

Qualitative Methods

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Organized Section for Qualitative and
Multi-Method Research

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Letter from the Section President

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First off, I want to thank Craig Thomas and Hillel Soifer for putting together an exciting set of panels, roundtables, and poster presentations for this year's APSA meetings, to be held in Boston (see pages 39-43 for the listings). The section received a record number of submissions. Even with our large panel allocation, the division chairs were faced with some very difficult choices. Although they were able to field a substantial proportion of these proposals, there were of course some that the section was not able to include. The long-term solution is to secure a larger number of panels. Let me, therefore, take this opportunity to remind everyone that the section's panel allocation at APSA 2009 depends not only on our submission rate but also, more importantly, on our attendance rate (which is carefully monitored by APSA). So, *Methodologists Unite!*—preferably at our panels. I also hope that you'll come to the section's annual business meeting (6:15pm–7:15pm) and reception (7:30pm–9:00pm) on Thursday, August 28.

At this year's APSA, the section will be also organizing and co-organizing its usual broad range of activities. These include two short courses (see page 38), the Methods Café (see page 40), and a working group (“Methodology: New Perspectives on Qualitative and Quantitative Tools”).

I also want to thank all of those who served this past year on the nominating committee, the executive committee, and our book, article and paper awards committees. Most of all, I want to thank Jim Mahoney (the departing president), Colin Elman (the current secretary-treasurer), and Gary Goertz (the current newsletter editor). Thanks largely to Jim, Colin, and Gary, the section continues to thrive. Indeed, we are one of the largest sections of the American Political Science Association, with approximately 1,000 members.

Next year, the annual Institute for Qualitative and Multi-Method Research will move from Arizona State University to its new home at Syracuse University, where it will be co-hosted by the Moynihan Institute of Global Affairs in the Maxwell School. Respecting the force of climate, the date of the next institute will be moved from its habitual January slot to summer 2009 (see page 38).

As a final note, I want to reiterate my commitment to methodological and epistemological diversity across the manifold

activities of the section. As I see it, the job of this section is to represent a broad range of methodologically relevant perspectives and to do so in a productive fashion—so that contending ideas meet one another in specific terms, rather than simply restate philosophical positions, engage in abstract labeling exercises (“positivist,” “interpretivist,” et al.), or ad hominem attacks. Speaking for the section (at least to the extent that I am entitled to do so), let me say that we really don’t care about the views of your pastor, or whether you were greeted with sniper fire at last year’s convention. We do, however, care about including your views on social science methodology in the

section’s activities.

A broad view of methodology is essential to the conduct of a professional body whose purposes include facilitating debate, encouraging the development of new methods, and helping to disseminate them. This commitment to diversity is reflected in the section’s recent name-change—from “Qualitative Methods” to “Qualitative and Multi-Method Research.” Although something of a mouthful, the new label is more capacious, as well as more in keeping with the actual diversity of methods and methodological perspectives that we find among our current membership. Well worth the extra syllables!

Symposium: Historical Causation

Causation in Time

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Social processes are rarely instantaneous. *Periodization*—specifying the beginning and ending of the temporal context within which a causal process plays out—is essential for a great many of the political processes that we study. Historically oriented political science research, in particular, is notable for its theoretically based expectation that various aspects of the temporal context matter for explaining outcomes. If comparative historical research is insufficiently attentive to the methodological importance of specifying completely the temporal context within which causal mechanisms work, then we can be sure that fault plagues other modes of political analysis, as well. We consider here some of the pitfalls inherent to the standard periodization techniques utilized by even those researchers most sensitive to temporal context.

As Pierson (2004) notes, a variety of aspects of time may be relevant to political explanation—not least because of the way they affect the functioning of causal mechanisms. Sequencing—*when things happen*—, either in world-historical time (Wallerstein 1974), in relation to signal events in political development like the development of working-class parties, or in relation to more contingent events or processes closer at hand (e.g., the availability of certain policy models), may affect how and whether a specific mechanism works. Falleti (2005), for example, shows that if a process of decentralization *begins* with what she terms “political,” rather than “administrative,” decentralization, the process will likely activate a policy-ratchet mechanism—the creation of a group of subnational supporters—that will affect the second round of decentralization reforms and lead to an increase in the amount of power transferred to subnational officials. The policy-ratchet mechanism is typically absent, however, if political decentralization occurs *after* administrative decentralization, a sequence that re-

sults in a lower degree of autonomy for subnational authorities. In this example, the sequencing of different types of decentralization policies affects which causal mechanisms come into play and its effect on the outcome of interest.

Tempo and duration—*how long things take*—may also suggest a likely set of plausible mechanisms. Outcomes that come about slowly, gradually, or after a long lag are likely to be produced by different kinds of mechanisms (policy drift, increasing returns) from those that produce outcomes that occur swiftly or suddenly (tipping points, rational choice) (see Pierson 2004, Chapter 3). Indeed, those political scientists who focus on *longue durée* processes have tended to emphasize structuralist, systems-oriented, and/or macro-social causal mechanisms, while scholars interested in the consequences of shorter-term processes often are more attuned to mechanisms posited at the level of individuals or collectivities of individuals.

In this essay, however, our focus is on a third aspect of temporality: *when things start*. Starting points have had particular relevance for historical institutionalist analysis because the notion of path dependence, which is at the center of many such analyses, relies on a well-specified starting point. Historical institutionalist scholars typically use starting points and critical junctures to delineate one context, “before,” in which a mechanism does not function, from a second context, “after,” in which it does function. We argue, however, that the act of periodizing as a way of marking shifts in context is often insufficiently theorized in historically oriented research, and runs into particular difficulties when confronted with causal mechanisms that operate at the aggregate- or structural-level rather than at the individual level.

Context and Periodization

In our previous collaborative work, we define causal mechanisms as *relatively abstract concepts denoting processes that can travel* from one specific instance or “episode” (Tilly 2001: 26) of causation to another, *and that explain how a hypothesized cause creates a particular outcome in a given context*. In turn, we define context as *the relevant aspects of a setting* (temporal, spatial, institutional, or analytical) *in which an array of initial conditions leads to an outcome of a defined scope and meaning via a specified causal mechanism*

or set of causal mechanisms (see Falleti and Lynch 2007). In other words, causal mechanisms are portable, but operate in a context-dependent fashion. Then, in order to develop causal theories we must be able to identify analytically equivalent contexts and from the temporal perspective specify where one context ends and another begins. For historical researchers, the passage of time is often the most obvious indication that a context has changed. So it is no surprise that in historically informed analyses, periodization plays an important role in the development of causal theories. But even self-consciously methodological works examining periodization in causal analysis often fail adequately to specify how one should place the markers that designate contextually homogeneous periods in time, or which aspects of a context must be constant within a given period and which may be allowed to vary.

Büthe (2002), for example, issues a plea for more careful attention to the placement of starting and ending points in research that utilizes historical narratives as case material. Büthe sees a tension between formal models, which provide “an explicit, deductively sound statement of the theoretical argument, separate from a particular empirical context” (482), and the analysis of complex causal processes over time, which often involve feedback loops or other forms of endogeneity. Decontextualized formal models may lead to invalid causal claims if they fail to consider sequencing. Büthe sees the analysis of historical narrative as a solution to this problem, but recognizes the difficulty of knowing where to start and end a narrative. In particular, he asks “how do we delineate a sequence of events so as to justify the imposition of a narrative beginning and end onto a continuous empirical record? How does the imposition of a narrative closure affect the generality of our conclusions?” (482). Ultimately, however, he is only able to offer the advice that “the specification of the *explanandum*...provides the criteria for choosing the beginning and end of the narrative” (488). Where the process to be explained does not have a “clear starting point (e.g., an exogenous shock)” and/or has not “run its course,” Büthe himself notes that even this advice will prove inadequate (487).

Büthe advocates delineating the beginning of a new context with reference to the onset of the causal mechanism that produces the outcome. Analyses that use critical junctures to delineate the beginning of a period are one example of this strategy. Critical junctures are often defined *ex post* as the starting point of a path-dependent causal process that leads to the outcome of interest. Many analyses situate the critical juncture at the point of some exogenous shock (war, depression, shift in commodity prices, etc.); nevertheless, some of the most widely read classic examples of critical junctures analysis (Moore 1966; Lipset and Rokkan 1967; Collier and Collier 1991) embed critical junctures in a richly detailed context, and make it clear that the outcome of the causal process that begins with the critical juncture may also be influenced by a variety of other features of the environment. Collier and Collier (1991, Chap 1), for example, note that the duration of a critical juncture need not be short, and that longer critical junctures often incorporate background conditions and cleavages into the production of the outcomes in question.

Some more recent analyses employing critical junctures, like these classic works, also recognize that although a juncture may be the starting point of a new “path,” it is also a product of what came before (see, for example, Hacker 2002). However, a new strain of theorizing about critical junctures tends to emphasize the contingency of such moments, highlighting their status as distinctive break-points with the previous context. Mahoney (2000) demands that the start of a path-dependent process be “contingent,” by which he means that “its explanation appears to fall outside of existing scientific theory.” Examples of these unpredictable events that may constitute critical junctures include exogenous shocks or particular decisions made by political actors, often with proper names (514). While Mahoney does not insist upon the equiprobability of pathways out of a critical juncture, his formulation does depart significantly from Collier and Collier’s (1991) notion that critical junctures may have strong links to the past, and that the outcome of interest may in fact be predictable given the weight of history that lies behind the critical juncture.

Similarly, Capoccia and Keleman (2007) emphasize the *delinking* from context that occurs at a critical juncture: “In institutional analysis, critical junctures are characterized by a situation in which the structural (that is, economic, cultural, ideological, organizational) influences on political action are significantly relaxed for a relatively short period” (343). Capoccia and Keleman distinguish their conception of a critical juncture, quite rightly, from Lipset and Rokkan’s (1967) and Collier and Collier’s (1991), who “embed” their critical junctures in antecedent conditions and in so doing downplay the agency of individual actors.

