2005 and 2008.

¹³ Bogen 2005, 397.

¹⁴ Bogen 2005, 397.

¹⁵ Machamer, Darden, and Craver 2000, 3.

¹⁶ Gerring 2005, 184.

¹⁷ cf. Kincaid 2009.

¹⁸ Cartwright and Bradburn 2011, Cartwright and Runhardt 2014.

diplomacy during the Fashoda crisis.¹⁹ The blue book contained all the exchanges between Britain and France and communicated to the British public the uncompromising position of the government firsthand, which in turn made it difficult for the government to renege on their position without incurring substantial political costs. Although one may speak of this activity more generally (there have been other instances in which a government made public their position in such a way), there is no reason to assume that publishing a blue book happens always or for the most part in the same way under the same conditions, nor is there reason to assume that it always or for the most part has the same effect. Thus, it is difficult to see how a regularity, as defined by MDC, could be used to establish with certainty the causal productivity of Schultz's purported causal process.

Note that this is not to say that general principles like regularities can never play a role in finding evidence for the productive continuity of a social mechanism. We may rely on more general political science studies to establish that, indeed, renegeing on promises made in a blue book incurs great political costs. However, such principles won't take the form of MDC's regularities, e.g., that making public a blue book "works always or for the most part in the same way under the same conditions." I will outline, in section 3.3.1 of this paper, the role of such general principles in providing evidence for productive continuity.

The second reason to reject regularities as necessary for social mechanistic explanation is as follows. In biology, we may see a straightforward relation between one mechanism and the process observed in a particular instance, e.g. the mechanism of protein synthesis in general and the production of a protein in a particular cell under study. In the social sciences, things are less straightforward. Consider, for instance, Elisabeth Wood's study of mobilization into the Frente Farabundo Martí para la Liberación Nacional (FMLN) rebel forces during the Salvadoran Civil War.²⁰ Wood argues that rural people mobilized because of three mechanisms: because they came to value participation per se, because of "defiance" (a refusal to acquiesce), and because of "pleasure of agency" (the "positive affect associated with self-determination, autonomy, self-esteem, efficacy, and pride that come from the successful assertion of intention"²¹). Although each of the three mechanisms may individually recur in other case studies, and thus have some degree of generality beyond the El Salvador study, the chain of events in El Salvador was a unique result; the same *process* has not recurred in the same way in other conflicts. The causal mechanisms that Wood stipulates have interacted with the particular background conditions in El Salvador to produce this unique, single case and its entities' activities. To establish that the process in Wood's study has productive continuity, i.e. that each part of the mechanism is causally related to (produces) the next, one cannot rely on the straightforward relation between one mechanism and one pro-

cess as in biology.

To sum up, many of the processes traced in process tracing studies seemingly occur only once, and there is reason to believe that one can always find causally relevant differences between, for example, two conflicts, or one conflict at different points in time. Moreover, MDC's regularities often cannot support the productive continuity of a social mechanism due to a lack of straightforward descriptive comparability between activities. As such, regularities as described by MDC are, at best, a possible but not necessary source of evidence for productive continuity.

3.2 Productive continuity supported by observable implications

Regularities, then, often cannot provide evidential support for the productive continuity of social mechanisms in the way MDC claim they do for biological mechanisms. Let us now consider a second source of evidential support for productive continuity from the methodological literature.

Conceptualizing top-down process tracing, methodologists like Andrew Bennett and Jeffrey Checkel²² argue that one should first formulate a hypothesis about what may be the cause of an observed effect, and by what mechanisms cause and effect are connected. One should subsequently try to provide support for one's own hypothesis, as well as refute any existing rival hypotheses, in a case study, using the *observable implications* of the hypothesized mechanisms. The observable implications of the mechanism are generally called 'causal process observations' (CPOs) in the literature.²³ We can think of CPOs as the salient observations a process tracer uses to evaluate a causal hypothesis. Bennett and Checkel define the observable implications of mechanisms as "the facts and sequences within a case that should be true if each of the alternative hypothesized explanations of the case is true. Which actors should have known, said, and did what, and when? Who should have interacted with, worried about, or allied with whom?"²⁴

As an example, consider Kristin Bakke's 2013 study of tactical innovation during the Chechen wars.²⁵ Bakke hypothesizes transnational insurgents could have influenced local fighters through two mechanisms: relational and mediated diffusion. To show that mediated diffusion and relational diffusion were indeed behind the radicalization of tactics in Chechnya, Bakke deduces several observable implications that should be true if these mechanisms were present. First, she shows the right background conditions were present to make diffusion aimed at a radicalization of tactics possible (e.g. local fighters accepted the idea of direct attacks against civilians more over time); second, one of the most prominent hostage crises took place *after* training camps and schools had been set up by transnationals and, as such, the chronology fits; third, Bakke presents some evidence that radical tactics were

¹⁹ Schultz 2001.

²⁰ Wood 2003.

²¹ Wood 2003, 235.

²² Bennett and Checkel 2015.

²³ See Brady and Collier 2010.

²⁴ Bennett and Checkel 2015, 30.

²⁵ Bakke 2013.

being communicated to the local insurgents in these training camps and schools (e.g. evidence that hostage techniques were taught there and that videos were produced that taught suicide tactics).

The difficulty, however, with focusing on CPOs as evidence for process hypotheses, is that such evidence for the observable implications of mechanisms does not necessarily provide support for the *causal connection* between the steps of the process.²⁶ In other words, CPOs alone do not necessarily support the productive continuity of the mechanism. In Bakke, for instance, evidence for productive continuity is thin on the ground. All that is required for Bakke, it seems, is that we observe the deductive implications of her hypothesized mechanisms in a case study. Yet it is less satisfying to state, for instance, that schools and training camps were built between the arrival of transnational insurgents and the use of radical tactics, than it is to clearly link that it was in those schools and training camps that local insurgents became convinced that using, for example, suicide bombings is an effective and acceptable tactic.

In other words, if productive continuity is what one is looking for, CPOs are not the (best) way forward. CPOs do not commonly provide support for productive continuity, since they are not focused on providing support for the causal connection between the links of a causal chain. In general, listing the observable implications of a mechanism's entities and activities alone is not enough evidence to show that a putative cause and effect are indeed connected. Further evidence is necessary to show that each event on the chain is causally connected to the events that directly precede and succeed it.

3.3 Productive continuity supported by counterfactuals

Now that we have seen regularities are unlikely to provide support for the productive continuity of social mechanisms, and that the use of causal process observations alone is insufficient, in the remainder of this paper I will consider a third source of evidence process tracers may use to support productive continuity: counterfactuals, as per James Woodward's interventionist theory of causation.

Woodward has argued that any successful description of a cause-effect relationship must refer to causal factors that can be manipulated to change the phenomenon under study.²⁷ Specifically, X is a cause of Y if there exists some 'intervention' I that we can use to change X , so that X will then, in turn, change Y without any interference of other factors linked to Y . In other words, using I , we can ascertain that X made the change in Y happen. As I have shown in earlier work,²⁸ Woodward's theory implies that if we cannot specify an 'intervention' for each separate link of the chain of events, then these links are not genuinely causal, and the process-tracing argument will fail. Evidence for interventions can support the productive continuity of social mechanisms, because it gives evidence for the arrows in a process $A \rightarrow B \rightarrow C$.

Woodward's is a counterfactual theory of causation,²⁹ and methodologists have previously considered what counter-factual evidence one may employ to support causal claims in the social sciences.³⁰ A counterfactual is commonly defined to be a claim of the form "If it had been the case that C (or not C), it would have been the case that E (or not E)."³¹ These frameworks, however, lack the concrete (Bayesian) evidential tests discussed by later process tracing methodologists.³² Woodward's theory on the other hand can be adapted to provide such a test.

In the Bakke study discussed above, the Woodwardian recommendation would be to go beyond the observable implications evidence (the CPOs) and show that e.g. *if* the local insurgents had not watched instructional videos on suicide tactics, *then* they would not have used such tactics during later incidents. The intervention, here, is a technical way of conceptualizing the evidence one should provide to support this counterfactual.³³ We would need to come up with an intervention that prevents the locals from watching the videos, which in turn would prevent the use of such tactics. In particular, Bakke needs to ask, could we have prevented the local insurgents from watching suicide bombing videos, in a way that will not by itself increase the use of radical tactics? (If we can only prevent the locals from watching suicide bombing videos by giving them a different source of information on such tactics, this arguably implies that they would have been led to using radical tactics regardless of watching the videos.) Would they have used suicide bombings less if we had prevented them from watching such videos?

There are several kinds of interventions, according to Woodward's theory: the *actual* (human) intervention, the natural intervention, and the theoretical intervention. In the particular context of Bakke's work, a human intervention (akin to

²⁹ My demand for further counterfactual evidence for the links of the causal chain fits with Stathis Psillos' distinction between mechanistic and counterfactual causation, and his belief that mechanistic causal claims must rely on counterfactual causal claims (Psillos 2004).

³⁰ cf. Fearon 1991, Tetlock & Belkin 1996.

³¹ Fearon 1991, 169.

³² cf. Beach and Pedersen 2013; Bennett and Checkel 2015.

³³ In Woodward's framework, I is an intervention for this causal relation if, firstly, I causes viewing of videos of suicide bombings; secondly, I acts as a switch for the local insurgents' increased use of suicide bombings (i.e., makes whether the insurgents use this tactic independent of any other influences); thirdly, I does not directly or indirectly cause the increased use of suicide bombings itself; fourthly, I is statistically independent of any factor A not on the path $I \rightarrow Z \rightarrow Y$ that links the viewing of videos of suicide bombings, Z , to the increased use of suicide bombings, Y . One should show that all four requirements are met by a (theoretical) intervention if it is to be accepted as evidence for a process hypothesis. For more details, see Runhardt (2015). (These strict requirements put on an intervention also imply that even in cases of redundancy, we can assess whether the putative cause was indeed causally relevant. These cases are caught under the second requirement, which fixes other influences on the effect of interest. As such, being explicit about the requirements one puts on an intervention responds to Beach's (2016) objection that counterfactual accounts break down in cases of redundancy.)

²⁶ cf. Runhardt 2015.

²⁷ Woodward 2003.

²⁸ Runhardt 2015.

an experiment in which the putative cause is actually intervened upon, e.g. in which we physically prevent the locals from watching the videos) is impossible, and more generally speaking, many process tracing case studies are unlikely to be compatible with this technique. A natural intervention relies on finding a sufficiently similar case in which the cause was not present, to see what would happen to the effect. In Bakke's case, one would need to find a (set of) conflict(s) that are similar in every other way to the Second Chechen War, but where e.g. transnational insurgents are not present. For reasons mentioned above, it is unlikely that Bakke would find such a comparable case.

As such, the most likely type of intervention from Woodward's framework that may provide us with a concrete (Bayesian) evidential test is the *theoretical* intervention: a process tracer would have to establish the counterfactual claim of what would happen under an intervention *if it were to be put in place*. Woodward suggests that one way of doing so is through a hypothetical experiment. One would have to formulate an appropriate hypothetical experiment for the causal claim one is testing, following the definition of an intervention variable *I* have described above. Then, one would collect data that tells us "what the results of the (...) hypothetical experiment would be if we were to perform the experiment, although in fact we don't or can't actually perform the experiment."³⁴

As an indication of how one may go about such a hypothetical experiment, consider Bakke's study again. She makes her causal claim regarding diffusion more salient by a counterfactual remark: "Suicide terrorism, in contrast [to hostage taking], does not have a local historical template among the Chechens, despite centuries of conflict with central rulers. Thus in the absence of outside influence, it is unlikely that the Chechens would have turned to such a tactic."³⁵ But it is only a remark. Establishing this counterfactual would be one way of corroborating what would happen under an intervention if it were to be put in place, i.e. what would happen if the Chechens had not been influenced by outside agents.

In order to establish this counterfactual, Bakke's assumption must be that the Chechens before the arrival of the transnational insurgents are sufficiently similar to the Chechens after the arrival of the transnational insurgents, and thus that the diffusion mechanisms that the transnational insurgents set in motion are the only cause of radicalization. Therefore, if there had been no transnational insurgents, nothing else would have caused the radicalization, and we would not have seen any use of suicide terrorism.

Note that, in order to support such a detailed hypothetical intervention claim, and avoid black-boxing the links between watching videos and using suicide tactics,³⁶ it is crucial that one is as detailed as possible. One must make explicit "what it is about one part that links it in a causal sense to the next part."³⁷

³⁴ Woodward 2015, 3587.

³⁵ Bakke 2013, 58.

³⁶ Beach's (2016) criticism, in this symposium, is that counterfactual understandings of mechanisms black-box or mask causal logics.

³⁷ See Beach (2016) in this symposium.

Now let us briefly consider how a theoretical intervention may fit in with the evidential framework of later process tracing methodologists, particularly with the Van Evera terminology adopted by, amongst others, Beach and Pedersen, and Bennett and Checkel.³⁸

Woodward's framework for causation tells us that if an intervention for the relation $X \rightarrow Y$ can be shown to exist, *X* causes *Y*. This definition, in Woodward's theory, means that if one finds an intervention *I* which answers to these criteria, it is *unavoidable* that *X* causes *Y*. In theory, our subjective probability in that causal claim should be raised to 1. Finding an intervention is a sufficient but not necessary test that substantially raises our subjective probability for the causal claim. Failure to find an intervention does not give us a reason to adjust our subjective probability for the causal claim one way or the other. As such, Woodward's intervention fits perfectly with the *smoking gun* test.³⁹

There are, however, limitations to the use of interventions as described above. Though in theory finding an intervention should raise our subjective probability in a process hypothesis to 1, in practice, finding an intervention will never actually *confirm* the process hypothesis. We will raise our confidence in the process hypothesis more or less depending on the quality of our evidence for the intervention. How much our confidence is raised should be assessed on a case-by-case basis, and how one ought to determine this is an important step for future work on process tracing methodologies. However, interventions remain a key source of evidence for the productive continuity of social mechanisms when compared to such sources as regularities and CPOs.

3.3.1 Are counterfactuals supported by regularities?

Before finishing, let me anticipate one potential objection to using counterfactual support for productive continuity. Authors like Fearon argue that the best support for counterfactuals comes through "invoking general principles, theories, laws, or regularities."⁴⁰ A critic could thus suppose that getting counterfactual evidence for productive continuity is no different from using regularities as per MDC's framework. This would undermine my argument against regularities, since counterfactuals would then simply be regularities in disguise.

The answer to this criticism lies in the way we interpret Fearon's "regularities." A straightforward regularity like the ones used in biology (e.g. that depolarization will work in the

³⁸ Van Evera 1997, Beach and Pedersen 2013, Bennett and Checkel 2015.

³⁹ According to Van Evera, we can use the predictions a claim implies (e.g., the predicted existence of an intervention) to test whether that claim explains a causal relation satisfactorily. A smoking gun test is a set of "predictions of high uniqueness and no certitude" (Van Evera 1997, 40). Passing a smoking gun test strongly corroborates a claim (due to its uniqueness), but failing to pass such a test does not strongly undermine a claim. After all, as Van Evera says, "suspects not seen with a smoking gun are not proven innocent." (Van Evera 1997, 40). Finding evidence for an intervention can strongly corroborate a causal claim.

⁴⁰ Fearon 1991: 176.

same way in different places or cases under the same conditions) is inaccessible for singular social science cases. The support referred to by Fearon must therefore be of a different type, and indeed it is. Consider Fearon's example of a general principle one might invoke in describing the influence of nuclear weapons on the postwar world (from John Mueller's 1988 study): "Wars are not begun out of casual caprice or idle fancy, but because one country or another decides that it can profit from (not simply win) the war—the combination of risk, gain, and cost appears preferable to peace."⁴¹ Comparing this general principle to the MDC regularity that presynaptic neurons, in the same circumstances, always release neurotransmitter molecules in a certain way indicates the difference between support for the productive continuity of biological and social mechanisms. The former is exacting, describing a particular cause-effect relationship that recurs in the same way under the same conditions. The latter is at best a guiding general principle, not a specific recurring relationship. As such, the demand for counterfactual support is not simply the call for regularities as per MDC in disguise. Indeed, following my logic in the preceding section, I would argue that the use of general principles as in Mueller's study are only supportive of productive continuity if such general principles can be used to support a specific intervention claim that implies that if the cause had not been present, the effect would not have followed, all other things being equal.

4. Conclusion

In this paper, I have analysed three kinds of support for assessing the productive continuity of social mechanisms: regularities, causal process observations, and interventions. I have shown that regularities, because of the dissimilarities between social and biological activities, often cannot provide support for social mechanisms' productive continuity. I have also shown that causal process observations do not commonly provide support for productive continuity, since they are not focused on providing support for the causal connection between the links of a causal chain. Finally, I outlined how counterfactual evidence can provide support for social mechanisms, in the form of Woodward's interventions, and finished with some concrete requirements that this evidence must meet. Future work should determine exactly how Woodwardian interventions can provide a smoking gun test for process tracing hypotheses.⁴²

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⁴¹ Fearon takes this example from Mueller's 1988 study. Mueller 1988: 68–69, as cited by Fearon 1991: 184.

⁴² This work should further highlight that the level of detail required in interventionist evidence avoids black-boxing or masking the logics of a causal process. See footnote 36.

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Invariant Causal Mechanisms

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Why Causal Mechanisms are Not Variables

Qualitative methodologists generally treat process tracing methods and a mechanistic view of causation as natural allies. Two conjoined propositions form the basis for this alliance. The first proposition is that the identification of causal mechanisms is the *sine qua non* of distinguishing causal relations from mere correlations. The second proposition is that process-tracing methods are uniquely qualified to identify these critical causal mechanisms. In one admirably pithy formulation, Gary Goertz and James Mahoney state categorically: "No strong causal inference without process tracing."¹ There appears to be a tacit consensus that process tracing is both necessary and sufficient for causal inference.

One can applaud the development of process-tracing methods without making such strong claims on its behalf. It would behoove us, first of all, to distinguish causal inference from causal explanation. We make inferences about the existence of a causal relationship by claiming that an observed association is not merely correlational. We can do this qualitatively, by making claims about necessary and sufficient conditions, or we can do this quantitatively, by making claims about unbiased estimates of causal effects. Philosophers who are in general sympathetic to mechanistic conceptions of causation have been quick to point out that inferences can and frequently are made without explicit reference to causal mechanisms.² Even as we affirm that the analysis of causal mechanisms can be a powerful tool in causal inference, there is no reason to reject experiments and design-based statistical studies as equally powerful instruments of causal inference.³ We make causal explanations, on the other hand, by invoking causes. In previous work, I have argued that a complete causal explanation

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¹ Goertz and Mahoney 2012, 103. See also Bennett and Checkel 2015.