The periodization strategies advanced in both Büthe’s (2002) framework and in critical junctures analysis following Mahoney (2000) and Capoccia and Keleman (2007), implicitly or explicitly, define the initiation of a path-dependent process or historical narrative as the starting point of the context within which the causal mechanism unfolds. In other words, the start of the context surrounding the input→mechanism→output ($I \rightarrow M \rightarrow O$) pathway is bounded precisely by the beginning of the causal mechanism of interest. This conceptualization of a starting point is a useful tool for identifying the beginning of a path-dependent process and may also highlight mechanisms that take place at the level of individuals or groups. It is not a good guide, however, to continuity and change in other important aspects of the context in which the causal mechanism plays out and that may have an important effect on the outcome of interest.

Lieberman (2001), in an article devoted to periodization strategies in historical institutionalist analysis, goes beyond critical junctures in his search for possible starting points. Lieberman’s typology identifies four types of starting points: the origination of a new institution of interest, or an important change in such an institution (in either case, the independent variable); an exogenous shock that changes the conditions in which the institution operates; or a change in some “rival independent variable” present in the “background” (1019, Table 1). This typology usefully points to the variety of po-

tential markers of the beginning of a new context, which need not all coincide with the onset of the mechanism presumed to be responsible for the outcome of interest. It also emphasizes that periodization may be based on activity in numerous layers of the context within which a causal process plays out (be they proximate institutions, background conditions, or truly exogenous events), a point to which we will return in a moment.

But while Lieberman introduces the idea that changes in a variety of different aspects of the context surrounding a causal mechanism may be consequential for the outcome of interest, his typology does not leave room for causal processes that might be generated by interaction or friction between the different aspects of the context. Yet, as Orren and Skowronek (1994: 321) note, the multiple layers or “orders” of institutions that constitute the polity or context at any given time are not “synchronized in their operations.” Rather, these orders “abrade against each other and, in the process, drive further change.” Lieberman’s strategies for periodization focus the attention on “important events, changes or turning points that can be conceptualized as markers of variation in a potentially important explanatory variable” (1017), which have “potentially important impact on the outcomes under investigation.” Lieberman notes that such events are relatively rare: “Within a mass of historical observations, only a few events define a period, whereas most other events and processes are explained as taking place during a period” (1017). This relatively narrow definition is nicely operational, but it also seems simultaneously to raise, and fail to grapple with, the central fact about context, i.e., its multi-layered composition.

Causation in Multi-Layered Contexts

Social processes take place in a context characterized by multiple overlapping layers of institutions and structures that govern the relationships between inputs and outcomes. Pawson (2000) cites as an example of this layered social reality the process of writing a check. Checks are “routinely accepted for payment only because we take for granted [their] place within the wider (institutional) rules of the banking system. The capacities that bring custom and order to the transaction do not reside solely with particular objects (checks) or agents (cashiers) but also belong to the institutional regulations (credit), legal constitutions (charters) and organizational structures (chancelleries)” (294). In historical analysis, we are likely to be concerned with a variety of contextual layers: those that are quite proximate to the input (*I*)—for example, in a study of the emergence of radical right-wing parties, one such layer might be the electoral system; exogenous shocks quite distant from (*I*) that might nevertheless affect the functioning of the mechanism and hence the outcome (e.g., a rise in the price of oil that slows the economy and makes voters more sensitive to higher taxes); and also the middle-range context that is neither completely exogenous nor tightly coupled to (*I*) and that may include other relevant institutions and structures (the tax system, social solidarity) as well as more atmospheric conditions, such as the rate of economic growth, flows of immigrants, trends in partisan identification, and the like. Lieberman (2001) conceives of this “background” context as the locus of “rival

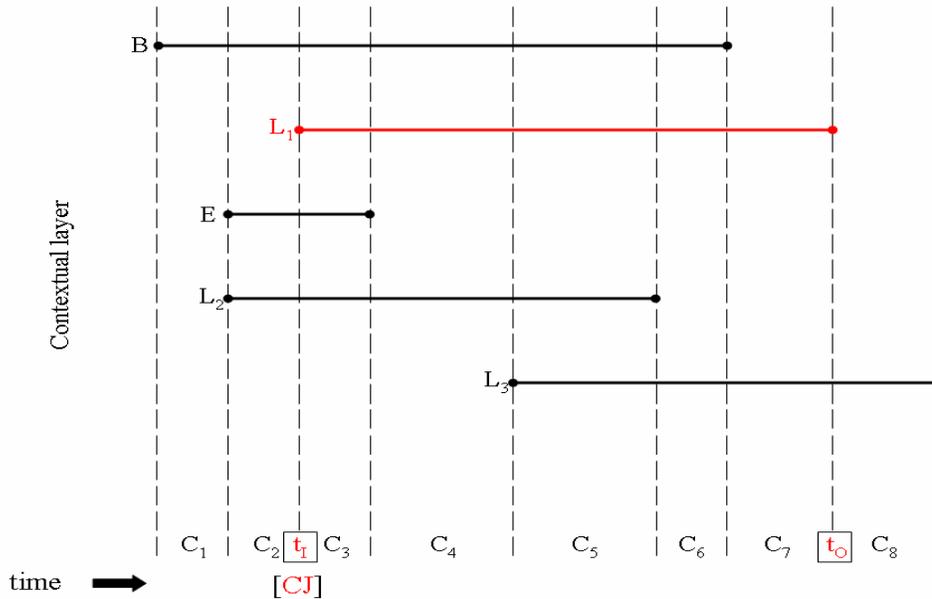
causes.” However, we believe that recent research (e.g., Hacker 2002; Streeck and Thelen 2005; Lynch 2006) bears out Orren and Skowronek’s (1994) contention that *the interaction of different layers of context may itself be the site of important causal mechanisms*.

Steinmo and Thelen (1992: 16-17) illustrate how changes in background conditions may affect the outcome of an institutionally-structured process, even if the direct institutional inputs do not themselves change. Changes in the social, economic, or political context may make previously marginal institutions more central to political life, bring new actors into play who use the same institutions to achieve different ends, or cause the same actors to pursue new goals through existing institutions. In all of these cases, the institutional inputs do not change, and the mechanism producing outcomes may or may not change, but the institutional outputs do change because the context has changed.

In *Age in the Welfare State*, Lynch (2006) illustrates how the multiple layers of context within which a causal mechanism operates can play an essential role in generating the outcome of interest—in this case, the extent to which social policies in different countries privilege the elderly over working-aged adults and children. In Lynch’s argument, two critical junctures mark choice points in the development of welfare state institutions, and path-dependent mechanisms tend to reinforce the choices made during these moments. But the age orientation of social policies in different countries cannot be satisfactorily explained within a framework that specifies critical junctures as moments of radical discontinuity. Rather, Lynch argues that processes occurring in three separate layers of context interact to produce the age orientations observed circa 1990. The first contextual layer is the political arena, where the policy preferences of parties and unions take shape; the second is the institutional arena of social policy programs; and the third is a layer composed of slow-moving background processes: population aging, the gradual closure of many Continental European labor markets to younger job-seekers, and the development of public and private markets for old-age insurance. Path-dependent mechanisms of institutional choice following critical junctures, which link the first and second layers, are not enough to explain the outcomes Lynch observes. Rather, much of the important “action” in Lynch’s analysis is caused by “policy drift” (Hacker 2004, 2005), a mechanism that links policy outcomes to the interaction between the first two (political and institutional) layers and the third (demographic and labor market) layer. It is worth noting that policy drift is in fact a mechanism that can *only* operate in a system characterized by multiple layers of relevant context.

If political contexts tend to be layered, with processes occurring at different speeds in different layers, and if some mechanisms are characteristic of the interaction of separate layers, then periodization in historical analysis should be attuned to the start and end points as well as to the tempo and duration of multiple processes in multiple layers. Consider a causal process that begins at time t_1 (for *input*) with a change in the main institution of interest, which is found in contextual

Figure 1: Periodization in Multi-Layered Contexts



layer L_1 , as in Figure 1. A critical junctures analysis would start the clock at time t_1 , tracing the outcome occurring at time t_0 (for *output*) back to the change in the institution in L_1 . In this case, the change in this institution follows closely (but not instantaneously) upon exogenous shock E (which itself spans considerably less time than most other elements in this diagram, but does have some measurable duration). Preceding the exogenous shock and lasting well past the critical juncture [CJ] at time t_1 , background condition B exerts a continuous influence on the unfolding of the causal process and can also be causally connected to the outcome of interest, t_0 . A second causal process linked to a change in contextual layer L_2 also predates and persists through the critical juncture, although its start and end points do not coincide neatly with B , either. Another process of potential relevance to O occurs in L_3 , but continues beyond the occurrence of t_0 . Our view of portable causal mechanisms implies that, under the same initial conditions, identical contexts will produce identical outcomes. But which context is the relevant one in this diagram? Only the temporal context starting at C_5 captures all of the major contextual layers, but it excludes the exogenous shock and resulting critical juncture.

It is clear from this schematic representation of unfolding causal processes in a layered context that a perfect periodization scheme may prove elusive, and that care must be taken, when making decisions about periodization, to specify which layers of context are relevant and in what ways. The use of critical junctures as starting points may pose particular problems in multi-layered contexts when important processes are not synchronized with the starting point of the $I \rightarrow M \rightarrow O$

pathway to which the critical juncture pertains.

Because the multiple layers of context that affect the outcomes of causal processes cannot all be expected to change at the same moment, dividing a historical narrative into periods based on the starting or ending point of a single causal process risks hiding from view precisely those interactions between layers moving at different speeds that can generate change over time. There are two crucial implications: first, critical junctures and other starting points that hone in on the initiation of a single $I \rightarrow M \rightarrow O$ pathway miss the causal impact of things that don't change at all, or don't change at the same time or pace as the critical juncture; and second, interactions between layers may be as important in producing outcomes of interest as any single causal mechanism.