² Steel 2004.

³ Dunning 2012.

requires the identification of the relevant mechanisms, but even on this point, philosophers sympathetic to a mechanistic perspective on mechanisms dissent; Harold Kincaid, to give one example, has argued cogently that no universal claim for mechanism-based explanation is reasonable.⁴

Moreover, reading various literatures on the discovery of causal mechanisms in disparate scientific disciplines makes it hard to credit the claim that "process-tracing methods are arguably the only method that allows us to study causal mechanisms."⁵ It may be the case that process-tracing methods are particularly well-suited to studying mechanisms in social science fields like Political Science, but this claim requires an auxiliary argument about why the social sciences differ from natural sciences and why that difference makes process tracing particularly well suited for studying social science mechanisms.

Finally, it is worth revisiting the vexing question of whether our current conceptual understanding of causal mechanisms is adequate to the tremendous inferential and explanatory burdens we process tracers place upon them. In a 2001 review essay, James Mahoney identified approximately two-dozen definitions of causal mechanisms. Somewhat alarmingly, Mahoney noted that many of these definitions do not clearly distinguish causation from correlation.⁶ In the intervening years, political scientists have made some important progress defining causal mechanisms, yet skeptics remain, even among political scientists who are sympathetic to process tracing.⁷ Much more work remains to be done: a recent commentary on process-tracing methods by a sympathetic philosopher concludes that "the social science literature has been preoccupied more with how mechanisms are found or theorized, and with the role they play in explanation and theory, and less with developing a definition that captures the essential elements of mechanisms or that applies to all scientific contexts."⁸

Yet it is not clear that we can make much progress by way of definition. Many of us, myself included, endorse a "generative" definition of causal mechanisms, as entities that generate an outcome of interest.⁹ There are various definitions in the philosophical literature that expand on this core definition, but all of them ultimately contain some version of the claim that causal mechanisms *produce* an outcome. Thus, we end up defining causation in terms of causal mechanisms and causal mechanisms in terms of generating or producing outcomes—that is to say, in terms of causation.

My recommendation is that we put an end to worrying about a unified and singular definition of causal mechanisms and that we instead focus on what *property* a mechanism must possess in order to perform its inferential and explanatory functions. By distinguishing properties from functions, my inten-

⁴ Kincaid 1996, 181; Waldner 2007.

⁵ Beach and Pedersen 2013, 1–2.

⁶ Mahoney 2001.

⁷ Gerring 2010.

⁸ Runhardt 2015, 453.

⁹ Mahoney 2001, 580; George and Bennett 2005, 137; Waldner 2012.

tion is to advance the debate without trafficking in tautology. The property I wish to emphasize is *invariance*. Definitionally, invariance is a property of a system that remains unchanged under some transformation. For example, the area of a surface remains unchanged if the surface is rotated in space, a property known as rotational invariance. In my usage, invariance is a property of an entity in a causal system that remains unchanged under intervention. We intervene on variables, setting them to new values; that intervention produces changes in other variables that are causal effects of the variable on which we intervened. Stated colloquially, wiggle the cause and the effect wiggles too; flip a light switch and the light turns on and off. To refer to mechanisms as possessing the property of invariance, then, is to claim that mechanisms are those elements of a causal system that we cannot wiggle.

To explore invariance further, let's highlight what it is not. First, invariance is not determinism. Determinism is a property of a causal relationship between singular events (token causation) or variables (type causation).¹⁰ To claim that mechanisms are invariant neither implies determinism nor precludes it; to insist that invariance is equivalent to determinism is to make a category error.

Furthermore, mechanisms embodying the property of invariance are neither intervening variables nor systems of variables connected by arrows. Andrew Bennett, in this symposium, attributes to me the idea that mechanisms are systems of variables connected by arrows,¹¹ but as the following discussion should make clear, I do not subscribe to either view of causal mechanisms. To see why not, it will be helpful to invoke the concept of a causal graph. A causal graph is nothing but a set of vertices or nodes, representing random variables, and a set of edges, representing postulated relations of statistical dependence. When edges have arrowheads, they denote direction and asymmetry; arrows represent postulated causal relations. Finally, when a path from a node does not return to itself, the graph is acyclic. These causal graphs are thus directed acyclic graphs.

It is common among both philosophers and social scientists to see references to causal mechanisms as mediators in a causal graph, as in $X \rightarrow M \rightarrow Y$, where X and Y are the independent and dependent variables, respectively, and M is the mechanism.¹² There is great value in thinking about causal relations in terms of mediating variables. Doing so produces more extensive causal chains that have the methodological virtue of increasing the opportunity for falsification and the explanatory virtue of providing richer detail and depth. But it seems futile to identify these mediators with causal mechanisms, for they simply reproduce the relationship of correlation between variables that we are seeking to transcend. Indeed, there is nothing ontologically distinct about mediators as a type of random variable; whether a variable is a mediator

or not depends on nothing but the construction of the causal model and the placement of the nodes. A variable that is a mediator in one causal graph may just as easily be a non-mediator in another.

Alternatively, we can think of the entire causal graph as a causal mechanism in terms of a system of interacting parts. This view is widely held among philosophers, especially in the literature on biological mechanisms. Rosa Runhardt, in this symposium, adopts this view, citing Machamer, Darden, and Craver 2000 to refer to the causal structure $A \rightarrow B \rightarrow C$ as the schematic representation of a mechanism, with each of A , B , and C conceptualized as independent stages of the mechanism.¹³ Stuart Glennan defines a mechanism for a behavior as “a complex system that produces that behavior by the interaction of a number of parts, where the interactions between parts can be characterized by direct, invariant, change-relating generalizations.”¹⁴ Social scientists have followed suit. Carly Knight and Christopher Winship define a mechanism as “a causal relationship involving one or more intervening variables between a treatment and an outcome.”¹⁵ Proponents of this approach, both philosophers and social scientists, offer the toy model of a car's engine as this type complex mechanism or system. As Alexander Gebharter explains, “The question of why a car speeds up when the gas pedal is pressed can be answered by pointing at/describing the underlying mechanism (i.e., the motor and how it is connected to the gas pedal, the wheels, the gas tank, etc).”¹⁶ Derek Beach and Rasmus Pedersen offer the identical analogy, with X as the motor, Y the car's movement, and “the driveshaft and wheels can be thought of as the causal mechanism that transmits forces from X (motor) to produce Y (movement).”¹⁷

Indeed, an automobile's mechanical components comport well with the idea of a complex system of interacting parts whose interactions can be characterized by direct, invariant, change-relating generalizations: under most circumstances, step on the accelerator and the car moves faster. But something critical is missing from this conception of mechanisms and the toy model that instantiates it. Most automobiles are still powered by gasoline and gas-powered engines are internal combustion engines. To think of the mechanical parts of an engine as the causal mechanism is to exclude combustion from the causal explanation of how automobiles work. We can say something further: the mechanical parts of an automobile are variables, and as variables, we can intervene to turn each variable to a new value. Combustion, on the other hand, cannot be turned off; it is an exothermic chemical reaction that occurs under proper conditions. Each of those conditions—heat, oxygen, and a fuel—can be turned on or off, just as any random

¹⁰ Many philosophers claim that all instances of type causation can be reduced to token causation. I agree, but see little harm in making claims about causation involving variables.

¹¹ Bennett 2016.

¹² Gerring 2008; Kincaid 2012.

¹³ Runhardt 2016.

¹⁴ Glennan 2002, S344.

¹⁵ Knight and Winship 2013, 283.

¹⁶ Gebharter 2014, 139.

¹⁷ Beach and Pedersen 2013, 30. Without invoking the theory of causal graphs, Beach and Pedersen represent causal mechanisms precisely by drawing boxes connected by arrows and referring to the entire system as a causal mechanism. See their Figures 4.2 and 4.3 for examples.

variable in a causal graph can be turned on or off. But if heat, oxygen, and a fuel are all present, combustion occurs. One cannot throw a lit match into a pool of gasoline in the presence of oxygen and the absence of any other flame retardant and then somehow intervene to turn off combustion. That makes combustion different from a random variable, for in a causal graph, to repeat, one can, in principle, intervene to set any of the variables to a new value.

In what way does combustion differ from the interconnected parts of an engine? As I describe below, combustion has the property of invariance. We invoke mechanisms to explain relations between variables; to perform this function, mechanisms cannot themselves be variables. Relations between variables, after all, are described by correlations, and we typically believe that causal inference and explanation are based on something beyond correlation. Therefore, we cannot be satisfied by defining mechanisms as intervening variables or as systems of variables.

What does it mean to define mechanisms as entities featuring the property of invariance? To understand invariance, we must recognize the critical distinction between *causation* and *constitution*. Causation refers to a particular type of relation of dependence between events (which can be coded as variables, at the aggregate level). Causal relations are temporal—they occur over time—and asymmetric, in that the manipulation of a cause produces an effect but the manipulation of the effect does not produce the cause. Causation implies etiology, in that we explain outcomes by detailing the causal path that led to them. Constitution, on the other hand, is about the fundamental nature of a phenomenon. As such, constitution is atemporal and provides no opportunity for manipulation. Consider the prosaic example of a batted baseball that strikes and shatters a glass window. The causal story has two events, the striking of the glass by the ball and the subsequent shattering of the glass. The two events are separated in time, albeit only briefly separated. There is asymmetry of manipulation: by striking the glass with the ball, the glass shatters; but shattering the glass does not cause it to be struck by the ball. The causal mechanism, on the other hand, is the inherent fragility of the glass, which is constituted by its molecular structure. “To be fragile is to have a particular molecular structure; the fragility is not a consequence of the molecular structure.”¹⁸ Similarly, combustion takes place when a fuel-oxygen mixture is exposed to heat due to the structure of dioxide molecules (their angular spin and momentum) such that oxygen molecules assault hydrocarbon molecules, converting them into molecules of water and carbon dioxide and releasing tremendous amounts of heat in the process.

Invariant Causal Mechanisms and Qualitative Causal Inference

Where does this discussion of invariant causal mechanisms get us? Elsewhere I have argued that process tracing should satisfy a “completeness standard,” consisting of four elements: a relatively complete causal graph representing the process

being traced, an event-history map representing the events of a particular case that correspond to a particular realization of the causal graph, a set of descriptive inferences from the event-history map to the causal graph (it is here that Bayesian updating can be extremely useful), and, finally, a full set of causal mechanisms.¹⁹ What constitutes a full set of mechanisms? The arrows in the causal graph that connect random variables denote the underlying causal mechanism. We can intervene on random variables but we cannot intervene on the causal mechanisms that generate causal connection. This is precisely the meaning of invariance. Thus, for a causal graph with N directed edges (connecting $N + 1$ random variables, in many cases), there must be N causal mechanisms.

Let me give one quick example that should help us distinguish causal graphs from causal mechanisms. Figure 1 is the causal graph that I have previously reconstructed from Elisabeth Wood’s analysis of insurgency and democratic transitions in El Salvador and South Africa.²⁰ The graph has seven nodes, each representing a random variable, and six directed edges, each connecting two of the random variables. By my account, this causal graph has to be supplemented by event-history maps supported by evidence (individual chapters of the book perform this task) and by six invariant causal mechanisms, one mechanism for each arrow. Let’s consider one such mechanism linking the third and fourth nodes in the graph: by what mechanism does insurgent collective action reshape economic structures to make them less reliant on the repression of labor? In her African case study, Wood articulates this causal mechanism as a decision-theoretic model of how investor confidence in the profitability of apartheid institutions was eroded by worker collective action leading to the suspension of investment in South Africa by both domestic and foreign capitalists. Wood derives two states of the world from the comparative statics of the model, one in which the political control of labor keeps wages below what they would be in a liberal market economy and one in which worker mobilization raises wages to above their level set by a liberal market economy. Worker mobilization thus changes elite preferences over labor market conditions and hence over political regimes. Wood then compiles considerable evidence that her model best depicts actual events, and is thus superior to several plausible alternative mechanisms.²¹

This brief summary does not do justice to either Wood’s theoretical model of the causal mechanism or her empirical confirmation of the mechanism. But it is sufficient to support two claims. First, causal mechanisms are distinct from either mediating nodes in a causal graph or the entire set of nodes in the causal graph. Second, causal mechanisms are invariant insofar as their fundamental constitutive features constrain their actions. Wood, like other advocates of rational-actor models, conceives of human beings as fundamentally endowed with intentionality and rationality and hence acting in ways

¹⁹ Waldner 2015a, 2015b, 2015c.

²⁰ Wood 2000. For my discussion of this work as an exemplar of process tracing, see Waldner 2015a, 137–141.

²¹ Wood 2000, 143–168.

¹⁸ Ylikoski 2012, 34; Ylikoski 2013.

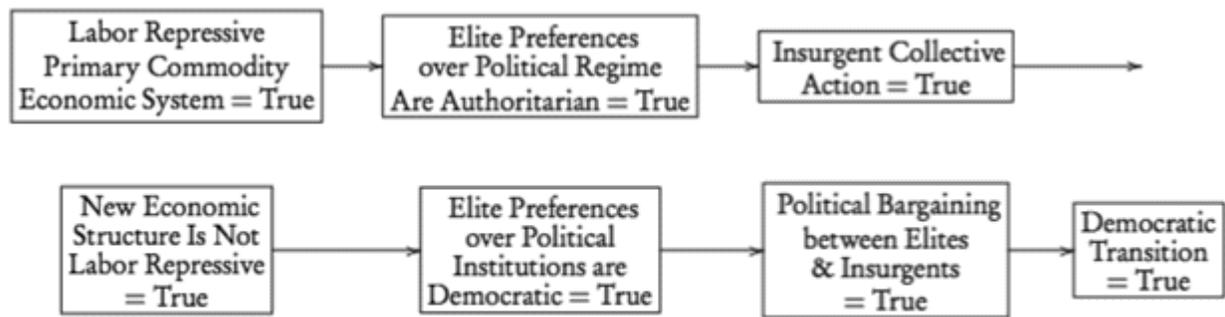


Figure 1: Wood's Causal Graph of Post-Insurgency Democratic Transition in El Salvador and South Africa

that maximize some objective function. This is not the only possible interpretation of how the constitution of agency constrains action and so I concur with Rosa Runhardt's observation, in this symposium, that "units in social science mechanisms are not as clearly defined as biological entities."²²

What is the payoff of conceptualizing process tracing in terms of causal graphs, event-history maps, and invariant causal mechanisms? Doing so gives process tracers a principled response to the fundamental problem of causal inference. The fundamental problem of causal inference is derived from the Rubin Causal Model, also known as the potential-outcomes framework, which defines a causal effect as the difference in a response variable between a unit under treatment and the same unit under control. The problem of inference at the unit level is a problem of missing data: one cannot observe a unit under treatment and under control simultaneously; one or the other state of the world cannot be observed and so one cannot observe the two quantities needed to infer the causal effect. This claim, if left uncontested, would seem to undermine the possibility of within-case causal analysis. Many qualitative scholars simply ignore this fundamental problem of causal inference, while others explicitly reject it as part of a quantitative perspective that is simply not relevant to the concerns or the philosophical commitments of qualitative scholars.²³

My approach to process tracing—the combination of causal graphs, event-history maps, and invariant causal mechanisms, aka the "completeness standard"—is based on the presupposition that process tracing would only be strengthened by directly engaging the fundamental problem of causal inference. The outlines of a principled response to the fundamental problem of causal inference would rely on what Paul Holland calls the "scientific solution," a third option to experiments with random assignment to treatment and design-based statistical inference. The scientific solution relies on exploiting "various homogeneity or invariance assumptions."²⁴ My claim is that a fully specified causal model that is fully identified by the

²² Runhardt 2016.

²³ See Goertz and Mahoney (2012) for the claim that qualitative and quantitative approaches represent two distinct cultures. See Beach and Pedersen (2016, 3) for the claim that case-study researchers should eliminate the "vestiges" of variance-based thinking embedded in such terms as "independent and dependent variables."

²⁴ Holland 1986, 948.

relevant invariant causal mechanisms can be used to satisfy this invariance assumption. Insofar as one has the full set of invariant causal mechanisms, one can use the fully identified causal model as a substitute for the missing observations about the counterfactual states of the world. To use the informal example favored by Donald Rubin, we can make unit-level causal inferences about the effects of aspirin on relief from the pain of a headache because we can produce causal graphs representing pathways from damaged cells to the production of a chain on enzymes to signals sent to the pain center of the brain, and also because we understand the nature of neurotransmitters and their receptors and how their activities are constrained by their fundamental structures. We can, in other words, tell the causal story in terms of etiology and in terms of constitution, and, in doing so, we can resolve the fundamental problem of causal inference.²⁵

Response to Andrew Bennett's Comments in This Symposium

In his contribution to this symposium, Andrew Bennett doubts my claim that the completeness standard constitutes a potential solution to the fundamental problem of causal inference.²⁶ His critical interrogation has two components, a discussion of mechanisms and a discussion of evidence. On mechanisms, Bennett equates my approach to mechanisms to Judea Pearl's explication of causal graphs and to James Woodward's manipulation account of causation.²⁷ I emphatically dissent from this characterization of my position. I consider causal mechanisms to be ontologically distinct from causal graphs; indeed, that distinction must be made since the completeness standard calls for both causal graphs and causal mechanisms, with one mechanism corresponding to each arrow in the causal graph.

Let me say a few more words to explain why I consider causal graphs and causal mechanisms to be irreducibly distinct elements of a causal explanation. In Pearl's account, causal graphs represent relations of probabilistic dependence; these

²⁵ This is obviously an abridged version of how invariant causal mechanisms can satisfy an invariance assumption and permit unit-level causal inference. For the full account, see Waldner 2015c.

²⁶ Bennett 2016.

²⁷ Pearl 2000; Woodward 2002 and 2003.

graphs are causal only insofar as we invoke the Causal Markov Condition, which states, informally, that in the causal graph $X \rightarrow M \rightarrow Y$, the value of Y depends only on its direct causal relation to M such that after conditioning on M , Y is statistically independent of X . Let me state emphatically: At no point in my account is causal inference predicated upon the Causal Markov Condition. I consider probabilistic dependence to be only a consequence of a causal relationship and not constitutive of that relationship.

Woodward, on the other hand, presents a manipulationist account of causation. Recognizing that many relations of counterfactual dependence are not causal in nature, Woodward restricts causal relations to a subset of relations of counterfactual dependence such that X is a cause of Y only in the case that an ideal intervention on X —setting it to a new value—also changes the value of Y without changing the value of any other variable that is not on the pathway between X and Y . Woodward occasionally refers to his account as mechanistic. Yet the key distinction that Woodward preserves is between *difference-making* accounts of causation and *production* accounts of causation.²⁸ Woodward's account is unambiguously a difference-making account, which does not assign any role to mechanisms, while my own is a production account that requires causal mechanisms. One possible source of Bennett's mistaken conflation of my approach with Woodward's is that I use the term *invariance*, a term that is central to Woodward's account as well. Yet we use that term in very distinct ways. For Woodward, invariance is a predicate of a generalization; empirical generalizations linking X and Y are causal if they are invariant under a range of interventions. In my account, on the other hand, invariance is a predicate of mechanisms; it is for this reason that I invoke the fundamental distinction between causation and constitution, as I explain above.

While I reject the characterization of my approach to process tracing as reducible to the accounts offered by Pearl and Woodward, I do see my project of qualitative causal inference as affiliated with work by philosophers that seeks to integrate difference-making and production accounts. I would continue to insist, however, that production accounts are more fundamental; we can logically derive relations of counterfactual dependence from causal mechanisms but I do not believe that the converse is necessarily true.²⁹

But to make ontological claims about mechanisms is not to deny the epistemological relevance of counterfactuals. There is a big difference between the claim that causation consists of a particular type of counterfactual dependence, a claim that I reject, and the claim that causation consists of causal mechanisms whose productive capacities logically imply counterfactual dependence, a claim I endorse. Therefore, when process tracers invoke mechanisms, it should not give them license to ignore the fundamental problem of causal inference, which is a fundamental problem precisely because observations about counterfactuals are missing data. Process tracers

do themselves no favor when they reject the fundamental problem of causal inference as relevant only to quantitative analysis. The problem is a logical consequence of any causal relationship, regardless of the method used to investigate that relationship.

In his contribution to this symposium, Bennett also expresses concern about the adequacy of the completeness standard, concern that stems from his skepticism that the standard sufficiently addresses the *practice* of process tracing. Bennett has done more than anybody to develop the practice of process tracing over the past dozen years and so his comments deserve very close attention. I believe his most fundamental point is that I underestimate the extent to which "explanations of cases and populations will always be potentially fallible." Using slightly different language, however, I made the same claim in the chapter I contributed to his recent edited volume, where I wrote that the standard is intended to justify the claim that a causal explanation "has weathered sufficient scrutiny relative to its rivals and to the current state of theory and data gathering that belief in its approximate truth is more reasonable than disbelief but is also *subject to revision in the face of future data gathering or theorizing*."³⁰ I believe we agree on this point.

Furthermore, I think Bennett's claim betrays some misapprehension of the goal of the completeness standard. The goal of the standard is not to achieve fallibility and certainty. The goal of the standard is to justify unit-level causal inference in light of the fundamental problem of causal inference, which denies that such inferences are possible. The appropriate contrast, then, is not certain inference versus uncertain inference but rather valid inference versus invalid inference. Insofar as the standard accomplishes its goal, it permits unit-level causal inferences that, *like all inferences, will be fallible*; that is to say, we will always be uncertain about our inferences precisely because they are inferences. Indeed, the standard allows us to form a typology of the sources of uncertainty; we may have uncertainty about the causal graph itself, uncertainty about inferences from the event-history maps to the causal graphs, and uncertainty about the identification and validation of causal mechanisms. Uncertainty about any element of the standard—concerns about the sufficiency of the causal graph, about the degree of evidentiary support for the event-history maps, or uncertainty about mechanisms—will translate directly into uncertainty about the explanation. But, to use a Bayesian framework, we can still update our posterior beliefs about the validity of the explanation.³¹ All I wish to say at this point is to make the conditional claim that *if* the standard is met, then unit-level causal inference can be justified.

Therefore, I agree with Bennett that no explanation is ever truly complete.³² We make fallible judgments about the suffi-

³⁰ Waldner 2015a, 129–130, emphasis added.

³¹ Our updated beliefs will be a distribution. I thank Alan Jacobs for helping me think about this point.

³² Ironically, I coined the term "completeness standard" after reading some of Bennett's early and still wonderfully influential work with Alexander George on process tracing, in which he claimed that

²⁸ See especially Woodward 2011.

²⁹ Waldner 2012.

ciency of a causal graph, about the adequacy of evidence for descriptive inferences, about the credibility of alternative hypothesis, and about the significance of omitted variable bias. Bennett's work is absolutely invaluable in thinking about these crucial topics. The completeness standard is an aspirational standard against which we judge specific process-tracing research and therefore judge it as more or less complete and therefore more or less supportive of a valid causal inference. It is true that all causal models simplify and thus leave some relevant causal knowledge unstated; causal graphs, after all, encode qualitative relationships. Bennett criticizes me for not stating explicitly that combustion requires gravity; but if we take Bennett at his word, all of our causal arguments are going to be burdened by enumerating relevant background conditions like gravity, oxygenation, and pretty much all physical laws. Causal models may also, of course, suffer from omitted variable bias; this is the existential fact of all science.³³

Bennett thus raises critical points that have not yet been fully addressed in the completeness standard. I believe his concerns can be accommodated in future iterations of the standard, in part by drawing on his work, and so I thank him deeply for raising them. I do not agree, however, that these concerns add up to a fundamental critique of the approach as a standard of causal inference.

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"[A]ll the intervening steps in a case must be predicted by a hypothesis or else that hypothesis must be amended..." See George and Bennett 2005, 207. This type of continuity criterion has distinguished pedigree in the philosophical literature and forms the basis for Runhardt's (this symposium) criterion of "productive continuity."

³³ Bennett is critical of my approach for omitting an error term from causal graphs. Given that my approach is qualitative, it is not clear what adding that term accomplishes, for we do not estimate standard errors and confidence intervals and we do not make assumptions about the distribution of errors. But it is easy enough to add an exogenous error term to remind readers of possible omitted variables.

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Do New Accounts of Causal Mechanisms Offer Practical Advice for Process Tracing?

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When Alexander George and I wrote our 2005 book, *Case Studies and Theory Development in the Social Sciences*, a central theme was that theories about causal mechanisms, and process tracing analysis of the observable implications of hypothesized causal mechanisms, were central to qualitative case study research. Our emphasis on causal mechanisms was not especially novel at the time, but it ran counter to the preeminent role that Gary King, Robert Keohane, and Sidney Verba devoted to causal effects, and in the intervening years causal mechanisms have gained added attention from social scientists.¹

At the time we wrote our book, we based our definition of causal mechanisms largely on that of Wesley Salmon, the philosopher of science whose concept resonated most closely with our own understanding of mechanisms. Being political scientists rather than philosophers of science, we were not aware, in those days before Google Scholar, that James Woodward, building on his earlier work and on work by Judea Pearl, had critiqued Salmon’s definition and pushed forward the philosophical discussion of causal mechanisms.²

The present symposium on causal mechanisms is thus an opportune time to examine whether the latest philosophical discussions of causal mechanisms offer practical advice for political scientists engaged in process tracing. In particular, two suggestions for improved process tracing practices deserve attention: 1) the construction of directed acyclic graphs³ and the use of such graphs to improve upon and assess the completeness of explanations based on process tracing;⁴ and, 2) the use of counterfactual analysis in process tracing.⁵

The present article first notes the differences between the understanding of causal mechanisms that emerges from Wood-

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¹ King, Keohane and Verba 1994.

² Woodward 2003; Pearl 2000.

³ Pearl 2000; Knight and Winship 2013.

⁴ Waldner 2012, 2015, and 2016.

⁵ Runhardt 2015. For present purposes I do not address recent contributions to more formal Bayesian process tracing (Bennett 2015, Fairfield and Charman forthcoming), which I believe are beginning to offer very useful guidelines for process tracing even if further research is needed on how much and what kind of Bayesian formalization is practically useful in carrying out and writing up process tracing research.

ward’s manipulationist account of causation and the definition George and I used in 2005. It then assesses whether and how David Waldner’s account of the potential completeness of explanations, building on Pearl’s work and also related to Woodward’s conception of “invariance,” and Rosa Runhardt’s approach to using counterfactual analysis, building on Woodward’s concepts, offer practical advice for process tracing in political science case studies.⁶ I conclude that Waldner’s account demands more than process tracing can deliver, and Runhardt’s suggestion needs additional development to systematize the kinds of counterfactual analysis that process tracers already carry out. Yet these are promising lines of development that deserve continued research.

Defining Causal Mechanisms: Wesley Salmon versus James Woodward

In the 1980s and later, Wesley Salmon developed what is known as the *causal mechanical* approach to causal explanation.⁷ In this account, a causal process is a physical process that transmits what Salmon termed a *mark* between one physical entity and another. A mark is a change in physical structure that persists beyond the interaction of the first and second entity in the absence of any further causal interactions. Building on Salmon’s discussion of causal mechanisms, Alexander George and I defined causal mechanisms as “ultimately unobservable physical, social, or psychological processes through which agents with causal capacities operate, but only in specific contexts or conditions, to transfer energy, information, or matter to other entities, [thereby changing] the affected entity’s characteristics, capacities, or propensities in ways that persist until subsequent causal mechanisms act upon it.”⁸ We noted the obvious inferential challenges of measuring the effects of particular mechanisms, isolating them from other mechanisms, and assessing the conditions under which particular mechanisms are activated, and we discussed the ways in which process tracing could address (but not in any ultimate or decisive sense resolve) these inferential challenges.

Given the lags involved in researching, writing, and publishing our book, and given that George and I were autodidacts in the philosophy of science, we were not aware that James Woodward⁹ had already convincingly critiqued Salmon’s definition. Woodward notes that Salmon’s account of mark transmission encounters problems in explaining, for example, the motion of billiard balls after their collision, which relies on knowing the masses and velocities of the balls before the collision, the assumption of a perfectly elastic collision, and the hypothesized mechanism of the conservation of linear momentum. As Woodward points out, Salmon’s account of mark transmission does not distinguish between the explanatorily relevant momentum of the balls and explanatorily irrelevant features like the transmission of a chalk mark from one ball to

⁶ Runhardt 2016; Waldner 2016.

⁷ Salmon 1989.

⁸ George and Bennett 2005, 137; see also Bennett and Checkel 2015, 12.

⁹ Woodward 2003.

another.¹⁰ Capturing this distinction, Woodward argues, requires an appeal to a counterfactual like that invoked by his own manipulationist account (that is, would the post-collision course change if there were no chalk mark, or would it change if one ball's pre-collision momentum was different).¹¹

Woodward's manipulationist account, in its simplest form, is that "if it is possible to manipulate Y [or, he adds *seriatim*, the probability distribution of Y] by intervening on X, then we may conclude that X causes Y, regardless of whether the relationship between X and Y lacks various other features standardly regarded as necessary for causation: even if X is not connected to Y via a spatiotemporally continuous process, even if there is no obvious sense in which there is a transfer of energy from X to Y, and so on."¹² This avoids the philosophical problems with Salmon's approach, and it is appealing to social scientists as we hope to find variables that policy makers can reliably manipulate to create a better social world.¹³ Of course, it is a definition of causation and explana-

¹⁰ Woodward 2003, 351–352.

¹¹ Woodward (2003, 353) adds a second critique, which is that causal mechanical accounts cannot address "action at a distance" processes that do not allow tracing of spatiotemporal processes (like gravity); a possible rejoinder is that the mechanisms behind gravity and other action at a distance phenomena are not sufficiently understood to allow tracing of their processes (and perhaps never will be sufficiently understood for this—the related physics theories collectively known as "string theory" do not as yet have any testable observable implications). Woodward notes further that "most explanations in disciplines like psychology and economics" do not involve transfer of energy or momentum, but Alexander George's and my definition is not as vulnerable to this critique as Salmon's, as we added to Salmon's account the transfer of information between entities as a kind of mechanism. Woodward's final critique is that the causal mechanical model faces difficulties when interactions are too complex or numerous to trace, such as the interactions of the numerous molecules that make up a mole of gas. A possible response to this critique is that the mechanistic approach posits a model of how molecules interact and tests the model's observable implications at the aggregate level rather than the level of individual molecules; analogously, George and I noted that it is often more efficient to do process tracing at the structural level rather than that of individual agents, but we added that a commitment to mechanistic explanation entails that if it could be shown that individual or underlying units do not behave as a structural theory posits, the structural theory is weakened even if it remains a simplification that is useful for some purposes (Woodward 2003, 142–145).

¹² Woodward 2003, 49.

¹³ To be clear, to the extent that they are policy-oriented, social scientists seek to understand mechanisms so that they can better predict the consequences of alternative manipulations or policies. Some very robust correlational findings, such as the relationship between smoking and cancer, may be sufficiently strong evidence of an average causal effect to properly become the basis for policy interventions even if the mechanisms underlying the correlations are not understood. At the same time, a better understanding of underlying mechanisms can improve our understanding of the conditions under which, and the cases for which, a generalization will hold. Improvements in our understanding of the mechanisms that relate smoking to cancer, for example, may help identify which individuals are at especially high (or low) risk if they smoke, and which interventions other

rather than a guide to causal inference: Woodward notes that "my [2003] project is semantic or interpretive, and is not intended as a contribution to practical problems of causal inference"¹⁴ or "inference to the best explanation."¹⁵

Woodward's account relies on three key features: 1) an intervention on a causal variable X (whether the intervention is in fact possible or not),¹⁶ 2) a counterfactual assertion that Y or its probability distribution would have been different if the intervention on X were carried out, and 3) a type of invariance relationship between X and Y that justifies the counterfactual assertion. In a lengthy chapter,¹⁷ Woodward carefully defines the kind of invariance relations he has in mind. For present purposes, I use his general definition that "a generalization is invariant or stable across certain changes if it holds up to some appropriate level of approximation across those changes."¹⁸ Variance, in this view, "comes in gradations or degrees";¹⁹ more invariant relationships hold across or are robust to many changes in background conditions, and less invariant relationships break down under such changes. Invariance relations, "at least within a certain range, are immutable or unconditional in the sense that there is nothing we or nature can do to turn them off."²⁰ In particular, it is critical that a generalization, if it is to be explanatory, should "continue to hold under an intervention that changes its independent variables sufficiently (or in such a way) that the value of its dependent variable is predicted by the generalization to change under the intervention."²¹ This is consistent with Waldner's view herein, and as Waldner notes, this conception of invariance does not imply (or reject) determinism, as it allows probabilistic relationships as long as they are stable in some conditions.

Waldner argues that the kind of *invariance relations* that Woodward discusses represent the ontological structures that Waldner defines as *mechanisms*. In this view, mechanisms are structures in the world that, unlike variables, we cannot turn on and off by interventions. I take this view to be superior to my own 2005 definition²² as it usefully builds on Woodward's critique of Salmon as well as Pearl's work. Waldner further argues that the mechanistic account of explanation is prior to other approaches because it implies the manipulationist, regularity, and counterfactual accounts of causal explanation, while the reverse is not true. I find Waldner's argument on this intriguing and appealing, but I do not pursue further here the question of the priority of mechanisms among different approaches to causal explanation. Rather, with regard to my own work, Waldner's argument raises two questions. The first is whether the process tracing practices Alexander George, Jeff-

than stopping smoking (such as changing the characteristics of cigarettes) can have beneficial effects.

¹⁴ Woodward 2003, 38.

¹⁵ Woodward 2003, 374.

¹⁶ Woodward 2003, 250.

¹⁷ Woodward 2003, 239–314.

¹⁸ Woodward 2003, 239.

¹⁹ Woodward 2003, 240.

²⁰ Woodward 2003, 242.

²¹ Woodward 2003, 250.

²² George and Bennett 2005, 137; or see page 2 of this essay.

rey Checkel, and I have advocated are compatible with the concept of causation and causal mechanisms that Woodward, Waldner, and Runhardt advance.²³ I believe they are, and as I have not read anything by these authors that indicates that they disagree, I do not pursue this question further here. The second question, which I address below, is whether there are additional or different practices of process tracing that emerge from the concept of invariant mechanisms that improve upon the process tracing practices that I and other methodologists have advocated. Here, I think there are real improvements, but they are not yet as substantial or unambiguous as Waldner and Runhardt have suggested.

David Waldner: Directed Acyclic Graphs (DAGs) and Process Tracing

On the issue of best practices in process tracing, David Waldner made an important contribution in the book I co-edited with Jeffrey Checkel by calling attention to the “completeness standard” of assessing process tracing. There, he argued that (italics are the author’s):²⁴

Process tracing yields causal and explanatory adequacy insofar as: (1) it is based on a causal graph whose individual nodes are connected in such a way that they are jointly sufficient for the outcome; (2) it is also based on an *event history map* that establishes valid correspondence between the events in each particular case study and the nodes in the causal graph; (3) theoretical statements about causal mechanisms link the nodes in the causal graph to their descendants and the empirics of the case studies allows us to infer that the events were in actuality generated by the relevant mechanisms; and (4) rival explanations have been credibly eliminated, by direct hypothesis testing or by demonstrating that they cannot satisfy the first three criteria listed above.

I will not explicate here the procedures of outlining causal arguments as directed acyclic graphs (DAGs),²⁵ although I find graphical representations of arguments to be an extremely useful operational procedure for clarifying my own thinking and understanding other authors’ explanatory claims. I further agree that Waldner’s four-step procedure is extremely useful as a practice in process tracing, and that completeness is an important criterion for judging process tracing. Waldner used the completeness standard in his book chapter to critique very

²³ Interestingly, Judea Pearl’s account and his explication of Directed Acyclic Graphs (DAGs) begins with “special emphasis on Bayesian inference and its connection to the psychology of human reasoning under uncertainty” and adheres to “the Bayesian interpretation of probability, according to which probabilities encode degrees of belief about events in the world and data are used to strengthen, update, or weaken those degrees of belief” (Pearl 2000, 2). I take this to suggest that Pearl’s account is compatible with my own view on the value of Bayesian inference in process tracing, although Pearl does not give much attention to singular causation or inferences from individual cases and does not mention process tracing.