Conclusion

If the context within which a social mechanism operates has many different institutional (or cognitive, or ideational, etc.) layers that may be relevant to the functioning of the mechanism, then periodizing as a method for generating contextually (and hence causally) homogeneous sub-units of a narrative, as Büthe (2002) recommends, becomes fraught with difficulties. If, as we argue, causal mechanisms are relatively abstract portable concepts whose causal force is given by the contours of the environment in which they operate, and if the contours of this environment change over time, then we must divide time into pieces within which the relevant context is constant in order to observe the causal mechanisms at work.

But when there are many layers of context that may affect the outcomes in which we are interested, whose properties

may change at different rates, that may be affected or not by different types of exogenous shocks, and that may or may not change at the same moment that the causal process we are observing begins, then how do we know where one context leaves off and another begins? We propose that as researchers we allow our theories about the social world to guide more strongly the selection of a relevant temporal context, rather than relying on “natural” starting points like major critical junctures or other historical breaking points.

We advocate periodizing based on important moments in those layers of the contextual environment that are likely to be most relevant to the outcome of interest from an explanatory point of view. Within the mass of all possible aspects of the environment that could be interconnected with the outcome, we must use theory to identify those that are most salient to the research question and hypotheses to be tested. Our research question, hypotheses, and the nature of the outcome of interest will determine which institutions, events, or background conditions are likely to be the most crucial. In Figure 1, for example, which of all the possible eight contexts (or their combinations) we choose to focus on will depend on which one our theories tell us is most likely to yield an “efficient” explanation for the outcome of interest.

An important corollary of this proposal is that no one type of starting point is ontologically superior to any other. Critical junctures or exogenous shocks are not inherently more interesting, or more causally important, than endogenously determined moments of institutional creation, or than the slow-moving changes that sometimes occur in the “background.” The context we choose may start with any one of these elements, it may contain some or all of them, or it may cut across the linear temporalities initiated with events, institutions, and background conditions.

References

- Büthe, Tim. 2002. “Taking Temporality Seriously: Modeling History and the Use of Narratives as Evidence.” *American Political Science Review* 96:3, 481-93.
- Capoccia, Giovanni and Daniel Keleman. 2007. “The Study of Critical Junctures: Theory, Narrative and Counterfactuals in Historical Institutionalism.” *World Politics* 59:3, 341-69.
- Collier, Ruth Berins and David Collier. 1991. *Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America*. Princeton: Princeton University Press.
- Falleti, Tulia G. 2005. “A Sequential Theory of Decentralization: Latin American Cases in Comparative Perspective.” *American Political Science Review* 99:3, 327-46.
- Falleti, Tulia G. and Julia Lynch. 2007. “Context and Causal Heterogeneity in Historical Analysis.” Northwestern University Comparative Historical Social Science (CHSS) Working Paper Series No. 3/07.
- Hacker, Jacob. 2002. *The Divided Welfare State: The Battle Over Public and Private Social Benefits in the United States*. New York: Cambridge University Press.
- Hacker, Jacob. 2004. “Privatizing Risk Without Privatizing the Welfare State: The Hidden Politics of Social Policy Retrenchment in the United States.” *American Political Science Review* 98:2, 243-60.
- Hacker, Jacob. 2005. “Policy Drift: The Hidden Politics of US Wel-

- fare State Retrenchment.” in *Beyond Continuity. Institutional Change in Advanced Political Economies*. Wolfgang Streeck and Kathleen Thelen, eds. New York: Oxford University Press.
- Lieberman, Evan. 2001. “Causal Inference in Historical Institutional Analysis: A Specification of Periodization Strategies.” *Comparative Political Studies* 34:9, 1011-35.
- Lipset, Seymour Martin and Stein Rokkan. 1967. “Cleavage Structures, Party Systems and Voter Alignments: An Introduction.” in *Party Systems and Voter Alignment*. Seymour M. Lipset and Stein Rokkan, eds. New York: The Free Press.
- Lynch, Julia. 2006. *Age in the Welfare State: The Origins of Social Spending on Pensioners, Workers and Children*. New York: Cambridge University Press.
- Mahoney, James. 2000. “Path Dependence in Historical Sociology.” *Theory and Society* 29:4, 507-48.
- Moore, Barrington, Jr. 1966. *Social Origins of Dictatorship and Democracy. Lord and Peasant in the Making of the Modern World*. Boston: Beacon Press.
- Orren, Karen and Stephen Skowronek. 1994. “Beyond the Iconography of Order: Notes for a ‘New Institutionalism.’” in *The Dynamics of American Politics: Approaches and Interpretations*. Lawrence Dodd and Calvin Jillson, eds. Boulder, CO: Westview.
- Pawson, Ray. 2000. “Middle-Range Realism.” *Archives Europeenes de Sociologie* XLI: 2, 283-325.
- Pierson, Paul. 2004. *Politics in Time: History, Institutions, and Social Analysis*. Princeton: Princeton University Press.
- Steinmo, Sven, Kathleen Thelen, and Frank Longstreth, eds. 1992. *Structuring Politics: Historical Institutionalism in Comparative Analysis*. New York: Cambridge University Press.
- Streeck, Wolfgang and Kathleen Thelen, eds. 2005. *Beyond Continuity: Institutional Change in Advanced Political Economies*. New York: Oxford University Press.
- Tilly, Charles. 2001. “Mechanisms in Political Processes.” *Annual Review of Political Science* 4, 21-41.
- Wallerstein, Immanuel. 1974. *The Modern World-System Vol 1*. New York: Academic Books.

Critical Antecedents and Informative Regress

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One of the first and worst inferential pitfalls political scientists are enjoined to avoid is “infinite regress.” Every cause has a cause in its own right. To begin inquiring into the causes of causes is to step upon a slippery slope, which threatens to hurtle political scientists all the way back in time to Marc Antony’s fateful distraction from battle by Cleopatra’s exquisitely sculpted nose.

Infinite regress is clearly a logical problem in causal inference. But is it really a substantive problem in historically oriented political science? Gary King et. al. reasonably caution that excessive attention to chains of causation “quickly leads

to infinite regress”¹; yet they provide no tangible example of research that actually commits this error. We argue that the real peril in historically oriented political science is not infinite regress, but unsystematic regress. When conducted systematically, historical regress can prove informative rather than infinite.

This essay seeks to introduce a new framework for historical analysis that can more systematically uncover the deepest causal factors in political life, yet without committing the sin of infinite regress.² It builds upon the “critical juncture” framework that currently dominates historical-institutionalist research, but also improves upon it by specifying the precise causal or non-causal status of the “antecedent conditions” preceding critical junctures. We show how antecedent factors can combine with causal factors during critical junctures to produce long-term outcomes of interest. We call these *critical antecedents*. By systematically incorporating critical antecedents into our historical arguments, we can clarify causal claims and reveal hidden causal factors (i.e., omitted variables) in political research.

Temporal Truncation: Are Critical Junctures Enough?

No historical argument goes back forever, so political scientists inevitably have to choose where to truncate their temporal chain. Paul Pierson suggests three very sensible options for how we might do so. Scholars can break the causal chain (1) “at ‘critical junctures’ that mark a point at which their cases begin to diverge in significant ways”; (2) “at the point where causal connections become difficult to pin down”; or (3) “on the basis of the theoretical interests of the analyst.”³

Pierson views the third approach as the “most instructive.” But it is not exactly the most systematic. Couldn’t a scholar always justify one’s truncation point by invoking “theoretical interests” in a post hoc manner? In our view, constricting one’s historical purview means excluding potential causal variables. Hence we see invoking theoretical interests when truncating one’s historical analysis as akin to neglecting to include socioeconomic status as a control variable in a multivariate regression due to a lack of “theoretical interest” in this factor. Pierson’s option 2 is the most convenient, but hardly the most ambitious. Aren’t historically oriented political scientists precisely in the business of seeking out “causal connections” that are “difficult to pin down”?

In our view, current best practice in historical methods is Pierson’s Option 1: to truncate one’s analysis at a critical juncture. Echoing Pierson, we define critical junctures as periods in history when the presence or absence of a specified causal force pushes multiple cases onto divergent long-term pathways, or pushes a single case onto a new political trajectory that diverges significantly from the old.⁴

But to truncate one’s causal analysis at a critical juncture is to imply that everything preceding that juncture must be causally unimportant. We submit that this is often—perhaps even usually—untrue. In fact, many leading works *do* pay considerable attention to “antecedent conditions” preceding a critical juncture. This would seem to imply that these antecedents deserve some sort of causal status. But what sort of

causation is it? And how might we make it more explicit?

Four Types of Antecedent Conditions: Why Not All Antecedents are Critical

Historical conditions preceding a critical juncture are currently either ignored as irrelevant, or lumped by default into the capacious category of “antecedent conditions.” Since this concept conveys no information whatsoever about causation, scholars inevitably struggle to specify in a systematic and transparent way whether such antecedents are causally significant, and if so, how.

Causal inference demands at a bare minimum that we differentiate causal from non-causal antecedents. We see four logical possibilities. First, antecedent conditions may have nothing whatsoever to do with a causal process. Such *descriptive context* may be of great interest to those who study history per se; but for those who seek to uncover historical causation, attention to descriptive context sacrifices parsimony without any gain in explanatory leverage.

A second possibility is that factors preceding a critical juncture may be directly responsible for the outcome of interest. Antecedent conditions should always be entertained as *rival hypotheses*, especially if we have theoretical priors that they might be causally significant. If the rival hypothesis outperforms one’s own, the existing causal argument is disconfirmed, and the presumed “critical juncture” is shown not to be critical after all.

In a third scenario, antecedent conditions represent *background similarities*. Comparative scholars often spend considerable time exploring and explicating these antecedents to justify a paired comparison research design. These ultimately serve as control variables, not causal variables.⁵ Antecedent conditions that establish cross-case similarities cannot logically be responsible for cross-case divergence.