²⁴ Waldner 2015, 128.

²⁵ See Pearl 2000.

effectively several prominent examples of process tracing in comparative politics for having failed to assess evidence on major and obvious steps in their hypothesized processes (in contrast, Waldner judges Elisabeth Wood’s work in *Forging Democracy from Below*²⁶ to have achieved a high level of completeness). I concur that more complete process tracing accounts are, other things equal, preferable to less complete ones.²⁷

Yet I am skeptical that Waldner’s completeness standard provides much clarity, as he claims, on a “much-needed *stopping rule*” regarding when to stop collecting evidence.²⁸ How do we know, from the passage quoted above, all of the following: 1) that the nodes in the graph are “jointly sufficient,” 2) that the correspondence between the event history map and the events in the case is “valid,” 3) that the events “were in actuality generated by the relevant mechanisms,” and 4) that rival explanations have been “credibly” eliminated? Waldner’s four-step procedure and Bayesian process tracing can provide answers to these questions, but those answers are inherently provisional to some degree because they depend in part on unknown unknowns, particularly rival explanations and omitted variables we haven’t thought of and evidence we didn’t think to look for or were not able to access. So true completeness is not possible. In addition, Waldner’s second step appears to be in some tension with Bayesianism: it is not clear that completeness is necessary. What matters is the relative likelihoods of the evidence under the alternative hypotheses, and priors on the latter, and these may lead to very high or low degrees of belief even when evidence and event maps are incomplete. Waldner’s four steps thus do not solve the fundamental problem of causal inference. Of course, *no* approach to causal inference can overcome the problem of unknown unknowns, which is why the problem of causal inference is fundamental.

It is worth noting here that in his discussion of DAGs and the equations associated with them, Woodward includes an “error term” to represent the “combined influence of all the other unknown direct causes of Y that are not explicitly represented in the equation.”²⁹ Similarly, Pearl includes an error term in structural equations to “represent errors (or ‘disturbances’) due to omitted factors.”³⁰ Woodward also notes the importance of specifying the changes under which a relation is and is not invariant, or what political scientists commonly call scope conditions.³¹ Yet as Woodward does not address causal inference, he does not offer any advice on how to assess scope conditions.

Consider Waldner’s example of combustion. He notes that his illustrative account of combustion is incomplete without knowing more about inter- and intra-molecular processes and

²⁶ Wood 2000.

²⁷ At the same time, there is a tradeoff between completeness and parsimony in explanatory accounts.

²⁸ Waldner 2015, 129.

²⁹ Woodward 2003, 43.

³⁰ Pearl 2000, 27.

³¹ Woodward 2003, 243.

sub-atomic particles. But it is incomplete in another way as well: it says nothing about the kind and concentration of the fuel, the concentration of the oxygen, the different flash point temperatures of different materials.³² This is not a terrible shortcoming for the example of fire, since there is no alternative explanation for combustion, but in more complex cases involving social behavior it is harder to tell the scope conditions of hypothesized processes and to assert with confidence that the levels of the variables in a theory were sufficient to generate the outcome observed in a case and that alternative variables or theories do not account for the outcome.

Waldner's example of combustion is also incomplete in that it does not address implicit assumptions about background conditions: in this simple example, perhaps the only consequential background assumption is the presence of gravity. While the absence of gravity does not prevent combustion when fuel, oxygen, and heat are present, combustion in zero gravity produces a very different kind of "fire" (actually it is cooler than fire on earth, both literally and figuratively: see youtube video on combustion in zero gravity³³). In social science, we often have many implicit background assumptions and many alternative explanations that complicate our task of inferring causation and specifying the scope conditions of hypothesized mechanisms.

Thus, I stand by Jeffrey Checkel's and my critique of Waldner's completeness standard, in the closing chapter of our book, namely, that "it is very ambitious to expect a theory or explanation to be fully complete, as there will always be steps in an explanation that involve variables exogenous to a theory, steps for which strong empirical evidence is not available, and steps that are at a more micro level than a researcher chooses to explore. Thus, not every step in a theoretical explanation of a process will fully determine the next step in it."³⁴

I also think Waldner's reading of Pearl³⁵ overlooks one of the most important claims that can be made for methods that focus on developing mechanistic explanations of individual cases. One of the most common and misplaced critiques of case studies is that their results do not "generalize." My standard response is that we do not have a good understanding of whether the findings of a case study will or will not generalize, or the scope conditions to which it will generalize, until we are satisfied that we understand the mechanisms that account for the outcome in the case study. This understanding often emerges from a combination of inductive and deductive process tracing on the case. Pearl makes a related point in arguing for the superiority of causal over associational knowledge:³⁶

causal models (assuming they are valid) are much more informative than probability models. A joint distribution

³² This information might be encoded into structural equations representing a causal graph; I am merely pointing out that a substantial amount of knowledge on scope conditions has to go into these equations even in the relatively simple case of combustion.

³³ DNews 2013.

³⁴ Bennett and Checkel 2015, 265.

³⁵ Pearl 2000.

³⁶ Pearl 2000, 22.

tells us how probable events are and how probabilities would change with subsequent observations, but a causal model also tells us how these probabilities would change as a result of external interventions—such as those encountered in policy analysis, treatment management or planning everyday activity. Such changes cannot be deduced from a joint distribution, even if fully specified.

He later adds that "true understanding enables predictions in ...novel situations."³⁷ Thus, by improving our understanding of how mechanisms operate, process tracing can help us anticipate how changes in the institutional or political context upon which past correlations were based might lead to different distributions in the future, as well as helping us anticipate or narrow the range of possible outcomes in future cases.

Waldner has done an important service in providing an account that shows how in principle we can work to explain individual cases. I share his view—and Woodward's³⁸ and Pearl's³⁹—that a philosophical account of causal explanation should encompass both type causal claims (or claims that X is causally related to Y where X and Y are variables) and token causal claims (claims that a particular X caused a particular Y in a particular case, sometimes called singular causal claims). I also agree that Woodward's and Pearl's works make important strides in this direction (even though Pearl focuses mostly on experimental and correlational methods rather than the token or singular causation of individual cases). In practice, however, explanations of cases and populations will always be potentially fallible.

Rosa Runhardt: Using Counterfactuals in Process Tracing

Rosa Runhardt, building explicitly on Woodward's work, similarly addresses the question of whether Woodward's approach offers potential improvements in process tracing.⁴⁰ Rather than focusing on the type of invariance relations envisioned by Woodward, as Waldner does, Runhardt focuses on the counterfactuals embedded in Woodward's conception. This is an interesting and promising approach, and others have also begun to explore whether and how process tracing relates to or can benefit from a potential outcomes framework.⁴¹ Yet more work needs to be done to clarify the uses of counterfactuals in process tracing, the kinds of counterfactuals that are useful, and the ways in which we can use counterfactuals to identify and address cognitive biases in our own thinking.

Runhardt critiques as an example Kristin Bakke's excellent use of process tracing to assess whether international jihadists caused the shift among Chechen rebels toward a globalized vision of Islamic revival, and toward the use of suicide tactics, through the provision of resources, training, and propaganda material framing Chechen aspirations for independence as part of a global struggle between Islam and other religions. While Bakke provides evidence on the key steps in her hypothesized

³⁷ Pearl 2000, 26.

³⁸ Woodward 2003, 74–79 and 209–220.

³⁹ Pearl 2000, 310–311.

⁴⁰ Runhardt 2015.

⁴¹ Mildemberger 2015; see related discussion in Psillos 2004.

mechanisms and on the timing of changes in the Chechen rebels' behavior and doctrine, Runhardt critiques Bakke for not adding sufficient detail to her assertion that Chechen terrorists did not use suicide tactics prior to their interaction with global jihadists. This assertion bolsters the counterfactual implication of Bakke's argument: had there been no contact with global jihadists, the Chechens would not have framed their struggle as global or used suicide tactics.

It is not at all clear, however, what kind of details we should expect Bakke to add, or whether any additional detail is in fact necessary or useful. The fact that Chechen rebels had attacked civilians but not resorted to suicide attacks prior to their meetings with global jihadists is common background knowledge to those who have studied Chechnya. Runhardt also does not cite the practical standards that political scientists have developed for building and assessing counterfactuals, such as projectability—accurate predictions on hitherto unobserved cases⁴²—and cotenability—“connecting principles that link the antecedent [the premise of the counterfactual] with the consequent [the posited counterfactual outcome]...that are cotenable [consistent] with each other and with the antecedent.”⁴³ Runhardt also does not take note of the alternative explanations that Bakke considers or suggest counterfactuals that might be assessed for these alternatives, though she provides some additional detail on this in her contribution to the present symposium.⁴⁴ For example, a key part of Bakke's argument is that Chechen rebels adopted the rhetoric and tactics of international jihadists not so much out of the inherent ideational appeal of these jihadists' global framing of the struggle, but because the global jihadists were able to offer material incentives—funding, expertise, etc.—for adopting their approach. This suggests a projection/prediction that jihadists will be more successful in winning converts when the jihadists have resources to offer and when the potential converts are resource poor, which has in fact proved generally true (after Bakke's writing) in the recruiting efforts of ISIS and in the radicalization of Islam in places like Kosovo,⁴⁵ although there are no doubt some cases in which materially endowed individuals joined ISIS and imams in well-to-do mosques became radicalized.

There are several additional fruitful uses for counterfactual reasoning in case studies that Runhardt does not address. Counterfactuals can help expose biases in our own thinking about the mechanisms that we hypothesize: if a scholar has confidence in a causal claim, but not an equal level of confidence in the logically equivalent counterfactual claim that the causal claim entails, then that scholar needs to identify and address the source of their cognitive bias. Counterfactuals can also be assessed to the extent that they entail factual ob-

servable implications. For example, if individual A discussed the possibility that individual or entity B might behave in a certain way, and made decisions and contingency plans for what to do if B took the anticipated action, but B did not take the anticipated action, it is plausible that A would have acted on his or her plan if B had behaved as anticipated. An alternative course of action is also a plausible counterfactual if individuals in positions of power argued for that course of action and appear to have nearly won the argument over what to do, and not plausible if it lacked any powerful advocate(s).

We can also use counterfactuals to clarify researchers' *thinking* about the relative “causal strength” of variables (which is analogous to the regression coefficients and their substantive significance in a statistical study, but is not the same as estimates of those coefficients because here we are talking about subjective degrees of belief about such coefficients). Any purchase on this notion of causal weight is otherwise very difficult to assess in qualitative research. For example, I have a long-running debate with William Wohlforth of Dartmouth College's Department of Government about why Soviet leaders did not resort to force to keep the Warsaw Pact countries from breaking away from Soviet control in 1989. We disagree on whether this was primarily due to: A) Soviet leaders' desire to gain access to world trade and technology in order to boost their sagging economy, or B) the lessons Soviet leaders learned from Afghanistan. I support the latter explanation and I have asked Bill directly whether he agrees to the following counterfactual: if the Soviet economy had started growing sharply under Gorbachev's perestroika policies (restructuring of the economy and society), the Soviet Union would have used force to prevent the independence of their Eastern European allies. He has declined to endorse this counterfactual, without specifying some alternative.⁴⁶ I am willing to endorse what I take to be the logically equivalent counterfactual of my argument: if Soviet leaders in 1989 had considered their use of force in Afghanistan a success, they would have used force in Eastern Europe in 1989. We can bring some evidence to bear on these counterfactuals as noted above, but the counterfactuals are of course not fully testable in that we are not able to re-run history. Thus my greater willingness to specify and endorse a counterfactual for my argument does not necessarily mean I am correct, and I do not discount entirely that economic incentives were a factor. Yet Wohlforth's and my different commitments regarding these respective counterfactuals convey useful information about our views on the strength of our arguments.

Runhardt mentions most-similar cases as a relevant kind of counterfactual comparison. Ideally, most-similar cases are similar in all but one independent variable of interest, and different in their outcomes. Runhardt notes that similarity claims are difficult to defend, but she does not note that case study methodologists have addressed most similar case comparisons in great detail. I have argued, for example, for using process tracing in most similar case comparisons to: 1) instantiate

⁴² Tetlock and Belkin 1996, 30–31.

⁴³ Tetlock and Belkin 1996; see also Levy 2015.

⁴⁴ Woodward (2003, 216) notes the role of counterfactual analysis of alternative explanations, calling it a “characteristic pattern of eliminative argument that plays an important role in establishing singular-causal claims and the counterfactuals associated with them.”

⁴⁵ Gall 2016.

⁴⁶ I recount this in part from conversations with Wohlforth; for the relevant published articles see Wohlforth 2005 and Bennett 2005.

that there is a plausible process connecting the values of the independent variable of interest in the two cases to their respective outcomes, and, 2) assess whether other potential independent variables whose values differed in the two cases might be responsible for their outcomes.⁴⁷ To be fair, Runhardt's 2015 article is focused on the inverse question: whether most similar case comparisons can be used to corroborate a counterfactual that, in turn, supports a process claim. Either way, we must remember that both case comparisons and process tracing, whether framed in factual or counterfactual terms, are potentially incomplete and fallible.

In sum, I agree with Runhardt on the importance of thinking about counterfactuals in process tracing, but I think she underappreciates the ways in which counterfactual analysis is already in use among process tracers. There are also broader ways in which counterfactuals might be used, and we need more clarity on how we can use counterfactual analysis to assess and reduce our own cognitive biases.

Conclusions

David Waldner and Rosa Runhardt have done qualitative researchers a great service by exploring whether contemporary philosophical discussions of causal mechanisms have implications for process tracing practices. They have offered useful practical advice for process tracing. These are important contributions, even if they do not warrant Waldner's ambitious claim to have solved the fundamental problem of causal inference. Runhardt's analysis, by reminding us how difficult it is to make defensible counterfactual claims, reminds us also that it is difficult to make defensible causal claims. These contributions offer fruitful directions for additional research on the intersection of philosophy of science and process tracing methods. In particular, we need research, including experimental studies, on how actual researchers, with the cognitive biases to which they are vulnerable, make and use claims about evidence, causation, counterfactuals in process tracing, and whether training in Bayesian analysis, DAGs, counterfactual reasoning, and other process tracing techniques can bring actual practices more in line with philosophical ideals.

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⁴⁷ George and Bennett 2005, 51 and 81.

Symposium: Frederic Charles Schaffer's *Elucidating Social Science Concepts: An Interpretivist Guide* (Routledge, 2016)

Elucidating Concepts: Introduction to the Symposium

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Concepts are the central building blocks of all theoretical work. Reflecting this centrality, conceptual analyses and debates have a long and prominent tradition in the social sciences, even if empirical research often moves very quickly from concepts to observations that are assumed to be empirical manifestations of those concepts. Consider, for example, the large literatures about concepts such as power,¹ democracy,² and the state³; as well as analyses of important concepts in specific subfields of political science, such as the conceptualization of anarchy in International Relations theory⁴ or the (ab)use of the concepts of supply and demand in regulatory governance.⁵

Such work scrutinizing and advancing our understanding of specific concepts has been accompanied by general methodological work on “concept formation”⁶—for which Schaffer suggests the term “concept reconstruction,” since it involves “refashioning already existing terms in an effort to remove deficiencies such as ambiguity and vagueness” rather than creating concepts truly *de novo* or completely re-organizing meaning.⁷ This work has been further extended and enriched by analyses of the interaction between conceptualization and

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¹ See, e.g., Bachrach and Baratz 1962; Baldwin 1989; Barnett and Duvall 2006; Blau 1986; Bowles and Gintis 2008; Dahl 1957; Frey 1996; Grant and Keohane 2005; Guzzini 2011; Isaac 1992; Lukes 1974.

² E.g., Bowman, Lehoucq and Mahoney 2005; Collier and Levitsky 1997; Elkins 2000; Gleditsch and Ward 1997; Lauth, Pickel and Welzel 2000; Lijphart 1968; Munck and Verkuilen 2002; Paxton 2000; Plümper and Neumayer 2010; Treier and Jackman 2008.

³ E.g., Burckhardt 1860; Evans, Rueschemeyer and Skocpol 1985; Mahdavy 1970; Miliband 1983; Nettl 1968; Poulantzas 1969; Sassen 1995; Skinner 1989; Spruyt 1994; Watkins 1934.

⁴ E.g., Bull 1977; Buzan 1991; Milner 1993; Powell 1994; Wendt 1992.

⁵ E.g., Bütke 2012.

⁶ See Bevir and Kedar 2008; Bürger 1987; Goertz 2006; Sartori 1970.

⁷ Schaffer 2016, 5.

measurement,⁸ an entire literature on “essential contestation” over concepts,⁹ and warnings about conceptual stretching.¹⁰

Frederic Schaffer's new book *Elucidating Social Science Concepts* cuts across much of this literature. His interpretivist critique has a consciously dual purpose. First, it seeks to advance our understanding of the role of concepts in social analysis by identifying the weaknesses of a positivist approach to concept formation or “reconstruction” and offering an interpretivist alternative, which Schaffer calls “concept elucidation.” Concept elucidation is partly a distillation of—and a “running commentary”¹¹ on—existing interpretivist work on concepts and partly Schaffer's original prescription for how social scientists might rethink their approach to concepts in social analysis. This should be of interest not only to committed interpretivists but also positivist social scientists that recognize the limitations of a positivist approach.¹² Second, the book seeks to provide “pragmatic guidance” to graduate students and other newcomers to an interpretivist approach, describing how to put elucidation into practice through the use of three “strategies,” which Schaffer calls grounding, locating, and exposing.

The first chapter, “Why Do Concepts Need Elucidating?” elaborates on the “positivist reconstruction versus interpretivist elucidation” of concepts. This rather stark dichotomization of scholarly positions on ontology and epistemology is intended to clarify the distinctive contribution of interpretivist conceptual analysis. Key to this discussion is the distinction between “experience-distant” and “experience-near” concepts, a distinction that Schaffer borrows from Geertz.¹³ Here, “experience-distant” is used to categorize the highly abstract concepts of scholars and other “experts” (concepts such as utility, social class, or the polarity of the international system). These concepts are the creation of the analyst and dominate positivist social analysis. “Experience-near” concepts, by contrast, seek to capture the understanding of terms as used, felt, and imagined by the social group(s) about whom the scholar and her reader hope to learn more through the analysis (concepts such as love, fear, or “we”). While interpretivists certainly use experience-distant concepts—at a minimum to transcend “the mental horizons of the people they wish to understand”¹⁴—they take a much greater interest, Schaffer suggests, in experience-near concepts, due to a fundamental interpretivist belief that those “intersubjective constructions, [which] provide people [...] a ‘set of common terms of reference’ to organize, navigate, and challenge their social arrangements” are fundamental to

⁸ E.g., Adcock and Collier 2001.

⁹ See, e.g., Connolly 1983; Gallie 1956; Lukes 2004.

¹⁰ See Collier and Mahoney 1993; Munck 2004; Sartori 1970.

¹¹ Schaffer 2016, xiv.

¹² Schaffer 2016, 22, 89ff.

¹³ Geertz 1983.

¹⁴ Schaffer 2016, 3; see also 71.

achieving an understanding of the social world as “social reality cannot be understood apart from the language people use to operate in it.”¹⁵

Schaffer illustrates the distinctive contributions of interpretivist conceptual analysis through an extended discussion of the concept of “family”¹⁶—one of the concepts used by Sartori in his pathbreaking work on concept (mis)formation. While recognizing the nuanced self-reflection sought, and even achieved, by the best positivist conceptual work, Schaffer nonetheless faults such work for an inability to overcome three related problems: one-sidedness, excessive universalism, and false objectivism. He illustrates these problems by examining the treatment of the concept of the family in positivist social science. An interpretivist approach, Schaffer posits, can overcome these problems in a way positivist approaches to concepts cannot, by asking a series of questions about “family” as an experience-near concept, with the goal of “clarify[ing] the meaning and use of concepts in lived practices”¹⁷—that is, through the elucidation of concepts.¹⁸

Chapters 2 through 4 then offer concrete guidance for how one might implement an interpretivist approach. Specifically, the three “strategies” presented in these chapters are chosen to help scholars address the three major problems of the positivist approach identified in chapter 1. “Grounding,” the focus of chapter 2, is meant to safeguard against one-sidedness, understood as “privileging those meanings of a concept that are important to the researcher while ignoring other meanings that are salient to situated actors themselves.”¹⁹ The chapter begins with a brief exposition of Wittgenstein’s reflections on language and on discerning the meaning of a word “from the different uses to which it is conventionally put.”²⁰ Those uses are, Schaffer suggests, not characterized by “essential [common] properties” (as assumed in positivist treatments of concepts) and not even by a “definable, closed, given set of properties,” but are, rather, living and changing, with different uses having at most some “resemblance” to each other.²¹ The remainder of the chapter provides specific and hands-on guidance for how to “investigate” the Wittgensteinian, likely-to-be-complicated “ordinary use.” Recommended *grounding* practices include observing different uses, observing what it is that cannot be said, making explicit differences between synonyms—and more generally “investigating grammar” through interviews or the analysis of text. Investigating grammar, moreover, entails doing so “ethnographi-

cally,” since ordinary language uses are often peculiar to different communities, even when those communities are situated within the same polity and ostensibly speak the same “language,” such as tramps and medical students (two of the group-labels used as illustrative examples in chapter 1) from the same linguistic group in a given country. Such careful examination (which positivists might consider to be an “inductive” approach to concept formation) yields an understanding of the varied ways in which “actors themselves understand a concept,” which is, Schaffer argues, not available from a positivist approach to concepts and conceptual analysis.²²

Chapter 3 focuses on elucidating concepts across different languages and cultural, political, and temporal contexts (which Schaffer had in chapter 2 assumed away in order to explore the notion of *grounding* independently of such macro-contextual factors). Schaffer proposes “locating” as an interpretivist strategy for understanding concepts—that is, “elucidating historical and linguistic specificity”—and seeks to overcome the excessive universalism that Schaffer sees as inherent in the starkly positivist approach laid out in chapter 1. Avoiding “translanguage and transhistorical generalizations that do not hold up,”²³ however, is by no means easy, as Schaffer shows in chapter 3 when he scrutinizes “missteps” in classic works by anthropologist Clifford Geertz and historian Quentin Skinner, whom he considers “two interpretivist virtuosos.”²⁴ Each of these critiques is followed by specific suggestions, including some illustrative examples, for applying Schaffer’s interpretivist approach to concepts across languages and over time.²⁵ When conducting analyses over time, for instance, Schaffer’s *locating* of concepts demands that scholars be attentive to the possibility of a concept’s “birth” (in advance of which certain ideas might have been effectively unthinkable) or “death” (usually reflecting major, disruptive political or social changes, such as the end of feudalism), as well as “shifts in word use.”²⁶

Chapter 3 ends with a caveat for those who might be tempted to take Schaffer’s advice to the extreme. While Schaffer’s key concern is a “misplaced...contemptuous attitude toward the particular case” in the contemporary social sciences, which he attributes to what Wittgenstein called our “craving for generality,” he cautions that we must not insist on cultural, temporal or other specificity to the point of losing our ability to abstract and generalize.²⁷ This cautionary note raises, of course, the question of how to find the right balance between grounding and locating on the one hand and abstraction and generalization on the other. On this score, the chapter

¹⁵ Schaffer 2016, 7, 6.

¹⁶ Schaffer 2016, 10–21.

¹⁷ Schaffer 2016, 7.

¹⁸ Somewhat surprisingly, especially given Pachirat (2015), Schaffer does not discuss the origins of the term “elucidation,” but it, too, appears to draw on Geertz, and specifically Geertz’s call for “illuminating” not just the understandings and experiences of the people studied but also the “connection” between those experiences and the experience-distant understandings of the scholarly community (Geertz 1983, 58; Schaffer 2016, 8).

¹⁹ Schaffer 2016, 12.

²⁰ Schaffer 2016, 27.

²¹ Schaffer 2016, 31.

²² Schaffer 2016, 32–53.

²³ Schaffer 2016, 12.

²⁴ Schaffer 2016, 56; for the critique of Geertz, see 56–59; for the critique of Skinner, see 64–67.

²⁵ To focus on change in other concepts, such as “class” or “altruism,” time is in chapter 3 treated by Schaffer as if autonomously given, but might itself be problematized as socio-culturally or politically, theoretically or analytically constructed; see Bütthe 2002; Grzymala-Busse 2011; Hutchings 2008; Jacobs 2008; Pierson 2000.

²⁶ Schaffer 2016, 64–70.

²⁷ Schaffer 2016, 70–72.

offers only the (good but rather generic) advice that the right balance “depend[s] on the theoretical aims that animate our project.”²⁸ This may be where good scholarship is more art than science.

Chapter 4, “Exposing: Elucidating Power” (supplemented in important ways by parts of chapter 5 on the “Ethics of Elucidating”) offers advice on how to avoid what Schaffer sees as the false objectivism of not just positivist but generally positive (as opposed to normative) social science: when under the banner of “value-free” analysis, scholars disregard “how everyday [and] social science concepts are embedded in relationships of power and thus carry a moral or political force.”²⁹ Implicitly, the book’s most overt turn to the political dimension of concepts thus highlights what is probably the most prominent, long-standing concern of the Frankfurt School.³⁰ This intellectual genealogy is not made explicit in the chapter but Frankfurt School progenies would surely be sympathetic to Schaffer’s advice, for instance, to scrutinize “the multiplicity of acts performed when a word is deployed”³¹ and to be attentive to how institutional context enables certain practices and forecloses others, so that alternatives that would be more advantageous to the “weak”³² are rendered invisible and thus can be hard even to conceive of as a counterfactual. Schaffer brings these ideas explicitly to bear at the level of words and concepts, drawing on Ido Oren’s work to suggest a specific four-step method for “rediscovering [earlier] struggle[s]” when the outcomes (reflecting the power relations at the time) have become embedded in our concepts.³³

This symposium brings together three sympathetic critiques by scholars who—from a quite diverse set of perspectives and grounded in very different kinds of empirical research—have long thought carefully about conceptual issues: Lahra Smith, Gary Goertz and Patrick T. Jackson. Their essays offer a critique in the best sense: recognizing strength but also incisively raising questions about weaknesses (including, especially in Jackson’s essay, offering alternative suggestions) for pushing our understanding of concepts forward. In the concluding essay, Fred Schaffer himself takes up the constructive challenge, sketching an agenda for moving forward.

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²⁸ Schaffer 2016, 71. See also Büthe 2002

²⁹ Schaffer 2016, 12.

³⁰ See, e.g., Marcuse 1964.

³¹ Schaffer 2016, 75.

³² Scott 1985. See also Gaventa 1980.

³³ Oren 2014; Schaffer 2016, 82–87.

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A Spark for the Political Imagination

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Fred Schaffer's deceptively slim volume on interpretive methodologies and conceptual analysis is a gem. It is exceptionally well written and concise, packed with interesting and wide-ranging examples from a variety of disciplines, historical and geographic spaces. Schaffer develops *elucidation* as an approach to apprehending social phenomena and contrasts it with positivist approaches (in particular, the idea of *concept formation*). Schaffer argues that rather than attempting to objectively contain a concept with positivist approaches to concept classification, we are better served by elucidating concepts. His elucidation techniques allow us to see the "meaning and use of concepts in lived practices" and to avoid precise but unrealistic, un-lived concepts.¹

The discussion of these distinctions in the text is refreshingly easy to grasp and jam-packed with excellent examples. Through these examples, Schaffer offers a set of methodological tools for "grounding, locating and exposing" data that help us interpret everyday, taken-for-granted concepts. For these

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¹ Schaffer 2016, 7.

reasons, the book would be a great addition to any graduate studies curriculum in the social sciences, and faculty should immediately update their syllabi. But with this volume, Schaffer's real contribution is the stimulating deployment of language, example and imagery to initiate a process of political imagination that is quite needed in political days such as these we face. In this way, Schaffer expands our classrooms and compels us to apply our new methodological tools to much larger arenas. Both students and scholars could benefit from such inspiration.

Through elucidation, and the techniques he develops, Schaffer helps interpretivists use their conceptual tools to examine social science concepts themselves, particularly those associated with power. For him, the task of elucidation is to "investigate the ways in which the social world is built up linguistically and the ways in which social actors deploy concepts to pursue their goals."² As such, Schaffer's guide to elucidating concepts could be a helpful tool (or toolbox) for scholars and policy-makers working in complex and real-time political contexts and on pressing political problems.

While much of our research addresses long-term processes, political scientists and other social scientists are well positioned to contribute to the public discourse on important contemporary issues such as national and local elections, race and gender issues and political protest, to mention just a few. Some, maybe even many of us, see this as an important extension of our civic and ethical engagement with our fields of study, as well as our teaching. We may appear on *The News Hour* or *NPR* or *Fox News*, we increasingly Tweet or make statements for *The New York Times*; we write for *Foreign Affairs*, contribute to roundtables and policy reports for major think tanks and collaborate with government and policy-making bodies. This is crucial to giving our research findings the relevance and exposure they deserve and, particularly for qualitativists and interpretivists, an opportunity to contribute our methodological and epistemological insights to wide political debate.

So what can interpretivist tools such as elucidation bring to the task of public meaning-making in contemporary political life? I will use an example from my own experience to demonstrate the imaginative spark that was generated by reading Schaffer's elucidation techniques. At the 2016 Rio Olympics, many outsiders were introduced for the first time to the movement broadly called the #Ethiopianprotests, or more specifically the #Oromoprotests, when the silver medalist marathon runner from Ethiopia, Feyisa Lilesa, crossed the finish line with his arms raised in an X above his head in symbolic protest.³ Scholars of Ethiopia, myself included, Ethiopians at home and in the diaspora, had been following the protests closely for some time, but were surprised to see Lilesa's powerful use of the protest symbol on the global stage. The International Olympic Committee and news media outlets attempted to explain the gesture, the context behind it, and the potential conflict with IOC rules on the politicization of the Olympics, and broader

debates were raised regarding the role of foreign aid in Ethiopia.

So here was my dilemma. Like Schaffer, I have an interpretive interest in how people use and understand words and concepts like *democracy*, *freedom*, and *protest*. How would these protests be interpreted? How do the participants themselves understand these protests? I do not assume that Ethiopians use or understand these words in ways that are always similar to how these words are used and understood by the media or the foreign policy and donor community in my home city of Washington, DC. For instance, the ruling regime, the EPRDF, has a very specific notion of what they describe as "revolutionary democracy" and their role as the "developmental state" that is distinct and fairly well articulated. The EPRDF today touts "freedom" as "development," generally understood as economic development. At some level, the message of the protests, though less clear, could be heard as a repudiation, by at least some Ethiopians, of these notions of democracy and freedom, or as a repudiation of the pace and fairness of the economic development that has occurred.

The protest movement has a specific historic and linguistic meaning in Ethiopia, shaped by past and present meanings, influenced by the specificities of the place and the time. What do *protest* and *democracy* mean today in comparison to earlier iterations of protest in Ethiopia?⁴ What does an Ethiopian of a particular ethnic, language group or region mean when she protests for more freedom in 2016 as compared to in 1974 or 1989? Is it a freedom that is distinct from earlier struggles for freedom or does it reflect a continuity?

There is no doubt in my mind that Ethiopians in the diaspora and their compatriots at home are searching for a shared language of *freedom* today. Protests in ethnic Oromo regions of Ethiopia began around November of 2015, and since that time more than 800 people have been killed and at least 25,000 jailed by the ruling government, the Ethiopian People's Revolutionary Democratic Front (EPRDF).⁵ Protests erupted in city after city and were met with harsh and violent crackdowns by the police and military forces of the ruling government, nationwide media blackouts, punitive firing of civil servants and expulsion of students.⁶ The spark in November of 2015 was the announced plan by the ruling government, called the Addis Ababa Master Plan, to expand the capital city onto the lands of the ethnic Oromo community. The protests spread across the country and into areas not only of the Oromo com-

⁴ For example, the 1974 Revolution deposed Emperor Haile Selassie and was the consequence of a broad movement of "students, teachers, unemployed youth, civil servants, taxi drivers, soldiers" (Bahru Zewde 1991, 229). The demise of the subsequent regime of the military dictatorship of the Derg came through civil wars rather than "protests" per se, but protests were there nonetheless (Keller 1988; Zewde 1991).

⁵ It is important to note that most of the people jailed were never formally charged but were released after "reeducation" on the ruling regimes' notions of "democracy" and "development." The highest-level political prisoners, including political opposition and journalists, have not been released and are charged with treason.

⁶ Human Rights Watch 2016.

² Schaffer 2016, 7.

³ Igunza and Edwards, BBC, 8/26/16.

munity but also areas of the Amhara ethnic group, where protests were related to issues of land rights and political autonomy as well. The sporadic but relatively sustained protests reflected complex constellations of both longstanding and more recent grievances of land loss and political alienation, charges of corruption and administrative mismanagement, rapid and uneven economic growth and unmet expectations.

The ruling regime blocks Ethiopians' ability to freely exchange perspectives, making it difficult for shared meanings to emerge or for outsiders to interpret or analyze any intended meanings. Exchange is blocked in multiple ways, particularly through internet and phone blackouts as well as by complex and highly effective surveillance networks and monitoring systems. It is also blocked in the sense that communities inside and outside the country are divided in their language of *freedom*, their sense of what that means today.

In particular, a clear connection between *development* and *freedom* has not yet emerged. Is the freedom sought by some a freedom that is shaped by a logic of economic development and the kind of hyper-capitalist economic development that Ethiopians have experienced in the last decade or so? Or is it freedom that is informed by individual political rights and liberties that somehow unleash economic opportunities? The degree to which these two sets of contestations are interlinked is informed by relationships among and between the 75 or so distinct ethnic and linguistic communities in the country. How do the various communities inside Ethiopia relate to one another and to the vast and valuable resources that provide the engine of the *development* that provides the *freedom* that Ethiopians raise their arms in an X in pursuit of? How do Ethiopians share that freedom with one another? These unanswered questions are urgently political but also conceptual.

In the elucidation technique of *location*, Schaffer points to the reality that words "do not necessarily have stable or fixed meanings across times or tongues."⁷ In a country with some 75 distinct ethnolinguistic communities, there are different understandings of *freedom* and *protest* worth thoughtful interpretive elucidation. When I was writing my book on language policy and national citizenship in Ethiopia a few years ago, I would ask respondents to explain to me the meaning of the opening line to the 1995 Constitution of Ethiopia which identifies the citizenry as the "*beheroch, behersebwoch ina hizbwoch*" or the "nations, nationalities and peoples" of Ethiopia.⁸ What that means as a founding set of citizenship identities was contentious and complex across the country. (What is the difference between a *nation*, a *nationality*, and a *people*? Why must they be identified together, and at the outset, and what role does that play in the shaping of Ethiopian national identity?) It would be even more difficult to investigate this question of language and citizenship in Ethiopia today.

The specifics of *how* Ethiopians understand national citizenship, inclusion, and especially *freedom* and *democracy* today must be elucidated in a modern context, as well as a historical one. However, just using an English word in translation

will obscure far more than it will illuminate. In his seminal history of modern Ethiopia, Bahru Zewde writes about the 1974 revolution: "indeed, the equivalent term for 'revolution' (*abyot*) was a relatively recent one in the Amharic lexicon; many came to learn it only after its eruption."⁹

These insights from Zewde and Schaffer help explain the difficulty that Western and international donors had when they attempted to respond to Ethiopians' recent calls for "freedom." In the present period, in the international and domestic news media, in the protests and the protest calls are critiques of the large amounts of international aid that go to Ethiopia annually from donors like the United States, the European Union and the multilateral institutions. What responsibility do these powerful partners have in influencing outcomes towards *freedom* in Ethiopia, particularly when the instruments of state violence are being used against unarmed and generally peaceful protesters?

In the last year or more since the Oromo/Ethiopian protests began I have been sometimes paralyzed by my own inability to navigate a space between two responses to the Ethiopian protests—one that is too embedded in advocacy for one or another particular local community for my comfort as a scholar, and one that is too removed and too enmeshed in the vague language of development. The former approach would suggest that I pick sides in a sense, somehow decide which ethnic community has suffered the most, or which history is true, a most suspect task for a political scientist. The latter approach, and one often chosen by social scientists, prefers a seemingly neutral concept formation, one that is "value-free, objective and...detached from a broader context of political contestation."¹⁰ In the Ethiopian case, the best way to do that is to retreat to the numbers on economic development. But that is precisely why so many experts did not anticipate the protests of the last 18 months and were unable to explain their persistence: international finance institutions and development experts have praised the country for its GDP growth of six to 10 percent (World Bank 2016). It is also a perspective that misses the deeply normative and ethical implications of equating GDP growth rate with freedom. Somewhere in between the former and latter approach is the opportunity to unpack the ideas of protest and freedom and maybe even democracy and development that are deployed by the protesters, the Ethiopian state, the donors and analysts in the international community.