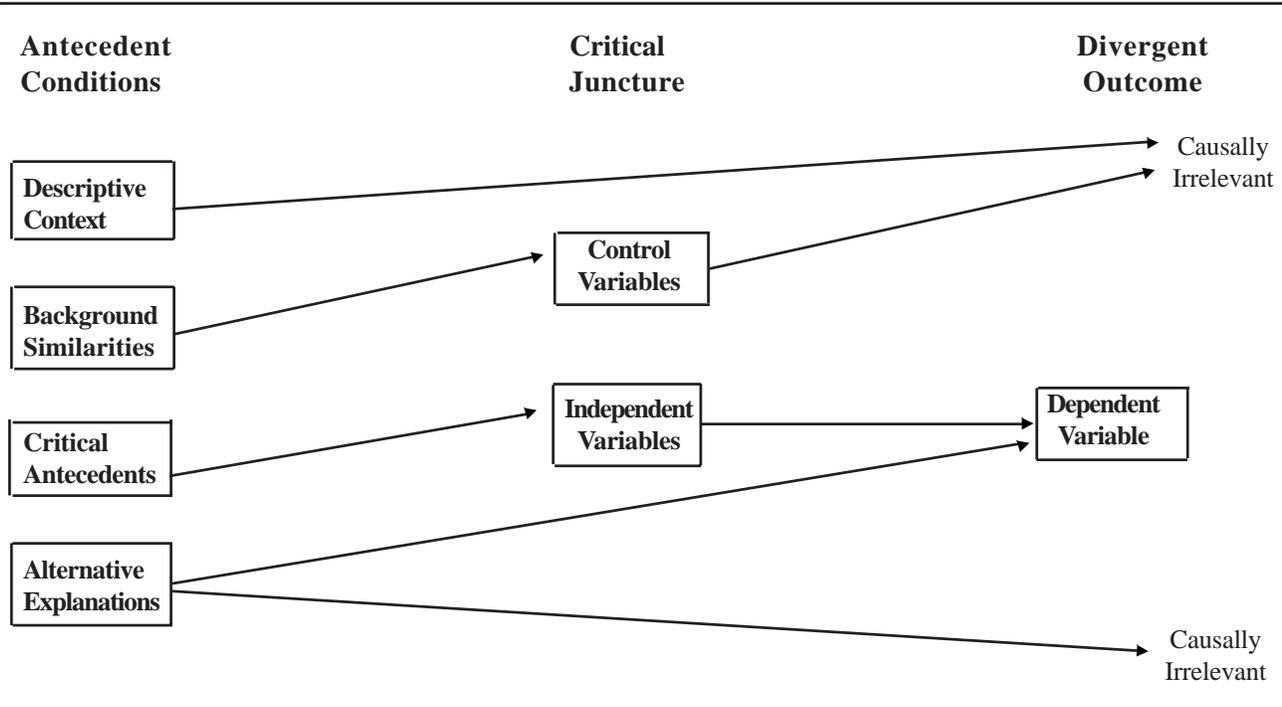
This hints at a fourth possibility, which animates this essay. Unlike descriptive context, *critical antecedents* help cause the outcome of interest. Unlike rival hypotheses, they have a causal effect that is indirect and combinatory. And unlike background similarities, critical antecedents always entail antecedent variation and/or divergence: either across cases in a cross-case analysis, or across time in a single case.

Critical antecedents can thus be defined as *factors preceding a critical juncture that combine with causal factors during a critical juncture to produce long-term divergence in outcomes*. For an antecedent to qualify as critical, it must be illogical or highly improbable to imagine the ultimate divergence in outcomes without it. Since the critical antecedent produces the outcome of interest in combination with the independent variable(s) operative at the critical juncture, it complements a critical juncture argument. It does not contradict it.

Two Logics of Antecedent Causation: Successive and Conditioning Causes

How precisely do critical antecedents combine with causal factors operative at a critical juncture? We suggest that this combinatory causation can occur in two distinct ways. In one

Figure 1: Four Types of Antecedent Conditions



scenario, critical antecedents are *successive* causes: they exhibit a direct causal effect on the independent variable, which emerges during the critical juncture to produce long-term political divergence. Such “causes of causes” are what methodologists typically have in mind when they warn about infinite regress, and we do not deny that treating successive causes as critical antecedents flirts with that inferential risk.

Yet the risk is not as great and our tools for distinguishing critical from non-critical successive causes are not as arbitrary as commonly assumed. In fact, John Stuart Mill offered a useful rule of thumb nearly a century and a half ago that neither King et. al. nor Pierson mention in their discussions of infinite regress. For Mill, the sensible place to truncate a causal analysis is at the point when causes can be “understood without being expressed” (cited in Rigby 1995: 236). So long as scholars limit their attentions to non-obvious causes, this should suffice to avoid infinite regress. Furthermore, the kind of successive causes that logically lead to infinite regress are usually background similarities, not critical antecedents.

Not all critical antecedents are “causes of causes,” however. Even more important in our view are *conditioning* causes, which are unrelated to infinite regress and which present greater dangers of omitted variable bias if ignored. These are conditions that vary before a critical juncture, and that predispose cases to diverge as they ultimately do. This kind of critical antecedent does not *cause* the independent variable to emerge; rather it helps to determine the *value* of the independent variable when the critical juncture eventually comes about.

How might critical antecedents and critical junctures combine in a causal manner? S.H. Rigby captures this logical possibility with the analogy of a glass bottle being shattered by a

stone. One could explain this outcome by saying that the bottle broke because the stone struck it, or because the bottle was brittle. While the first explanation stresses a proximate factor (or a critical juncture), the latter emphasizes an underlying condition making breakage likely if not predetermined (or a critical antecedent). In Rigby’s view—and in ours—there is no philosophical justification for claiming that one type of causal factor is more important than another. “In reality,” Rigby argues, “both of these conditions (the brittleness of the bottle and a stone being thrown at it) were indispensable if the outcome we are seeking to explain (the bottle breaking) was to be brought about” (1995: 235).

Especially in comparative politics,⁶ crafting this kind of combinatory causal argument is both more tractable and more essential than in the example Rigby offers. It is more tractable because comparativists wish to explain divergence in outcomes, not just a single outcome. Background similarities—e.g., the similar brittleness of all bottles—can be safely set aside when building a causal argument for cross-case divergence. If two bottles are similarly brittle and only one is struck by a stone, it is perfectly fair to say that the stone was *the* cause of divergence in breakage outcomes. To begin delving into background similarities (i.e., control variables) such as the brittleness of bottles is to take a first step into the pitfall of infinite regress.

Yet societies are not as alike as bottles; not all background *conditions* are background *similarities*. When underlying conditions vary across cases, it becomes essential to examine whether this pre-existing variation might have predisposed cases to diverge as they did after the critical juncture. In fact, anyone constructing a critical-juncture argument should already be examining antecedent conditions to see if

enough background similarities exist to construct a controlled comparison, and to determine whether rival explanations grounded in antecedent conditions might outperform one's own. Historical-institutionalists thus already look before critical junctures in their historical *analyses*; what we wish to provide is a systematic and transparent way for them to bring the antecedent information they uncover into their causal-historical *arguments*.

**How Causation Can be Antecedent:
One Successive and One Conditioning Example**

Critical antecedents are currently a silence as much as an absence in political science. In this section we illustrate how causal factors can indeed precede a critical juncture by discussing two scholars who—like Moliere's Monsieur Jourdain unknowingly speaking prose his entire life—use critical antecedents in their arguments without naming them as such. Given the causal significance of factors and conditions preceding critical junctures in major works by Doug McAdam (1982) and Anna Grzymala-Busse (2002), it should be evident that political scientists require a concept that systematically captures and conveys these causal forces.

McAdam's *Political Process and the Development of Black Insurgency, 1930-1970* is particularly noteworthy for its emphasis on the causal importance of a critical antecedent. Like other scholars, McAdam sees the Brown v. Board case in 1954 and the Montgomery Bus Boycotts in 1955-56 as "watershed" moments in the emergence of the American civil rights movement (McAdam 1982: 3). He famously goes beyond existing work in specifying the causal factors at work that these events helped unleash during this critical juncture. Offering what would become an influential new model for social move-

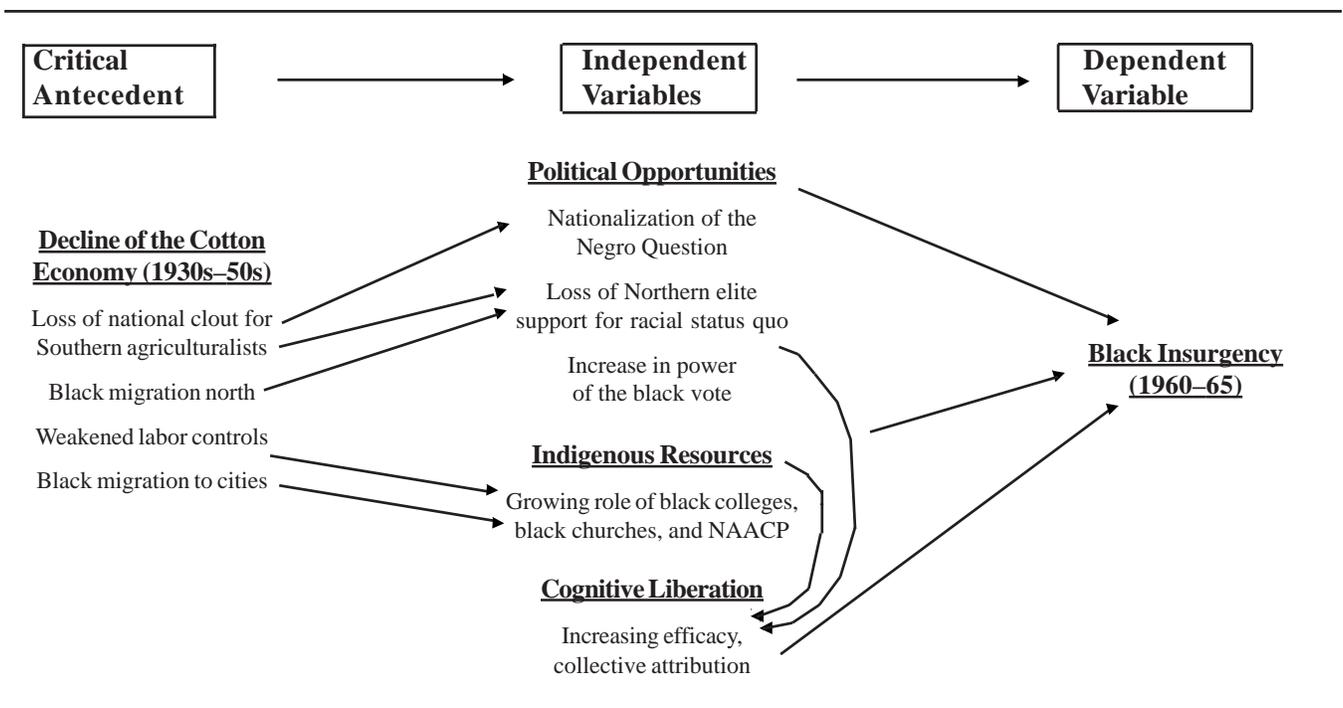
ment analysis, McAdam looks to political opportunities, organizational resources, and processes of cognitive liberation to explain the emergence of black insurgency.