To grapple with these questions requires at least some role for social scientists and historians who can contribute an elucidation of the historicized, meaning-making understandings of *freedom* by protesters in Ethiopia. Schaffer reminds me of the many important applications of interpretivist tools to this challenge of meaning-making and their potential to yield "liberating insight."¹¹

I think that what we are witnessing in Ethiopia is a complex dynamic of *protest* and *acquiescence*: some citizens have taken to the streets, but most Ethiopians in the country con-

⁷ Schaffer 2016, 55.

⁸ Smith 2013.

⁹ Zewde 1991, 228.

¹⁰ Schaffer 2016, 19.

¹¹ Yanow 2014, 145, cited in Schaffer 2016, 90.

tinue to go about their business of work, school, religion, attending to family and community. Many citizens are still contemplating the choice between the kinds of exceptionally cautious reform paths they have been on in recent decades versus the more fully wrought political revolution that is touted in some corners, mostly in the diaspora. A fair number of Ethiopians have benefited from recent economic gains, particularly in Addis Ababa and other regional towns, and are reluctant to put those economic gains at risk. Surely many are also impatient for more than just the kind of “developmental state” vision that the regime has been touting to the donors, particularly with its paternalistic and restrictive vision of social space and the practices that accompany it. But many Ethiopians have been down this road of protest and revolution before and they are wise enough to contemplate its implications and the alternatives.

For these reasons, I think we see the protests proceed in fits and starts, in spurts that fizzle and reignite. Something may set that spark, however, which is how the earlier *abyot*, or revolutions, have taken off, sometimes before there was even language for them. We know that protest is part of the process. Protest is not necessarily a rejection of citizenship or the regime or the 1995 Constitution, as the state tends to project it, but may rather be an embrace, through contestation, a “citizenship act”¹² that is still distinguishing itself from earlier epochs in Ethiopian history. If the ruling government could see peaceful protest as civic engagement it could perhaps find an imaginative spark for new civic identities, and new civic spaces, rather than older models of state repression.

I am certain that the outcomes of ongoing political protests in Africa’s second largest country and the world’s fifth largest refugee-hosting country, Ethiopia, are quite consequential for us all. Schaffer offers the exciting possibility that the techniques of elucidation and, in fact, the methodological resources of interpretivism in general can create new windows of inquiry into the objects of our study. This “emancipated” knowledge is possible because the tools do not take concepts as givens but treat the concepts themselves as fields of study. That is very much what concepts should be—windows into the social world around us, ways to make sense of processes with potentially profound consequences.

Classroom Applications

In introducing Schaffer’s book, I said that it would be a great addition to any graduate studies curriculum in the social sciences. The book is a thoughtful introductory volume for a graduate course in social science methods as the title and series it is placed in suggests. It is, however, significantly more than that, and I want to say something here about that. It would be a serious mistake for this guide to be read only by interpretivists. It will also make an invaluable contribution to a general survey course, should departments and programs be genuinely committed to intellectual and methodological pluralism. It is accessible and broad ranging enough for those new to non-quantitative methods and filled with examples from

other disciplines (history, anthropology, literature) as well as a variety of sub-fields in political science, including American Politics and International Relations (American elections, just war theory).

It is full of interesting and succinct examples and cases, both old and new. Schaffer addresses Sartori’s use of the *family* as a universal concept and its deconstruction as a specific, local and subjective concept.¹³ And Schaffer introduces discussion of *Columbusing* with which he demonstrates how the technique of “grounding” or using “ethnographic investigation of grammar” provides a powerful tool of conceptual elucidation.¹⁴ He describes the use of the term *Columbusing* in reference to one’s travels abroad and as a critique of gentrification or racial discrimination, shedding light on social processes with distinct social and political meanings.

Schaffer uses his elucidation techniques to critique work in sociology, literature, history, anthropology and political science, demonstrating the tremendous reach of interpretivist contributions and making the slender volume a likely contribution to methodological training and theory work in a variety of disciplinary traditions, not only political science. He challenges the positivist social science notion that concepts are formed anew, but rather points to what they do as a “refashioning [of] already existing terms in an effort to remove deficiencies such as ambiguity and vagueness.”¹⁵ For an interpretivist, the positivist notion of concept formation is an exercise that removes the lived and intersubjective nature of social reality and the language in which concepts are deployed by the very social beings that we study. Elucidation, then, is the study not of the individual opinions about any set of games, but the shared “terms of reference, the rules that constitute . . . games.”¹⁶

I encourage scholars to use this useful textbook to spark their own imagination as well as that of their students and colleagues. Even for the majority of social scientists, including political scientists, who intend to use quantitative and formal models, exposure to the insights of interpretivists and the examples that Schaffer lays out here will be of tremendous value. An excellent addition to upper-division undergraduate and early graduate methods courses, this guide can also provoke a bit of healthy imaginative conceptual mapping for even the most seasoned scholar hoping to get a new angle on a topic that has been nagging at them.

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¹³ Sartori 1975 cited in Schaffer 2016, 10–25.

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¹⁵ Schaffer 2016, 5.

¹⁶ Schaffer 2016, 7.

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Schaffer's Elucidating Social Science Concepts: Notes of a Conceptualist in the Field

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I am quite sympathetic to many aspects of the anthropological and ethnographic approach defended by Fred Schaffer in his *Elucidating Social Science Concepts: An Interpretivist Guide*.¹ Much of my methods work is motivated and informed by what I call "methodological anthropology," which I define as the examination of the practices of social scientists and philosophers regarding concept formation and construction.

My original interest in concepts started from the observation that social scientists and philosophers spend a lot of energy and effort defining, disputing, and thinking about concepts. At the same time, concept methodology was completely absent from methods, statistics, and research design textbooks. These have chapters on measurement but nothing about concepts.

Schaffer's chapter 2 has the subtitle "how people understand a concept." He focuses on "everyday people" as subjects of his methodology; in contrast, I have focused on social scientists and philosophers as subjects. This is a major difference between our interests. For example, in addition to understanding social science practice, I give advice to my subjects (social scientists) on how to do things better. Fred is not telling everyday people how to do concepts better.

At the same time, I completely agree with almost all the "lessons" he has for elucidating concepts and would apply these lessons to my target groups. For example, his postulate to "investigate ordinary use" is critical because people, in-

cluding social scientists, do all kinds of odd things with concepts. This analysis is critical in producing better social science concepts. "Compare the use of the same word in different language games" means, for example, that one should look at how political theorists or philosophers work on a concept, say democracy, versus quantitative social scientists. "Examine opposites and negations" is absolutely essential to distinguishing between what I call the positive and negative poles. Terminology is critical and signals all sorts of issues. For example, social scientists cannot agree on what to call "not-democracy" and this has varied over time, with popular options like monarchy (19th century), dictatorship,² authoritarian, totalitarian, etc. So I completely endorse his recommendation to "follow the clouds of etymology."

In short, much of Schaffer's ethnographic advice works very well in understanding how social scientists develop and use concepts.

Much of chapter 3 resonated with me as well. The analysis of historical developments and genealogy is critical to understanding social science concepts. For example, one cannot understand the polity or Freedom House datasets without an understanding of their history. Many things that seem odd or curious about these datasets arise from the fact that they were not meant to capture concepts of democracy at the beginning! They have evolved and been adapted over time, but still retain traces of their origins. Freedom House was about the concept of liberty—social, economic, and political. It eventually morphed into a democracy dataset. The polity concept of anocracy, which is now used to refer to competitive-authoritarian regimes, originated in the concept of anarchy.³

In short, much of chapters 2 and 3 is directly relevant to thinking about how social scientists and philosophers develop, debate, and use concepts and is good advice to all those interested in concept methodology.

Chapter 4 is about "elucidating power." A good example of this practice is the literature on gender and politics because one of the first moves of a gender scholar is to deconstruct and analyze the gender bias of traditional concepts. For example, it is fascinating to see how the World Bank conceptualizes "indigenous people," a concept that is very politicized and that has large real-life implications for these peoples. The discussion in the literature is reminiscent of Foucault talking about an institutionalization of "insanity-madness" (*folie* in the 18th century). To apply this practice more broadly, theories involving democracy and democratization would probably require some significant changes if women's voting were included in the major concepts and datasets, where women are quite notable by their absence.⁴ The same issue applies to minorities, e.g., African-Americans, in democracy concepts and datasets: for example, the USA in 1920 is coded a maximal democracy by polity.

Schaffer contrasts "positivist reconstruction" with "interpretivist elucidation." What "positivism" means is a hotly

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¹ Schaffer 2016.

² Przeworski et al. 2000.

³ Gurr 1974.

⁴ See Paxton (2000) and Paxton et al. (2003) for nice discussions.

contested concept itself. I see Schaffer as engaged in positivistic empirical research. His long discussion about what “family” means in different cultures is meant to be an accurate representation of these differences.

So Schaffer and I agree on many points dealing with the semantics of concepts. We part ways on the role of concepts in describing the world and their use in explaining how the world works. His book stresses the value of understanding how people use concepts and what they mean by various concepts. But concepts have an instrumental value as well. We can ask how well they describe the world and if they are useful in explaining the world.

The biggest difference between us lies in the role of explanation, causation, and causal hypotheses. I am interested in concepts because they are essential in describing the world, but also very much because they are core to explaining the world. This of course makes me a “positivist,” but some interpretivists also want to explain the world.⁵ Causal explanation is the goal of my main target group, social scientists, and is my goal in my substantive work. I work from the philosophy that high quality concepts are critical to high quality social science. Bad concepts, e.g., terrorism, lead to bad research. A very big chunk of my applied work over the years involves very serious conceptual analysis. Before I can explain international peace I needed to think very hard about the concept of peace.⁶ To analyze how people, say Wolofs in Gambia, differ in their concept of democracy, is interesting to me if that somehow “matters.” Mattering is that it influences behavior or is influenced by something. These are causal questions. I am interested in differences in meaning and concepts, but only those that somehow matter in causal explanations, hypotheses and theories.

The title of Schaffer’s book indicates that it is about “social science” concepts. To advance social science we need to know how interpretivist methodologies of concepts help or relate to causal explanations and hypotheses.

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Commanding a Clear View: Words, Concepts, and Social Science

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To lay my cards on the table at the outset: I am broadly sympathetic to Frederic Schaffer’s overall campaign in favor of conceptual elucidation: “investigating the ways in which the social world is built up linguistically and the ways in which social actors deploy concepts to pursue their goals.”¹ On numerous previous occasions I have been, like Schaffer, decidedly critical of scholarly efforts to “fix” the meaning of a concept (like *the West* or *civilization*) and then to use that scholarly reconstruction as a base from which to legislate appropriate and inappropriate practical claims using that concept—as though our task as scholars were to correct the social world rather than to explain and understand it. So Schaffer’s careful explication of techniques for elucidation, grouped under the headings of “grounding,” “locating,” and “exposing,” provides a refreshing alternative to the sort of advice about concept analysis one typically receives from scholars engaged in the kind of project I think rather problematic.

That said, in my view Schaffer’s book also illustrates—practically and performatively if not deliberately—an important liability of his approach to concepts. The version of “interpretivism” that emerges from his account, while grounded in how people in the field conventionally use the word, obscures rather than clarifies important philosophical distinctions between theory, methodology, and method, and shores up philosophically misleading but practically operative dichotomies opposing “interpretivism” to “positivism” as if those were coherent intellectual packages. Despite conventional use, I do not believe that “positivism” and “interpretivism” name such coherent packages. For that reason I do not believe that the only alternative to reconstructing a concept so that it can be inserted into a statistical study as an independent or dependent variable—which is the so-called “positivist” strategy—necessarily means taking on all three of Schaffer’s “sets of interpretivist questions”² about ranges of meaning, linguistic and historical specificity, and political context. By adhering

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¹ Schaffer 2016, 7.

² Schaffer 2016, 21.

⁵ e.g., Wedeen 2002; Foucault 1972.

⁶ Goertz et al. 2016. There are many war-conflict datasets, we offer the first peace dataset.

too closely to contemporary scholarly convention, Schaffer reproduces a simplified picture of our choices, and tacitly insists that we choose a side. Despite his admirable caveat about the book being “more akin to a collection of recipes written for an adventurous cook,”³ the overall picture he paints is still dichotomous.

The difficulties begin on the first page of the book, where Schaffer introduces positivism and interpretivism as “two different conceptions of social science or, more precisely, two different methodologies that the scholar may bring to the study of the social world.”⁴ He takes positivism to be a stance about the mind-independent existence of entities in the social world (which we might call a mind-world dualism); this gives rise to the task of formulating propositions “about those entities based upon the identification and measurement of regularities within and between them.” Interpretivism, by contrast, maintains that “there are no ‘real’ social entities, only culturally mediated social facts,” and that this stance, which we might call mind-world monism, means that the scholarly task is to “shed light on how shared meanings and their relation to power inform or structure the social world and the study of the social world.”⁵ There is a lot crammed into this distinction: philosophical ontology (also known as “methodology”)⁶ concerning the hook-up between the mind and the world, scientific ontology (also known as “theory”) concerning the character of social entities and the nature of social action, and a series of concrete research tasks that supposedly follow or flow from those positions. Theory and methodology are also linked, although the links are not made especially clear; we aren’t told precisely *why* a commitment to mind-world dualism on the part of the researcher both implies a task of looking for nomothetic empirical generalizations and directs researchers away from investigating the role of shared meanings.⁷ Nor are we told why an appreciation of “culturally mediated social facts” means that researchers have to prioritize the role of shared meanings in their scholarly investigations.

In my view the ambiguity here arises from the conflation of philosophically distinct registers with the unfortunate consequence of disciplining thinking space into a binary either/or choice between “positivism” or “interpretivism.” The two registers of theory and methodology speak to different aspects of scholarly claims; the former concerns the *content* of a claim about the world, while the latter concerns the *status* of that

claim. And they can in principle be combined in a variety of different ways, despite the fact that we conventionally reproduce only certain combinations in our existing research practice. There is no philosophical problem at all involved in taking the theoretical position that shared meaning shapes the world, and simultaneously adopting a research strategy intended to disclose systematic cross-case correlations between variations in shared meaning and variations in social arrangements or outcomes. Similarly, there is no philosophical problem at all involved in taking the methodological position that scholarly knowledge is not a representation of a putatively mind-independent reality but is instead tightly co-constitutive of that reality, and simultaneously adopting a theoretical perspective that focuses on social structures or arrangements of practical activity rather than on shared meaning.

What Schaffer identifies as “positivism” and “interpretivism” are combinations of theory and methodology that are not exhaustive of the combinatorial possibilities—and this matters precisely because Schaffer’s discussion presumes, albeit tacitly, that they are exhaustive. One is either a “positivist” or an “interpretivist,” and the whole of each package has to be accepted at the same time. His recommendation of three aspects of conceptual elucidation does not entertain the possibility that a scholar might not engage in all three aspects, and I submit that Schaffer *cannot* entertain that possibility because the menu of choices has been framed too dichotomously.

To elucidate a concept, in Schaffer’s account, requires first grounding that concept in experience-near language (“commonplace words used in everyday contexts”⁸) by looking at how ordinary people use terms in their everyday lives, and then locating a term in its specific context to prevent a mistranslation of the relevant conceptual category into something in our vocabulary as though it seamlessly fit there. Grounding and locating terms in this way provides knowledge of concepts precisely because “a concept is constituted by the different ways in which a word is used,”⁹ and the scholar’s role is to call attention to that diversity rather than to identify commonalities across uses and contexts.

Why? Schaffer provides two broad answers, one involving criticizing concepts that “have become stabilized, naturalized, or neutralized in ways that obscure from view their histories of contingency and contestation,”¹⁰ and the other involving an effort to make certain that scholarly concepts correspond to the concepts in use among the people being studied: “we would need to know who each voter considers to be a member of his or her family in the context of ballot casting” in order to determine whether family members influence one another’s votes.¹¹ Both of these ends reflect a skepticism about universals and a desire to provincialize scholarly discourse: if scholarly concepts are just one set of conventional uses of terms, there is no compelling reason to prefer the scholarly concepts over those operating in the contexts under investi-

³ Schaffer 2016, xv.

⁴ Schaffer 2016, 1.

⁵ Schaffer 2016, 2. Note that Schaffer does not use the terms “mind-world dualism” or “mind-world monism.” Nor does he distinguish between philosophical and scientific ontology. But I am not quibbling about terms here. Regardless of which terms one uses, I am arguing that these distinctions *need* to be a core part of our methodological ruminations.

⁶ On this expanded use of the term “methodology,” see Jackson 2016.

⁷ After all, mind-world dualism can also lead to a critical realist disclosure of real-but-undetectable causal powers, which is a far cry from a search for nomothetic generalizations. On this alternative, see Patomäki and Wight (2000).

⁸ Schaffer 2016, 2.

⁹ Schaffer 2016, 74.

¹⁰ Schaffer 2016, 83.

¹¹ Schaffer 2016, 16.

gation. We are thus enjoined to look at language-in-use in our explanatory theories, presumably because to do otherwise would be to commit the same sin that Sartori commits when he advances an abstract concept of the family designed to travel across contexts: “he reifies a particular and partial conception of what a family is” and thus “risks guiding in unexamined ways the thinking of anyone who adopts such a definition.”¹² Sticking close to how people themselves use terms is the antidote to such a scholarly imposition.

But all of this makes very specific assumptions about the purpose and potential of scholarship. Knowledge about how a group of people conventionally uses a term might suffice as an explanation of an outcome if we had a theoretical reason for connecting conventional uses and outcomes—but such a connection is not an inevitable consequence of the close examination of conventional use per se. But the connection between conventional uses and outcomes *is* a consequence of the rejection of “positivism” understood as a more or less behaviorist quest for systematic correlations. The implicit argument seems to be that if we are to take people’s conventional uses of terms—their concepts—seriously, then we have no choice but to restrict scholarly explanation to an exercise of explication: an outcome or arrangement is explained when we disclose the concepts in terms of which that outcome is comprehensible to the people involved. In such a conception, operational concepts can’t be wrong, and people can’t be mistaken. It is difficult to see how we might explain a situation as produced by something people were doing without meaning to do it. People also cannot be unaware of the reasons for which they do things, affected by factors that lie outside of their conceptual universes. It is also unclear how a scholar might *critique* concepts, whether her own or those of the people under investigation, except by bringing to light their false universalism; there does not seem to be much space here for anything like a better concept.

Many of these criticisms likely sound like the sort of thing a “positivist” might deploy against an “interpretivist,” since in Schaffer’s account those are the only alternatives. I do not agree. We are not faced with a dichotomous choice between a mind-world dualist strategy of producing universal concepts that populate nomothetic generalizations and a mind-world monist strategy of explicating diverse patterns of the conventional use of terms that results in enhanced awareness of the locality of meaning and a consequent privileging of the experience-near language of participants in our scholarly accounts. We are instead faced with a series of choices involving different philosophical registers, and a plethora of ways of making good use of Schaffer’s excellent technical advice about a variety of methods of grounding and locating concepts. One can act on Schaffer’s advice and pay attention to operational concepts understood as terms in conventional everyday use for a number of different reasons:

In the *theoretical* register, we might broadly distinguish¹³ between experience-near theories that focus on shared mean-

ings as an explanatory factor, and social-relational theories that focus on patterns of transaction and the positions of actors relative to one another. The former would engage in grounding and locating in order to make sense of the cultural lifeworld of the actors being investigated, aiming to disclose the situationally-specific rules of, for example, how competence is defined and negotiated in a diplomatic setting¹⁴ or what makes for an acceptable public statement about foreign policy¹⁵ or an acceptable course of action regarding sovereign prerogatives.¹⁶ The latter would engage in grounding and locating in order to track how the deployment and invocation of particular concepts generate outcomes, or how available concepts are shaped by networks and specific histories; here the emphasis is on sketching transposable mechanisms like brokerage¹⁷ or legitimation¹⁸ and investigating how they play out in specific cases. In a way, the former is more interested in the *content* of a concept, while the latter privileges the *form*, either of the overall conceptual space or of particular types of rhetorical deployment, and while the former aims to produce “thick” locally specific knowledge, the latter aims to refine “thin” ideal-typical mechanisms and processes that can inform singular causal accounts.¹⁹

In the *methodological* register, we might broadly distinguish²⁰ between at least four different ways of producing social-scientific knowledge: subsuming outcomes under general laws, identifying dispositional causal capacities, elaborating ideal-typical models, and reflexively grounding claims in the social position of the social scientist herself. Each of these four approaches would have a use for concepts understood as experience-near and grounded in conventional everyday usage, whether that was coding an operative meaning variable, abductively inferring a condition of possibility for a way of using terms, connecting the terms of a model to the local cultural context of the scholar herself, or dispelling the white-washed universality of assuming that every place and every social situation is fundamentally identical and thus seamlessly translatable into one’s own default native language.

Also operating in the methodological register, we might broadly distinguish between descriptive knowledge intended to communicate facts, causal knowledge intended to convey the skills and capacities involved in making something happen, and a kind of knowledge intended to equip the recipient to go on appropriately—a type of knowledge I would personally prefer to call “interpretive,” with full awareness that this is a considerably narrower use of the term than Schaffer might want. As with the four social-scientific methodologies men-

¹⁴ Pouliot 2016.

¹⁵ Neumann 2012.

¹⁶ Adler-Nissen 2015.

¹⁷ Tilly 1998; Nexon 2009.

¹⁸ Jackson 2006; Goddard and Krebs 2015.

¹⁹ In actual scholarly practice, this is not a hard and fast distinction, and “practice turn” and “processual/relational” accounts frequently incorporate both types of theorizing—as did a strain of “constructivist” theorizing that differed quite significantly from the U.S.-mainstream version (McCourt 2016).

²⁰ As I do in Jackson 2016.

¹² Schaffer 2016, 14.

¹³ Here I invoke a distinction explored in Jackson and Nexon (2013).

tioned in the previous paragraph, Schaffer's techniques for grounding and locating concepts can contribute to any of these three kinds of knowledge: we could engage in locating and grounding in order to answer questions about what some group of people mean by some term (descriptive knowledge), questions about which terms we might use ourselves in order to provoke a particular response (causal knowledge), or questions about how to use terms properly as a member of the community in question (in my lexicon if not in Schaffer's, interpretive knowledge).²¹

By breaking apart Schaffer's "interpretivism" and "positivism" into a series of commitments in different philosophical registers, we produce more thinking space, more possible combinations of substantive claims about the world and ways of evaluating those claims. We also disrupt the false dichotomization that would invariably connect examinations of how terms are used in practice with particular theories and methodologies, to the exclusion of other combinations. There is nothing whatsoever about studying how people use terms that commits us to any particular kind of theory or flavor of methodology. Looking at the everyday use of terms in order to ground and locate concepts does not lock us into theories that connect shared meaning to outcomes or obligate us to eschew causal explanations in favor of something like "understanding." The fact that many self-proclaimed "interpretivists" do in fact combine a focus on everyday language with a rejection of causation does not mean that the combination itself forms some kind of seamless and compelling logical whole.

Indeed, moving beyond the "positivist"/"interpretivist" dichotomy makes it possible to articulate an approach to conceptual analysis that avoids the problems of objectivism, one-sidedness, and false universalism, which Schaffer rightly criticizes,²² but *also* serves causal-explanatory purposes. Such an approach might involve using the precise grounding and locating of concepts not so much to "destabilize" putatively "timeless, essential properties"²³ as to show the *relatively* stable patterns that emerge in the course of actual lived experience. Ideal-typifying such patterns would allow us to investigate how situated social actors deploy terms and the concepts they carry with them, and how such deployments produce outcomes: through processes of legitimation, boundary-creation and boundary-maintenance, and the foreclosing of otherwise-possible courses of action. Proceeding in this way means that the patterns of use that figure into the resulting causal account are only analytical instruments; their apparent "universality" is only a logical generality, and not any kind of

a categorical pronouncement about the essence of a concept or its limitless empirical applicability. And although an ideal-typified notion is by definition²⁴ "one-sided," this is what we might call a *tactical* one-sidedness for the purpose of explaining a specific outcome: what is extracted from lived experience are the broad outlines of how the group being investigated actually uses the relevant terms, and this is done not in order to elucidate one or another dimension of how a concept *might* be defined, but to explain how *this* outcome arises from *this* conventional use.

Ideal-typification thus provides a potential response to two of the problems Schaffer identifies: universality and one-sidedness. As for "objectivism," another problem he identifies, Schaffer worries that treating concepts in anything but an "interpretivist" way means that scholarly treatments of a concept present "a seemingly value-free, objective definition that appears to be generated by a purely analytic set of operations (identifying essential properties, etc.) and detached from a broader context of political contestation."²⁵ This might be the case if we were talking about scholarship that intended to define a concept, and thus to tell audiences both inside and outside of the academy precisely what something means. But I very much doubt that this is the intent of much social-scientific scholarship, and I would argue that it *shouldn't* be our intent when we are operating as social scientists or as any other kind of scientist. Our job—our *vocation*—when operating in a scientific idiom is to produce well-reasoned conclusions about matters of fact: descriptions and explanations that are systematic, public, and worldly insofar as they bracket the realms of the divine and the transcendent in order to focus on producing "disenchanted" accounts.²⁶ So I, as a social scientist, should never be pronouncing judgment on what, for example, "democracy" *is*; rather, I should be using a precisely defined notion of "democracy" in my descriptions and explanations, and explicitly refraining from using that definition to legislate appropriate and inappropriate use by others. In that way, a better awareness of the limits of the social-scientific enterprise functions to disrupt "objectivism," and in so doing can address Schaffer's concerns.

Of course, I am very aware that the philosophical limits of social-scientific inquiry are often blurred in practice, as political and social actors either look to scholarly definitions as guidelines for their own thinking, or deploy scholarship as part of a campaign to produce one or another practical outcome. Sometimes we scholars even aid and abet that process. Regardless, my point is that the problems of "positivism" that Schaffer identifies are *not*, in my view, philosophical problems so much as they are institutional and political problems. I think that Schaffer is quite correct that we can start to address these problems by bringing our technical language "back to the rough ground," as Wittgenstein might put it,²⁷ and regarding it as nothing other than ordinary language in a specific con-

²¹ Knowledge that criticizes particular concepts is also interpretive knowledge in the sense I am using the term here: our use of our concepts to critique what some other group is doing is an interpretation of their actions through the lens of our concepts and thus tell us how to *go on* in our community. Our use of our concepts to clarify what we ourselves ought to be doing is an interpretation of our own context that tells us how to go on in our community (as in Wittgenstein 1953). All of these are variants on the problem of *how to use terms properly as a member of a community*.

²² Schaffer 2016, 13–20.

²³ Schaffer 2016, 86.

²⁴ By Weber's 1999 definition.

²⁵ Schaffer 2016, 19.

²⁶ "Disenchanted" in Max Weber's (2004, 14–19) sense.

²⁷ Wittgenstein 1953, sec. 107.

text.²⁸ But I disagree that the only important contribution that grounding and locating scholarly concepts can make is to re-activate histories of contestation and make us aware that our scholarly notions do not drop intact from some rationalist heaven. Even if some of us need to be reminded of that from time to time, I very much doubt that pointing to past and ongoing social contestation about the meaning of a concept is going to do the trick. In my view, reflexive critique of our own scholarly concepts should start by clarifying how we as social scientists are *supposed to* use concepts—and that in turn comes from (to invoke Wittgenstein again) an investigation of our own “form of life” intended not to explain outcomes, but to normatively prescribe ways of appropriately “going on.” If we did that, in my view, we would be in much better shape to contribute to ongoing conversations in a complex and turbulent world.

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A Few Words about Methodology

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In mulling over how to most productively respond to the reflections offered by Lahra Smith, Gary Goertz, and Patrick Jackson, I tried to place myself in the armchair of a *Qualitative & Multi-Method Research* reader. What big methodological questions, I asked myself, are raised by their reviews of my book? How might I weigh in, generatively, on those questions?

One distinctive feature of this newsletter is that it provides a forum for political scientists of diverse methodological commitments to speak to one another. It serves as a platform for thinking together (and sometimes arguing against one another) about what those epistemological and ontological commitments are and how they matter. One area on which all three contributors and I agree is that such commitments matter for how we work with concepts. There is less consensus among us on how to characterize those underlying methodological differences, so it is on this question that I will mostly focus in this response.

My own view is that it makes sense to distinguish, broadly, two loose communities of scholars who hold different clusters of methodological commitment. Here is how I describe those commitments in *Elucidating Social Science Concepts*:

A widely shared methodological commitment of positivism, as I understand it, is a belief that social scientists can directly and neutrally observe a social world that is made up of entities (like families and classes and revolutions) that enjoy, or are treated as if they enjoy, a real existence independent of how people think of them. The aim of much positivist inquiry is, correspondingly, to formulate propositions about those entities based upon the identification and measurement of regularities within and between them. An interpretivist approach to social science, in contrast, usually starts from the dual premises that there are no “real” social entities, only culturally mediated social facts, and that social science is always perspectival and entwined with the pursuit of moral or material goals. The aim of much interpretivist inquiry, consequently, is to shed light on how shared meanings and their relation to power inform or structure the social world and the study of the social world.¹

I hedge so many of these claims (“much,” “usually,” etc.) because I think that there is a good deal of diversity in what scholars actually think and do. Again from my book:

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¹ Schaffer 2016, 2.

²⁸ Schaffer writes: “In their own arena of practice, social scientists’ language is ordinary language and can be analyzed as such” (Schaffer 2016, 32).

Positivism and interpretivism are not the banners of monolithic, rival camps of scholars who all walk in lockstep with their respective methodological comrades. Some scholars move back and forth between these orientations. Others seek ways to cut a middle path between them. Even individual scholars who work largely within one tradition or the other may hold views that correspond only partially with, or perhaps differ from, the ones that I have laid out.²

Another point worth clarifying is that drawing a distinction between positivism and interpretivism should not be taken to mean that these two approaches—even when enacted in their most dissimilar forms—share nothing in common. Among other things, scholars working in both traditions typically believe that good research should be trustworthy and that the achievement of trustworthiness requires attention to method, while disagreements are often about what counts as trustworthiness or which methods should be used to secure it.³

Such complications notwithstanding, I posit that many scholars do hold or enact the methodological views I describe, or something like them, and that whether one brings a more positivist or more interpretivist orientation to the study of the social world matters for how one thinks about and works with concepts. As best as I can tell, within positivist methodology the central conceptual task is, typically, to generate, by means of reconstruction (often called “concept formation”), a precise terminology that faithfully represents a reality taken to be independently pre-existing. The scholar’s own near-to-hand experience is usually viewed as the raw material out of which this scientific language is constructed, while the experience of those being studied is valued for the information about individual opinions or perspectives that it yields. Within interpretive methodology, in contrast, the central conceptual task is, typically, to elucidate the everyday categories of those being studied as well as the scientific categories of those doing the studying. This elucidation involves, more specifically, mediating between the experience-near language of the people who are the subject of research and the experience-distant terminology of the scholarly community.⁴ Such an approach requires the interpretivist social scientist to see both experience-near and -distant concepts as intersubjectively meaningful, socially constitutive, and part of a broader politics of concept use.⁵

It is sometimes asserted that the main difference between positivist- and interpretivist-oriented scholars is that the former embrace the task of causal explanation while the latter reject it; Jackson’s review, for instance, characterizes my own position

² Schaffer 2016, 24.

³ In my graduate-level Comparative Politics Proseminar I have students search for commonalities in King, Keohane, and Verba (1994, 3–33), Mahoney (2010), and Schwartz-Shea (2014). They find plenty. The exercise is highly illuminating; try it yourself!

⁴ I borrow the distinction between “experience-near” and “experience-distant” language from Clifford Geertz (1983, 57). The former consists of commonplace words used in everyday contexts, whereas the latter consists of words employed by specialists in ways that are extra-ordinary and experientially remote.

⁵ Schaffer 2016, 4–10.

in the latter way.⁶ Such an assertion, I believe, is erroneous.

Goertz is correct when he notes that many interpretivists *do* seek to explain and *are* interested in causes.⁷ 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. Illuminating on this point are comments made by Clifford Geertz in an interview he gave to John Gerring for this newsletter. His words are worth quoting at length:

If you get interpretation right, I believe the causes will fall out. If you understand the cockfight, you’ll understand why people are engaging in it, why things are happening the way they are happening. It must be clear, of course, whether you’re talking about a cause or a causal law. There’s a big difference. Everything is caused. On the molar level there is no uncaused behavior. If I look at the cockfight and something happens, I don’t say “There’s no cause for this, it just happened.” I don’t write that way, and no one really writes that way. So that isn’t what we’re doing. But the question about causal laws gets more complicated. There’s one issue concerning the difference between causality and determinism. If you are familiar with Elizabeth Anscombe’s work, you will understand that the search for causes is close to detective work. You come in and you find the pitcher has fallen to the floor and there’s glass. Did the cat push it, or did the wind blow it over? The one thing you know is that there is a cause. It’s on the floor and there’s milk all over. But whether the cat did it or the wind did it, or you put it down in a way that made it tumble later on—there are evidently lots of possibilities. The point is, you need to have the story of what happened. But you don’t have to have a causal law. There’s no causal law that cats tip over milk.⁸

Even the thickest interpretivist description is not an alternative to explanation. Such a description is a specific kind of explanation, a “story of what happened” as Geertz says. Causes are not absent from it, just more situated and particularized than they are in explanations couched in the language of laws or mechanisms.

One conclusion that I reach about current debates over the status of causes and explanation is that the vocabulary social scientists use to conduct their research, the intellectual scaffolding that they erect, is in need of greater elucidation. Even the most cursory examination of “cause” reveals (at least) two distinct uses. In the first use, which derives from David Hume’s philosophy, causes are seen as forces external to the individual. In the second, much older use, reasons and

⁶ See Jackson (2016) in this symposium.

⁷ See Goertz (2016) in this symposium.

⁸ Gerring 2003, 27.

motives themselves can be causes. The latter understanding of causes, by the way, is still present in the legal realm where it is embodied in expressions like “probable cause,” “having good cause,” and “cause of action”—all of which refer in some way to having proper or adequate reasons for acting in a particular way.⁹ I suspect that some interpretivists who avoid explicit talk about causes do so not, or not only, because they are devoted to the kind of thick storytelling described by Geertz; they may also, or instead, do so because they have come to see causes as purely external forces. John Gunnell, for one, seems to adopt the latter position when he argues that “any thoroughgoing attempt to explain action and the relationship between mental episodes and observed behavior in causal terms, that is, [in] the language of physical events, will necessarily founder.”¹⁰ “Cause,” of course, is not the only term that would profit from elucidation. Also in need of analysis are terms like “fact,” “case,” “data,” “mechanism,” “reflexivity,” “explanation,” “qualitative,” “quantitative,” and most salient for the topic of this symposium, “methodology,” “interpretivism,” “positivism,” and “concept.”

One of my aims in writing *Elucidating Social Science Concepts* was to provide scholars with tools to critically reflect on their use of such terms, tools that I believe can help them use more carefully and self-consciously the language in which and with which they think. Another of my aims in writing the book was to simply open up more spaces for thinking. As Smith generously phrased it, a promise of the book is to “create new windows of inquiry” and “initiate a process of political imagination.”¹¹ The utility of the book for this purpose is borne out in Smith’s own contribution to the symposium. In her hands the book becomes a prompt to raise a whole series of probing questions about her own area of research: the contextually specific aims of protesters in Ethiopia. Most intriguing to me is the possibility of interrogating more deeply even the basic characterization of what was going on there as people “protesting.” Does it make sense, she asks, to think of Ethiopians taking to the streets as “protesters” even though Ethiopians themselves have different languages and vocabularies that possibly reflect different self-understandings? The “protester” marching out on the street, I would point out, is neither a transhistorical nor a transcultural personage. Prior to the 17th century, a protester in England was not someone who objected or complained, but someone who avowed or declared solemnly; someone who for instance made protestations of love. Today, even languages that are fairly similar to English have terms that are only roughly equivalent to “protester.” In Spanish, for instance, people use the word “*manifestante*.” If the protester is someone who expresses dissent by means of public declaration, the *manifestante* is someone who displays (makes manifest) their dissent in their actions. Historically, the Protestant reformation seems to have decisively shaped how English-speakers today conceive of protesters, a history not shared by everyone around the world. What might we not be

noticing when we unreflectively label people back in time and in other cultures “protesters”? How did or do they themselves conceive of their own actions?