Yet McAdam does not allow himself to be confined to the critical juncture of when these three causal factors emerged and combined. He insists that one cannot adequately comprehend why black politics changed paths during this critical period without understanding the deeper, antecedent cause of his independent variables: the long-term decline of the cotton economy in the American South.

Cotton's decline had three indirect, combinatory causal effects on the rise of the civil rights movement. First, it facilitated a collapse in North-South elite alliances, lowered the risks of organizing, and drove blacks north where they garnered increased political attention. These factors combined to restructure political alignments in ways that were more favorable to blacks. Second, the move off of agricultural land and into cities increased organizing capacity among blacks, leading to the creation and/or strengthening of black churches, colleges, and the NAACP. Finally, the increased political leverage created by the northern migration, in combination with the economically driven erosion of consensus among Northern and Southern elites, "triggered a growing sense of political efficacy among certain segments of the black community" (ibid: 110).

Socioeconomic change had a direct, causal bearing on the political opportunities, organizational resources, and processes of cognitive liberation that McAdam argues explain the development of the civil rights movement. Thus, we interpret the decline of the cotton economy to be a *successive* cause—the collapse of "King Cotton" caused political opportunities to change, resources to be developed and cognitive

Figure 2: McAdam on King Cotton and Black Insurgency



shifts to take place. These three factors are very much the heart of McAdam’s causal argument—so much so, in fact, that they have become the basis for “an integrated perspective on social movements and revolutions” (McAdam, Tarrow, and Tilly 1997).

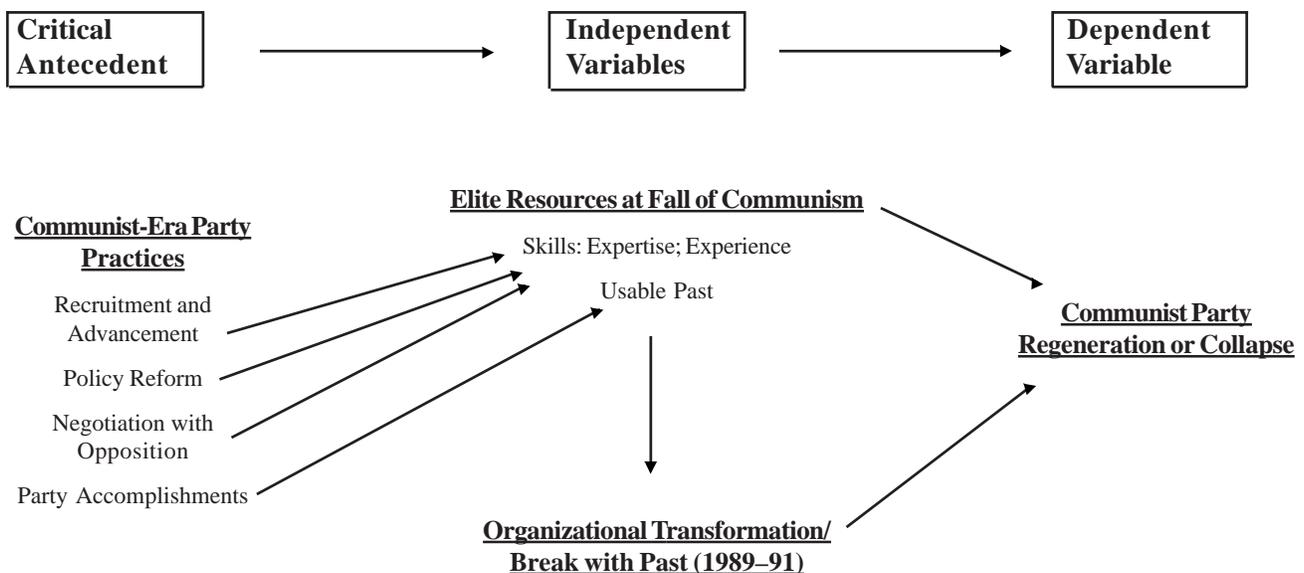
It should thus be evident that cotton’s decline is an additive rather than an alternative explanation for black insurgency. Far from detracting from the causal importance of political opportunities, mobilizing resources, and cognitive liberation in social movement emergence, cotton’s long-term decline is the key that opens that explanatory door. Anyone who reads McAdam recognizes how much explanatory weight he places upon this critical antecedent. The collapse of cotton may be a “cause of a cause,” but McAdam’s decision to detail it looks like informative rather than infinite regress.

In a very different part of the world and on a very different topic, Anna Grzymala-Busse posits a subtly different type of critical antecedent: what we call a *conditioning* rather than a successive cause. Grzymala-Busse’s analysis is animated by the puzzling regeneration of some but not all communist parties in East Central Europe. To explain this divergence, Grzymala-Busse looks to conditions existing prior to her critical juncture. Variation in the post-1989 success of the Czech, Slovak, Polish, and Hungarian communist parties is, she argues, best explained by variation in the resources possessed and strategies pursued by party elites. Elite resources determined the degree to which party leaders were able to implement the decisive and rapid organizational transformation necessary for resurgence.

The window for party transformation, from 1989-91, serves as a critical juncture. If parties missed this historic opening, they would not have a second chance to change their stripes. Yet Grzymala-Busse looks *before* the critical juncture to explain subsequent party trajectories. At the moment of communism’s collapse, each party had a different menu of choices available to it. The elite skills and “usable pasts” critical to transformation “did not emerge during the transition or in the months preceding it, but in decades-long practices of the communist parties.”⁷ In Poland and Hungary but not in Czechoslovakia, longstanding practices of recruitment and advancement, policy reform, and negotiation with oppositionists “gave rise to a cohort of skilled and experienced politicians...with the ability to reinvent the communist parties when the regime collapsed.”⁸

Critical antecedents work in tandem with the independent variables in Grzymala-Busse’s analysis. We cannot explain long-term divergence in post-communist party regeneration without understanding the elite transformation strategies of the 1989-91 critical juncture. Yet neither can we explain the cross-case variation in those initial strategies themselves, nor these parties’ ongoing capacity to gain public support, without recognizing the causal significance of pre-existing variation in their usable pasts and inherited resources and skills. In sum, preexisting elite resources serve as a *conditioning* cause. They influenced the divergent directions that party transformation would likely take, but did not directly cause the transformation in the sense that cotton’s decline directly caused political opportunities and other causal

Figure 3: Grzymala-Busse on Communist Party Regeneration



factors to shift in McAdam’s work.

McAdam and Grzymala-Busse thus provide examples of antecedent causation that are easy to see, yet difficult to define. Introducing the concept of critical antecedents makes the successive and conditioning causes witnessed in these analyses more transparent. In other instances, it is more difficult to determine whether antecedent conditions are causal at all. Even some of our very best historical scholarship in political science can struggle to clarify the causal status of “antecedent conditions” when using a critical-juncture framework that fails to differentiate causal from non-causal antecedents.

**How Antecedents Can be Causal:
Collier and Collier’s *Shaping the Political Arena***

Some scholars are admirably attentive to antecedent conditions. Yet all scholars lack a concept conveying whether antecedent conditions are causally relevant. This can make it devilishly tricky to determine whether some, all, or none of these antecedents should be considered causal. Ruth Berins Collier and David Collier’s *Shaping the Political Arena* offers the most important example of a work that wrestles mightily and explicitly with antecedent conditions, yet struggles to convey and clarify their causal or non-causal status.

Even the Colliers’ most sympathetic readers actively disagree on how far back in time their causal argument begins. In one chapter of James Mahoney and Dietrich Rueschemeyer’s (2003) edited volume, for instance, Pierson suggests that the Colliers are consummate examples of authors who “choose to break the chain at ‘critical junctures’ that mark a point at which their cases begin to diverge in significant ways” (2003: 188).

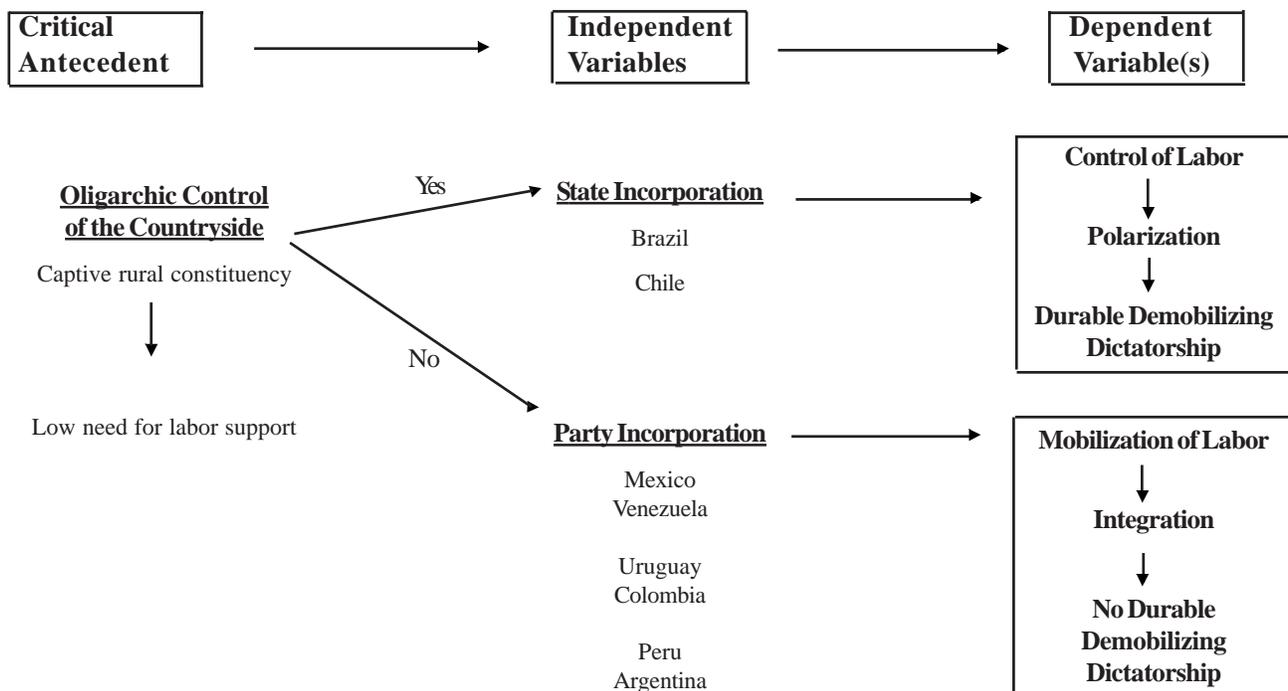
Yet in a separate chapter, Mahoney looks *before* the Colliers’ critical juncture to identify “their main explanatory variable—the political strength of the oligarchy” (2003: 359).

The irony is that these interpretations are at once contradictory *and* correct—but only if one recognizes the status of critical antecedents as combinatory causes in critical-juncture arguments. Divergent patterns of labor incorporation indeed emerged during the critical juncture in the Colliers’ argument, and this is properly seen as the main independent variable in their effort to explain long-term regime outcomes (as Pierson suggests). Yet the key contrast between state-led and party-led labor incorporation was causally and systematically influenced by a critical antecedent: pre-existing variation in the political power of the national oligarchy in the countryside (as per Mahoney).

Figure 4 details the Colliers’ causal chain as we interpret it. At the heart of their argument rests an assertion that party and regime dynamics in Brazil and Chile diverged from the Colliers’ other six Latin American cases when the state took the lead in incorporating labor into politics during the early 20th century. By contrast, the Colliers identify multiple varieties of party incorporation of labor in Mexico, Venezuela, Uruguay, Colombia, Peru, and Argentina.⁹ State incorporation in Brazil and Chile commenced a causal chain in which class politics became highly polarized and durable dictatorships would eventually emerge to demobilize popular sectors. The six cases of party incorporation witnessed a more integrative than polarizing dynamic, helping to forestall the emergence of durable demobilizing dictatorships in the postwar period.

If the labor incorporation period is the critical juncture,

Figure 4: Collier and Collier on Labor Incorporation in Latin America



as the Colliers repeatedly assert, why do they devote over 100 pages to exploring antecedent conditions? Is it because of their “theoretical interests,” or their descriptive commitments in Latin American studies, or because they did not fully appreciate the dangers of infinite regress? Or might they be exhaustively establishing background similarities and eliminating rival explanations?

None of these recognized reasons for going beyond a critical juncture captures what the Colliers are doing. Instead, they are exploring antecedent conditions to establish the deepest historical cause of divergence in Latin American regimes; but they lack the terminology to convey this causal relevance and impact clearly. The vital point is that the Colliers’ comparative exploration of Latin America’s antecedent era of the “oligarchic state” reveals not only cross-case commonalities, but also “differences so profound that some discussion of contrasts among cases is possible” (1991: 104).

The contrasts at issue here are the pre-existing differences in the political strength of the oligarchy. These different conditions are “important” in a causal sense because variation in oligarchic power *before* the critical juncture systematically and causally influenced the directions that labor incorporation took *during* the critical juncture. Oligarchic power is neither a background similarity nor a rival explanation supplanting the Colliers’ labor-led causal argument; it is a critical antecedent.

Labor incorporation options were causally conditioned by the pre-existing political position of the oligarchy. Where oligarchs enjoyed unmediated control over a captive peasantry, they had no incentive to mobilize urban labor for electoral support. In Brazil and Chile, clientelistic ties in the countryside meant leaders could focus on controlling rather than mobilizing workers. State incorporation of labor was the result. In the Colliers’ other six cases, where elites could not rely on rural political hegemony for electoral domination, they mobilized urban workers, and party-led incorporation trajectories were launched.

Variation in pre-existing patterns of oligarchic power meant that Brazil and Chile were very different *places* from their Latin American neighbors before labor incorporation would set them on such different *paths*. They *varied* before they *diverged*. Antecedent variation in oligarchic power in the countryside thus proves critical for understanding the nominal variation between state and party incorporation—and hence the long-term divergence in regime outcomes—in the Colliers’ eight Latin American cases.

Conclusion: Critical Antecedents in Knowledge Accumulation

Our intervention should be seen entirely in a constructive, not a condemning, light. We do not need to repair the critical-juncture framework because of any shortcomings in *these* scholars, but because *all* scholars lack a concept that conveys the causal importance of some but not all historical factors preceding a critical juncture. Our concerns are emphatically systemic rather than individual.

The biggest benefits for political science from incorpor-

ating critical antecedents are systemic, as well. Careful readers might pinpoint the causal significance of variation preceding a critical juncture in individual works, and seek to test the importance of such critical antecedents on new data. But until we have a concept explicitly signaling the causal significance of critical antecedents, knowledge accumulation within research agendas will be hindered.¹⁰

More substantively, inattention or unsystematic attention to critical antecedents will not merely cause us to miss descriptively interesting details. It will cause us to de-emphasize or even ignore the deep historical roots of contemporary political life. For historically oriented political scientists, uncovering such causal factors is not an alluring, descriptive distraction. It is our most important explanatory work.

Notes

¹ King, Keohane, and Verba (1994: 86).

² The article-length version of this manuscript, which surveys a much wider range of comparative work and enunciates further substantive benefits from bringing critical antecedents into comparative research, is currently under review. Please contact the first author at slater@uchicago.edu to request the most recent iteration.

³ Pierson (2004: 89).

⁴ By defining critical junctures as *divergence* points rather than *choice* points, we remain agnostic as to whether these are moments of unusual agency or of tightly constraining structures. For a similarly agnostic definition, see Brady and Collier (2004: 282).

⁵ Control variables can of course also be causal variables in a multivariate regression, but they are (or should be) non-causal by definition in a controlled-comparison research design.

⁶ Critical antecedents may ultimately prove less vital for the kind of single-outcome qualitative studies often found in American Politics and International Relations. But we do provide one example from the former subfield in the next section (McAdam, 1982).

⁷ Grzymala-Busse (2002: 5).

⁸ *Ibid.*, 5-6.

⁹ Critical antecedents do not help explain ordinal variation among the six cases of party incorporation, at least as far as we can tell. Thus we do not discuss this important range of long-term variation here.

¹⁰ We explore the potential benefits for knowledge accumulation in the article-length version of this manuscript, offering analytic reviews of leading literature on democratization in Central America, ethnic violence in India, and race and nation-building in Brazil and South Africa.

References

- Brady, Henry E. and David Collier, eds. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, MD: Rowman & Littlefield.
- Collier, Ruth Berins and David Collier. 1991. *Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America*. Princeton: Princeton University Press.
- Grzymala-Busse, Anna M. 2002. *Redeeming the Communist Past*. New York: Cambridge University Press.
- King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton: Princeton University Press.
- Mahoney, James. 2003. “Strategies of Causal Assessment in Comparative Historical Analysis.” in *Comparative Historical Analysis in the Social Sciences*. James Mahoney and Dietrich

Reuschmeyer, eds. New York: Cambridge University Press.
Mahoney, James and Dietrich Reuschmeyer, eds. 2003. *Comparative Historical Analysis in the Social Sciences*. New York: Cambridge University Press.
McAdam, Doug. 1982. *Political Process and the Development of Black Insurgency, 1930-1970*. Chicago: University of Chicago Press.
McAdam, Doug, Sidney Tarrow, and Charles Tilly. 1997. "Toward

an Integrated Perspective on Social Movements and Revolution." in *Comparative Politics: Rationality, Culture, and Structure*. Mark I. Lichbach and Alan S. Zuckerman, eds. Cambridge: Cambridge University Press.
Pierson, Paul. 2004. *Politics in Time*. Princeton: Princeton University Press.
Rigby, Steve H. 1995. "Historical Causation: Is One Thing More Important Than Another?" *History* 80:259 (June), 227-42.

Symposium: Bridging the Gap? Connecting Qualitative and Quantitative Methods in the Study of Civil War

Introduction

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Civil wars—and the violence, death, destruction and dislocation they cause—have become a central concern in the post-Cold War and post-9/11 worlds, even as we witness a more general decline in armed conflicts and related fatalities (Human Security Report Project 2006). Indeed, a new generation of scholarship has placed the study of such wars squarely in the academic mainstream, generating an array of findings on all phases of civil conflict (Tarrow 2007).

For readers of this newsletter, the particular way in which this knowledge has been generated will be of interest, as well. With a few important exceptions—Petersen (2002), Kalyvas (2006)—these findings flow from the rigorous application of quantitative methods, based on datasets of increasing power and sophistication (Gleditsch 2007; Humphreys and Weinstein 2007, among many others). Scholarship of this type is an outstanding example of the best that quantitative methodologies can offer—large, cross-national, as well as sub-national, disaggregated samples that yield “new insights into the causes of civil war, the forces impacting its duration, and the factors shaping its termination” (Weinstein 2007: 366).