Such questions about self-understandings “matter” or have “instrumental value”—to take up a question posed by Goertz in his review—for at least two kinds of reasons.¹² For one, they help us gain insight into what people (in 2015 Ethiopia, in 2011 Spain, in 14th century England) understood themselves to be doing. They thereby give us a thicker story of what happened, a richer explanation of what was going on.¹³ This does not mean scholars need to imprison themselves in the language used by the people that they study or that scholars cannot think critically about the local self-understandings that they uncover. On the contrary, much interpretivist work brings to self-understandings what Paul Ricoeur called a hermeneutics of “suspicion”¹⁴—a sensitivity to the ways in which people’s self-descriptions may be shallow, deluded, deceitful, shaped by power relations, or the like. What this does mean is that scholars can come up with fuller explanations of what happened and guard against various forms of misrecognition if they take those self-understandings into account. Exemplary in this regard is the work that Lee Ann Fujii¹⁵ has done on the interpretation of lying in post-genocide Rwanda.

Attending to the self-understandings of people different from us (whoever “we” may be) also matters because such understandings can enlarge our own political imagination. By moving back and forth between our own commonsense and the commonsense of other communities of people, by investigating historically or cross-culturally basic concepts of political life, the promise is to disturb the taken-for-grantedness of our own views. This disturbance, hopefully, will allow us to see, critique, and change aspects of our political life that we had previously left unexamined. To put it another way, one aim of historically or cross-culturally elucidating a familiar concept like *protester* is to loosen the hold of common sense over us. Quentin Skinner¹⁶ likens this grip of our common sense to bewitchment. The seeming naturalness of our views deludes us into believing that our current way of thinking is *the* way of thinking. Seeing that things can be otherwise offers, as Skinner puts it, a kind of “exorcism” which can help break that spell.¹⁷ Elucidation opens to view new or forgotten ways of thinking from which we might learn.

I turn now to the critique of *Elucidating Social Science Concepts* put forward by Jackson in his review.¹⁸ He takes issue with my using the categories of positivism and interpretivism because, as he puts it, they are not “coherent intellectual packages.” They are, in his view, an oversimplified binary that forces upon the social scientist a dichotomous set

⁹ Schaffer 2013.

¹⁰ Gunnell 1968, 193.

¹¹ See Smith (2016) in this symposium.

¹² See Goertz (2016) in this symposium.

¹³ I unpack this argument in Schaffer (1998, 86–115) and Schaffer (2016, 16, 89–90).

¹⁴ Ricoeur 1970, 32–36.

¹⁵ Fujii 2010.

¹⁶ Skinner 2002, 6.

¹⁷ Skinner 2002, 6.

¹⁸ See Jackson (2016) in this symposium.

of choices that unduly restrict analytic opportunities. Jackson prefers instead his own typology, which he has laid out in his *Conduct of Inquiry in International Relations*.¹⁹ This typology rests on two dimensions: the relationship between the knower and the known (one can be committed to either mind-world dualism or mind-world monism) and the relationship between knowledge and observation (one can be committed to either phenomenalism or transfactualism). Combining these two dimensions in different ways results for him in four possible methodological commitments: “neopositivism” (mind-world dualism + phenomenalism), “critical realism” (mind-world dualism + transfactualism), “analyticism” (mind-world monism + phenomenalism), and “reflexivity” (mind-world monism + transfactualism).²⁰

I commend Jackson for his efforts to think systematically about methodology and I recommend his book highly. All the same, I think he is mistaken to brush aside the categories of positivism and interpretivism for in doing so he dismisses what scholars in many empirical fields of research actually do and the distinctions that many members of epistemic communities—both in and beyond political science—actually make. His own typology rests on philosophical positions that he himself packages together. But the distinction between positivism and interpretivism is, as he acknowledges, “practically operative” and corresponds to “existing research practice.” I would add that authors from a variety of disciplines have produced over the past forty-plus years a whole body of scholarship devoted to working out what they themselves call “interpretive” methodology.²¹ Even scholars working outside the interpretivist tradition acknowledge the salience of *interpretivism* as a category for talking about methodology. For instance, Goertz and James Mahoney,²² in their own critically important methodological intervention, recognize the distinctiveness of interpretivism even if they do not include a discussion of it in their book about qualitative and quantitative research cultures: “Interpretive approaches are not featured in our two cultures argument...Such a book would bring to light fundamental clashes over epistemology and ontology that exist within parts of the social sciences.”²³

Jackson relies almost exclusively on the experience-distant concepts of the philosopher with little attention to the self-understandings of actual research communities as they exist in practice. The risks of doing so are to misrecognize what scholars in those communities are in fact doing and to overlook some of the methodological richness of their scholarship. These risks, with regard to interpretivist scholars and scholarship, reveal themselves in Jackson’s reading of my book. He claims, to give just one example, that by my account “it is...unclear how a scholar might critique concepts” because

“we have no choice but to restrict scholarly explanation to an exercise of explication.” One line of criticism here, as I understand it, is the charge that the kind of elucidative strategies that I propose cannot be used by scholars to transcend or gain critical distance from the self-descriptions of the people being studied. This rendering of my argument regrettably passes over a central distinction that my book makes between “departing from” and “disregarding” such self-understandings.²⁴ A scholar with an interpretivist sensibility, I argue, should be free to depart from a person’s self-understanding but not to disregard it. Departures add analytic insight but nonetheless need to be tethered somehow to how people understand themselves and the categories that they use. As I put it elsewhere in the book, “we often benefit from the broadened perspective that experience-distant language provides—as long as we take care not to lose or expel the experience-near from our field of sight.”²⁵ One example that I give in the book to clarify this argument is the term *genocide*. The Nazis never used this word; it was coined toward the end of the war by Professor Raphael Lemkin of Duke University. I believe that scholars with an interpretivist sensibility are justified in using this term to describe the murderous project of the Nazis. But a thick account of that genocide also requires investigating the (often deceitful and euphemistic) uses of terms like *Endlösung*, *Aussiedlung*, and *Sonderbehandlung* by the Nazis themselves, as ways of talking about mass murder. Genocide scholars should not feel compelled to encase their own analyses in the language of the Nazis, but neither should they ignore that language if they wish to take seriously the actor’s point of view.²⁶

I find Jackson’s remarks on this and similar points unfortunate since our two books may be more complementary than his review might lead one to believe. There is much to be gained, I would argue, by reading the books together and seeing how one might customize the various strategies of elucidation that I develop to fit within the four philosophical positions that he identifies. I deliberately presented the strategies of elucidation, after all, in a modular fashion so that they might be adapted to the specific but variegated needs of scholars. It is in this sense that I likened the book to a collection of recipes written for an adventurous cook.²⁷ In his review, Jackson himself does some of that adventurous, adaptive work.²⁸

Let me expand on this last point with a few more comments about the audience I imagined for *Elucidating Social Science Concepts*. I wrote it for inclusion in a book series on interpretivist methods, so the volume contains strategies for thinking about and working with concepts that I anticipated would be useful to scholars with an interpretivist sensibility.²⁹ Still, I hoped the book would appeal to other scholars as well.

²⁴ Schaffer 2016, 3.

²⁵ Schaffer 2016, 71.

²⁶ Schaffer 2016, 71, 73n12, 93.

²⁷ Schaffer 2016, xv.

²⁸ Jackson (2016) in this symposium.

²⁹ Other books in the series—the Routledge Series on Interpretive Methods—include Schwartz-Shea and Yanow (2012), Lynch (2014), and Shenhav (2015). Volumes by Fujii (on interviewing) and Timothy Pachirat (on ethnography) are forthcoming.

¹⁹ Jackson 2011.

²⁰ Jackson 2011, 37.

²¹ See, e.g., Taylor 1971; Geertz 1973; Rabinow and Sullivan 1979 and 1987; Hiley, Bohman, and Shusterman 1991; Schwartz-Shea and Yanow 2012.

²² Goertz and Mahoney 2012.

²³ Goertz and Mahoney 2012, 4–5.

For one, I hoped it might serve as an invitation to learn about or even experiment with a different set of methodological starting points and strategies for working with concepts. I thought the book might also be helpful to those positivist scholars interested in investigating the particular dangers or limits of reconstructing a concept in this or that way. Goertz is just the kind of scholar I had in mind.³⁰ Indeed, he shows in his symposium contribution how the strategies of elucidation contained in my book can be deployed to that end. There are many creative and insightful scholars working outside of the interpretivist tradition, so I have no doubt that they can work out still other ways to adapt elucidation to their own needs and interests.

In writing a book about concepts from an interpretivist standpoint, I found it necessary to point out the analytic shortcomings and ethical dangers that so often bedevil positivist conceptual reconstruction, the predominant way of working with concepts today in disciplines such as political science. But it bears emphasizing that interpretivist elucidation has its own analytic shortcoming and ethical dangers, some of which I discuss in the concluding chapter of the book. No matter our methodological commitments, we would all do well to take seriously the warning of Anne Norton: “The hope for a tool that will not turn in the hand, for a language that will speak without deception, for a method that cannot be used irresponsibly, is illusory.”³¹ Humility should be a watchword on all our lips.

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³⁰ Goertz is modest in not mentioning the excellent book that he co-edited, with Amy Mazur, on the pitfalls of ignoring gender when reconstructing concepts (Goertz and Mazur 2008).

³¹ Norton 2004, 135.

2016 QMMR Section Awards <https://doi.org/10.5281/zenodo.823318>

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, that 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 Macartan Humphreys (Columbia), chair; Katerina Linos (Berkeley); and Craig Parsons (U. of Oregon).

Winner of the 2016 Award: Ronald R. Krebs, *Narrative and the Making of US National Security*. New York: Cambridge University Press, 2015.

Honorable Mention: Anna Grzymala-Busse, *Nations under God: How Churches Use Moral Authority to Influence Policy*. Princeton: Princeton University Press, 2015.

Prize citation: Ronald Krebs' *Narrative and the Making of US National Security* takes on an important but very difficult question: What is the role of narrative in shaping decision making in international politics? His beautifully crafted book examines cases where leaders failed to seize opportunities to change dominant narratives as well as cases where they succeeded. Missed opportunities include FDR's failure to counter anti-interventionist narratives before Pearl Harbor, or Reagan's failure to win the public over to his position on the Sandinistas. Krebs suggests that FDR missed a chance to change the narrative while Reagan failed to marshal arguments in a context where the narrative frame was not in dispute. Seized opportunities include the "writing of the enemy" after Pearl Harbor as regimes rather than populations and the framing of the "war on terror" after 9/11. Causal inference is obviously difficult in all of these cases, but Krebs takes up the task in a remarkably comprehensive and careful way. He seeks evidence for mechanisms via text analysis, analyzes speeches and documents for evidence of rhetorical strategies, considers political context and conditions around this rhetoric, engages in explicit counterfactual analysis to identify historical clues for what might have happened had different choices been made, and directly confronts alternative explanations. This eclectic mix of methods supports a surprising and provocative argument that suggests that the space for altering narratives can be greatest in times of strength and not at moments of crisis.

In addition, the committee nominates Anna Grzymala-Busse's *Nations Under God* for honorable mention. This magisterial book examines the role of churches in policy formation across three paired sets of cases, examining when and why they have influence. Combining archival research, formal theory, and statistical analysis, Grzymala-Busse argues that in those cases in which national and religious identities are most closely aligned, churches enjoy unusual moral authority. They can use this

authority to gain effective policy influence while at the same time seeming to be detached from politics.

Alexander George Article/Chapter Award

This award recognizes the journal article or book chapter, published in the calendar 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 Carolyn Warner (Arizona State), chair; Sarah Parkinson (U. of Minnesota, now Johns Hopkins); and Jonathan Mercer (U. of Washington).

Winner of the 2015 Award: Thomas Rixen and Lora Anne Viola, "Putting Path Dependence in its Place: Toward a Taxonomy of Institutional Change." *Journal of Theoretical Politics* vol.27 no.2 (April 2015): 301–323.

Prize citation: The articles nominated for the 2016 award covered a diverse set of substantive topics and represented various theoretical and methodological approaches. The impressive range and quality of these articles made it difficult to select only one. However, we ultimately agreed that Thomas Rixen and Lora Anne Viola's "Putting Path Dependence in its Place" best exemplified the Alexander George Award's ideals.

Rixen and Viola's article makes a strong, broad contribution to methodological debate and provides a terrific example of empirical engagement. Their approach is grounded in and speaks to a long tradition of empirical work on institutional change. It sheds new light on concept stretching and concept proliferation, uses path dependence to illustrate those problems, and demonstrates how thinking clearly about path dependence advances empirical studies of institutional change. Rixen and Viola's careful and systematic development of their argument makes this piece an important contribution to the literatures on qualitative methods, case studies, path dependence, and institutional change. Moreover, while it speaks to an empirical realm, it does not limit itself to or reproduce a specific intellectual silo; it is intellectually accessible and portable. It is a productive contribution in every sense of the word, and admirably carries on the intellectual ambitions of Alexander George.

Sage Best 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 Rasmus Brun Pedersen (Aarhus University); Ramazan Kilinc (U. of Omaha); and Daniel Beland (U. of Saskatchewan).

Winner of the 2016 Award: Erica S. Simmons and Nicholas Rush Smith, "Comparison and Ethnography: What Each Can Learn from the Other."

Prize citation: Ethnography and comparative research are typically perceived as incompatible forms of inquiry. Yet, in their innovative paper, Simmons and Smith suggest that comparative researchers can learn from ethnographers, and vice versa. More important, their paper outlines a comparative approach to ethnography that is both novel and compelling. This paper proposes a highly original combination of ethnography and comparative method that offers new and important insights. Their claim that at least a certain type of comparative thinking is compatible with ethnographic research is illustrated by concrete and relevant empirical examples. This is a paper that many scholars could learn from, within different sub-fields of political science and even beyond, as social scientists from other disciplines could find the comparative ethnography most relevant, as a methodological approach in qualitative analysis. The deals with crucial issues in a clear and innovative manner while potentially fostering inter-field and interdisciplinary dialogues.

David Collier Mid-Career Achievement Award

This award recognizes distinction in methodological publications, innovative application of qualitative and multi-method approaches to substantive research, and/or institutional contributions to this area of methodology. The selection committee consisted of Peter Hall (Harvard), chair; John Gerring (Boston University); James Mahoney (Northwestern); and Lisa Wedeen (Chicago).

Winner of the 2016 Award: Lauren Morris MacLean, Indiana University

Prize citation: Lauren M. MacLean of Indiana University is a worthy recipient of the David Collier Award for mid-career achievement. She has made excellent use of multiple methods in her own research, well-reflected in her book on *Informal Institutions and Citizenship in Rural Africa* and advanced our understanding of methods as co-author of an important text on *Field Research in Political Science*. Moreover, her contributions to teaching and learning methods, in many forums, have been outstanding. For four years, she was a co-instructor for the QMMR short course on 'Designing and Conducting Field Research' and she has made signal contributions to advancing the understanding of methods among African scholars, raising funds to bring African scholars to the IQMR workshops and running workshops on research issues in Africa. For her fine scholarship and dedication to advancing the methods of the discipline, the committee is pleased to make this award to Professor MacLean.

Note: Funding for the award for "**best qualitative submission to the APSR**," given since 2012, expired with the 2015 award, and it was therefore no longer given in 2016.

Announcements

<https://doi.org/10.5281/zenodo.823317>

2017 ECPR Summer School in Methods – Central European University, Budapest, Hungary

Good research requires a solid methodology. The vision of the ECPR Methods School is to offer an unparalleled breadth and depth of courses offered in an informal, stimulating teaching setting, at the lowest possible cost to participants.

Held at the Central European University in Budapest from the 27th of July to 12th of August, 2017, the 12th ECPR Summer School is comprised of 44 main courses either lasting one or two weeks, and 14 intensive refresher courses held over three days before the main courses.

The ECPR's Methods School offers start-of-the-art methods training across the whole range of methodologies (and across different paradigms and approaches), which are particularly salient for research questions in political science and neighboring disciplines, as well as dealing with all stages of a project and catering to the needs of research set at the macro and at the micro level.

While primarily aimed at Ph.D. students, courses can also be taken by junior and more senior faculties members. Courses offered range from introductory courses to a method or approach, to more specialized, advanced courses on innovative new methods and techniques. Courses cover both quantitative and qualitative designs as well as more positivist and more interpretative perspectives. Courses can either be followed on a stand-alone basis or can be combined over one or more events to provide a fuller, more comprehensive course of training.

The School comprises an intensive program of seminars and lab sessions that require a strong commitment from participants. The teaching language is English, and therefore all participants must be fluent in spoken English.

A sample of courses at the 2017 Summer School:

Introduction to Relational Social Science - Pragmatic Analytics – Patrick Jackson

Introduction to Reflectivist research designs - Xymena Kurowska

Introduction to Interpretive Research Designs - Peregrine Schwartz-Shea

Applied Mixed Methods Research – Erin Jenne

Case Study Research: Method and Practice – Ingo Rohlfing

Qualitative Comparative Analysis and Fuzzy Sets – Patrick Mello and Carsten Schneider

Causal Inference in the Social Sciences – Daniel Horn

Psychometrics and Item Response Theory – Stanley Feldman

Field Research I and II - Cai Wilkinson and Dvora Yanow

Big Data Analysis in the Social Sciences – Pablo Barbera

Applied Multilevel Regression Modelling – Zoltan Fazekas

Advanced Structural Equation Modeling – Levi Littvay

A new format is also offered this year: two 3-day 'à la carte' workshops for seasoned scholars (27-29 July): a "quantitative methods" workshop and a "qualitative, interpretive, case-oriented and comparative methods" workshop.

More information on the full range of courses and application procedures can be found at: <https://ecpr.eu/Events/EventDetails.aspx?EventID=116>.

Registrations open on 22 March; early bird discount up till 21 April.