This symposium does not dispute such claims, but instead asks how we can improve or build upon them. In particular, how can the application of mixed methods—quantitative and qualitative—consolidate and push forward the civil war research program? The contributions presented below suggest advances in three related areas: (1) a move from Humean or correlational accounts of cause to more robust mechanism-based understandings; (2) greater understanding of dynamics and processes; and (3) an argument to put quantitative and qualitative techniques on a more equal footing.

Nome's contribution nicely illustrates how mixed methods can move work on civil war from correlation to causation. As he notes, causal mechanisms are not simply intervening variables or variables at lower levels of aggregation, but recurrent processes connecting specified initial conditions and outcomes. Nome then goes on to argue that Gerring and Sea-

wright's (2007) notion of a pathway case study is especially well suited to uncover causal mechanisms. In his research, these mechanisms connect third-party military interventions in ethnic civil wars with transnational ethnic affinities. Selecting the best pathway case, however, still requires the careful, prior application of several quantitative techniques.

The essays by Arjona and Steele indicate that qualitative methods are an essential tool for grasping the complexity and dynamics of the relations between rebel groups and civilian populations. In both cases, they begin by applying quantitative methods to novel, sub-national datasets on the Colombian civil war; statistical analysis shows whether patterns of civilian displacement (Steele) or of local order (Arjona) match theoretical expectations. The comparative case method is then used to establish causality at the micro-level of individuals and groups. What causal processes best explain displacement or the creation of local order? However, theirs is not a simple story of quantitative first and qualitative second. Rather, both Arjona and Steele show that qualitative methods are essential not only for uncovering causal mechanisms, but also to explore the validity of the assumptions upon which their quantitative data collection is based.

Finally, all contributors argue for a more equal treatment of qualitative methods. As Jung notes in his essay, mixed methods applications in research on civil war have too often meant that qualitative techniques play an auxiliary function, where case studies are employed to detect limitations in statistical models or to improve the original models with new variables taken from the cases. If we are to accord the study of causal mechanisms a more central role, then qualitative techniques need to shed this auxiliary status (see also Lieberman 2005: 436).

In sum, contributors address *and* give operational content to Sambanis' (2004) call for integrating statistical analyses and qualitative methods in work on civil war. They are indeed “bridging the gap” between qualitative (Kaufman 2001) and quantitative studies (Fearon and Laitin 2003). More importantly, they demonstrate that the bridging exercise has real value added, giving students of civil war more robust measures of causality and a better understanding of micro-dynamics, which are embedded in and indeed flow from large-N studies.

At the same time, the increasing application of mixed methods, both here and elsewhere (Symposium 2007), suggests new directions for methodological and theoretical research and

reflection. First—and echoing concerns raised by Gates in his contribution—methods are no substitute for theory. Put bluntly, what kind of theory results when we mix methods? Reading across the contributions—with their invocation of contingency, complex causal chains, scope conditions and mechanisms—one gets a sense that these dissertations will not produce broad, generalizable theories. Instead, we would appear to get middle-range theory (Johnston 2005), or what sociologists call grounded theory. If this is so, users of mixed methods might do well to connect their efforts to an earlier generation of middle-range theory building (Glaser and Strauss 1967) so as not to repeat past mistakes. More generally, is the civil war research program best served by a collection of complex theories, where the parts may not add up to a (unified) whole? Is there a trade-off to be considered: mixed methods → mechanisms and dynamics ≠ coherent, general theories of civil war?

Second, are there epistemological limits to multi-method work? Multi-method designs typically utilize some combination of formal models, quantitative techniques, and qualitative methods such as process tracing or case studies—all of which have an epistemological basis in positivism (Symposium 2007: 9-11). While qualitative methods could in principle include post-positivist techniques such as ethnography, discourse analysis or genealogy, these are rarely employed by practitioners of multi-method research (Symposium 2006). Are they irrelevant for understanding the complexity of civil war? Recent work would seem to suggest otherwise (Wood 2003; Hansen 2006). Perhaps, though, the problem is more fundamental: Many would caution that we simply cannot combine methods from differing epistemological traditions. Yet, in practice, several scholars have engaged in precisely such epistemological boundary crossing, and with significant empirical and methodological payoffs (Hopf 2002; Klotz 2008). Moreover, newer work in the philosophy of social science indicates that these epistemological dilemmas are not nearly as great as some once feared (Katzenstein and Sil 2008).

Third, mixing methods is but a means to an end, and that end is more complete theoretical accounts of civil war. However, multi-method work in this area seems to build theory whose roots are overwhelmingly anchored in rationalist social theory. Indeed, the rational choice language of constraints and incentives looms large in the contributions by Arjona, Jung, and Steele in this symposium; this is much in keeping with the political economy origins of much contemporary work on civil war. Yet students of mixed methods might also explore how their methodological toolkits can help bridge another gap—that between rational choice and its constructivist/sociological competitors.

Theoretical bridge building of this sort has become a cottage industry in recent years (Fearon and Wendt 2002). What role do social norms, feelings of community, and emotion, say, play in civil war? While we have hints of how to address such questions (Kaufman 2006; Kalyvas n.d.), much work remains. Multi-method research, with its problem-driven, pragmatic focus, seems well placed to bridge this other—theoretical—gap. This double bridging exercise of methods and social theory would not only generate richer theoretical accounts of civil

war. By incorporating instrumental and non-instrumental causal mechanisms, it would also contribute to central disciplinary debates.

References

- Fearon, James and David Laitin. 2003. "Ethnicity, Insurgency and Civil War." *American Political Science Review* 97:1, 75-90.
- Fearon, James and Alexander Wendt. 2002. "Rationalism v. Constructivism: A Skeptical View." in *Handbook of International Relations*. Walter Carlsnaes, Thomas Risse, Beth Simmons, eds. London: Sage Publications.
- Gerring, John and Jason Seawright. 2007. "Techniques for Choosing Cases." in *Case Study Research: Principles and Practices*. New York: Cambridge University Press.
- Glaser, Barney G. and Anselm L. Strauss. 1967. *The Discovery of Grounded Theory: Strategies for Qualitative Research*. New York: Aldine de Gruyter.
- Gleditsch, Kristian Skrede. 2007. "Transnational Dimensions of Civil War." *Journal of Peace Research* 44:3, 293-309.
- Hansen, Lene. 2006. *Security as Practice: Discourse Analysis and the Bosnian War*. London: Routledge.
- Hopf, Ted. 2002. *Social Construction of International Politics: Identities and Foreign Policies, Moscow, 1955 and 1999*. Ithaca: Cornell University Press.
- Human Security Report Project. 2006. *Human Security Brief 2006*. Vancouver, BC: Human Security Centre, University of British Columbia.
- Humphreys, Macartan and Jeremy Weinstein. 2007. "Demobilization and Reintegration." *Journal of Conflict Resolution* 51:4, 531-67.
- Johnston, Alastair Iain. 2005. "Conclusions and Extensions: Toward Mid-Range Theorizing and Beyond Europe." *International Organization* 59:4, 1013-44.
- Kalyvas, Stathis. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.
- Kalyvas, Stathis. n.d. "Ethnic Defection in Civil War." *Comparative Political Studies* (forthcoming).
- Katzenstein, Peter and Rudra Sil. 2008. "Eclectic Theorizing in the Study and Practice of International Relations." in *The Oxford Handbook of International Relations*. Christian Reus-Smit and Duncan Snidal, eds. New York: Oxford University Press.
- Kaufman, Stuart. 2001. *Modern Hatreds: The Symbolic Politics of Ethnic War*. Ithaca: Cornell University Press.
- Kaufman, Stuart. 2006. "Symbolic Politics or Rational Choice? Testing Theories of Extreme Ethnic Violence." *International Security* 30:4, 45-86.
- Klotz, Audie and Deepa Prakash, eds. 2008. *Qualitative Methods in International Relations: A Pluralist Guide*. New York: Palgrave.
- Lieberman, Evan. 2005. "Nested Analysis as a Mixed-Method Strategy for Comparative Research." *American Political Science Review* 99:3, 435-52.
- Petersen, Roger. 2002. *Understanding Ethnic Violence: Fear, Hatred and Resentment in Twentieth-Century Europe*. Cambridge: Cambridge University Press.
- Sambanis, Nicholas. 2004. "Using Case Studies to Expand Economic Models of Civil War." *Perspectives on Politics* 2:2, 257-79.
- Symposium. 2006. "Ethnography Meets Rational Choice: David Laitin, For Example." *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods* 4:1, 2-33.
- Symposium. 2007. "Multi-Method Work, Dispatches from the Front Lines." *Qualitative Methods: Newsletter of the American Political*

- Science Association Organized Section on *Qualitative Methods* 5:1, 9-27.
- Tarrow, Sidney. 2007. "Inside Insurgencies: Politics and Violence in an Age of Civil War." *Perspectives on Politics* 5:3, 587-600.
- Weinstein, Jeremy. 2007. *Inside Rebellion: The Politics of Insurgent Violence*. New York: Cambridge University Press.
- Wood, Elisabeth Jean. 2003. *Insurgent Collective Action and Civil War in El Salvador*. Cambridge: Cambridge University Press.

Local Orders in Warring Times: Armed Groups' and Civilians' Strategies in Civil War

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The type of relations between insurgent and counter-insurgent armed groups on the one hand, and the civilians with whom they interact on the other, is subject to wide variation. At times, armed groups try to approximate the behavior of states by extracting taxes, imposing new social norms, establishing predictable and routinized systems of rule enforcement, and supplying public goods. Yet, at other times, armed groups interact with civilians only through the use of violence. There is variation not only across wars and armed groups, but also within these organizations. Civilian populations, for their part, also vary in how they respond to the presence of such groups. Some stay and collaborate, some choose to leave their hometowns and become refugees or internally displaced persons, some fight back by forming self-defense groups, and some enlist as full-time combatants. What explains a group's decision to employ a specific strategy towards civilians? How can a civilian's response to the presence of armed groups in her hometown be explained?

My dissertation sheds light on these questions by examining the ways in which both sets of actors interact in a context of irregular warfare. I start from the premise that civilians' behaviors—collaboration, displacement, recruitment—cannot be understood in isolation from the very context in which their choices are made. I argue that this context varies not only across local territories, but also through time. The strategies of armed groups cannot be understood without taking into account three facts: (1) the essential nature of civilian collaboration for the warring sides in an irregular war; (2) the advantages that armed groups gain by bringing about local order in war zones; and (3) the possibility of institutional learning, which allows these organizations to fine-tune their strategies depending on the context in which they operate.¹

In this essay, I discuss the ways in which combining quantitative and qualitative methods allows me to test the different components of my theory. In particular, I stress the importance of relying on qualitative methods not only to test the causal mechanisms that I claim to be at work, but also to explore the validity of several of the assumptions on which the argument

is built (see also Steele, this symposium). By causal mechanism, I mean the underlying rationale of the causal link between the independent and dependent variables. As Jon Elster (1998: 45) has argued, "mechanisms are frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences. They allow us to explain but not to predict." Put differently, a causal mechanism complements a general claim about causality of the form "if X then Y," by providing an answer to the question "why is Y occurring when X has taken place."²

The value of combining qualitative and quantitative methods is that the latter are usually better at showing general correlations, but rarely can offer a way to test our claims about the underlying rationale of such correlations. Qualitative methods, by contrast, allow us to illustrate the specific rationale we offer for the alleged causality—that is, the mechanism (or mechanisms) that we claim to be at work.

I start with a brief discussion of the research question and the approach I offer to theorize it. I then summarize the components of the research design, and the advantages of relying on both quantitative and qualitative methods. I conclude by arguing that further advances in the civil war research program require that we take micro-level variation more seriously. To do this, collecting more fine-grained data and giving importance to causal pathways—rather than only correlations—is a must.

Theoretical Discussion

Research Question: What explains the variation in armed groups' strategies towards civilians? Why do some local communities react differently to the strategies of armed groups? How does the interaction between civilians and combatants shape the behavior of both through time? To explore these questions, I define my dependent variables as follows. First, the behavior that the armed group adopts towards any given local community can vary along a continuum that goes from the exclusive use of violence to the creation of an encompassing system of governance. Second, local communities may provide different levels of support to the armed groups that are present in their territories. I differentiate between obedience and endorsement as well as between limited and full collaboration, and propose a typology that goes from resistance to full endorsement.

Theoretical Approach: Civil wars can be fought in very different ways. In some cases, victory relies on success in the battlefield—the so-called regular wars—as in most international conflicts. In others, the fight consists of controlling territories, with the armies rarely having direct encounters. This type of war is generally known as irregular, and is the most common type of civil war (Balcells and Kalyvas 2007). Even though the exact formula for gaining and maintaining territorial control in these wars remains disputed, most scholars and practitioners agree on the crucial role that civilian collaboration plays (Mao 1997; Guevara 1978; McColl 1969; Kalyvas 2006). I argue that in their quest for control and civilian collaboration, armed groups have strong incentives to create order in the territories where they are present, which shapes both their strategies and

the ways in which civilians react to the presence of combatants in their territories.

This view of war zones as ordered pieces of territory may be counterintuitive at first. War is supposed to entail anarchy and chaos, not order. Yet, if we think about armed groups' ability to survive and grow in the context of an irregular war, it becomes evident that anarchy is seldom a good option. Violence alone cannot bring about all the types of collaboration that the group needs, and in some cases it can trigger resistance. An armed group that aims to amass territory is better off setting up a regulatory system in controlled areas; this allows it to monitor more easily the local inhabitants as well as outsiders. In addition, setting up a system of norms that regulates civilian behavior provides the opportunity to shape local dynamics—from the economy to the structure of social organizations—in ways that benefit the group. It also allows for putting into practice some of the group's ideological goals. Finally, by influencing different aspects of local life, the group is able to attain civilian collaboration through a variety of means.

Even though armed groups benefit from establishing order in the territories where they are present, they cannot always do so in the same way. The communities that inhabit those territories vary along several dimensions, which explains why some may readily welcome an armed group that aims to change the current state of affairs, but others may fiercely resist it. While for the group the best scenario is to achieve full endorsement and create a type of local order that allows for maximum control over the territory and the population, sometimes it prefers to limit its aspirations and adopt a strategy that allows it to earn a moderate level of collaboration. Put differently, the group is better off by gaining *some* collaboration than fueling a resistance movement. Hence, it chooses its behavior towards civilians based on its expectations of how they would react to alternative strategies.

What explains the different reactions that local communities display? I argue that civilian behavior in the midst of war cannot be understood in isolation from this process of creating local order. Because such a process involves disrupting a pre-existing state of affairs, it not only makes some individual behaviors more or less costly, but also involves a comprehensive transformation of different aspects of local life. Unless we take into account how this process evolves, we can hardly identify the context in which civilians may or may not decide to provide support to the warring sides. I argue that the strategy the armed group adopts in any given territory has a different effect depending on the structure of the community that inhabits it. The causal mechanism entails not only the transformation of the alternatives available to locals, but also of the payoffs that they associate with them. Some of the processes whereby local order is created also involve the transformation of preferences and beliefs.

I model the interaction between armed groups and local communities as a game with multiple sub-game equilibria: for each type of community, there is an equilibrium strategy of the armed group and an equilibrium type of civilian collaboration. Assuming that the armed group accurately predicts civilian behavior makes sense because institutional learning often takes

place in these organizations—that is, through time, they learn what works best with different communities. Under certain circumstances, however, they may choose a strategy that does not lead to the maximum possible level of collaboration. They may even choose one that triggers credible resistance and results in a huge loss of life, as well as of territorial control. I argue that these off-equilibrium strategies are the outcome of a set of factors related to the competition with other warring sides; the strategic value of the local territory; and the military and political capacity of the group.

Research Design

I test my hypotheses using quantitative and qualitative evidence on the Colombian conflict. My research design allows me to test my theory at three levels of analysis: the armed group, the locality, and the individual. I combine statistical analysis of both original survey data and existing quantitative data; a controlled-comparison of six cases; and in-depth analysis of interviews.

The Colombian conflict is particularly well suited to my research topic for several reasons. First, in addition to the state, various armed groups compete for local control: several right-wing paramilitary groups and two leftist guerrilla organizations. This allows for comparisons across armed groups while controlling for national-level conditions. Second, there is great variation across Colombian localities in their geography, income, race, traditional political affiliations, history of pre-war violence, social capital, state presence, and type of economy. This variation allows for testing alternative hypotheses about the role that local characteristics play in armed groups' strategies and civilian collaboration, while controlling for national and regional factors. Third, the Colombian government's recent demobilization program offers a unique opportunity to conduct extensive research with ex-combatants of both guerrilla and paramilitary groups. This allows for gathering micro-level data on a variety of aspects of armed groups and their behavior towards civilians—a possibility seldom available to researchers of civil wars.

I rely on a four-step procedure to test my general argument about the strategies that armed groups adopt and the type of collaboration that they are able to elicit. I begin (step 1) by conducting a quantitative analysis of a database of localities that I am compiling, with information on the pre-war characteristics of the community, the type of behaviors that armed groups and civilians exhibited through time, and the characteristics of the local order that was established. I also include existing data that allows me to control for a variety of factors such as violence, settlement patterns, wealth, education, population diversity, and state presence. Statistical analysis then allows me to see whether armed groups choose the strategies that I hypothesize for each type of community, and whether locals respond to those strategies as the theory predicts. In addition, the data also allow for testing the effect of the factors that I expect to explain off-equilibrium strategies.

Even though this exercise is very useful as it allows me to test the argument on a large number of cases, it can only poorly illustrate the causal mechanisms that I claim to be at work in

the interactions between armed groups and civilians. Given that my argument stresses the importance of the dynamics that characterize each path to local order—and how they affect civilian behavior through time—I make claims about a set of causal links. Hence, a method that allows me to trace causal chains is essential. To do this, I rely (step 2) on a controlled comparison of two sets of cases. Each set consists of three local communities in the same region, which share the same history in terms of settlement patterns and violence, and display a similar local economy and level of state presence. There is variation, however, in the structure of the community.

By comparing the process of creating local order in each locality, and the type of civilian collaboration that emerged, I will be able to test my argument on both outcomes as well as on the mechanisms that I claim to underlie them. For example, fine-grained evidence from the cases allows me to show how commanders anticipated the reaction of different communities when planning their strategies, and the strategies they used at different stages in their attempt at creating local order. At the same time, the cases offer empirical evidence of the ways in which locals of different communities perceived the arrival of the armed groups, and why they reacted in the ways they did. The evidence also shows how different local orders function, and how their characteristics shape the choices of civilians through different causal mechanisms. In other words, this detailed information on individuals' experience of different local orders illustrates the micro-foundations of my theory.

While this controlled comparison of cases allows me to document causal mechanisms, it does not allow me to generalize. However, there are observable implications of those mechanisms at the level of the individual that can be tested on a larger number of cases. To do this, I derive implications from my theory to a particular type of collaboration: recruitment (step 3). I formulate several hypotheses on the conditions under which voluntary recruitment is more likely as well as on the motivations that underlie it. I argue that different processes of creating local order not only lead to different levels of recruitment, but also make some motivations for it more likely. I test these hypotheses by relying on a survey with ex-combatants from both guerrilla and paramilitary groups and a control group of civilians (Arjona and Kalyvas 2008); it gathers evidence on the local order in which both civilians and combatants lived, as well as on the process that led the latter to enlist.

Finally (step 4) I rely on qualitative evidence to validate my assumptions about how the logic of irregular warfare imposes a set of challenges and local goals on armed groups. I conducted several in-depth interviews with former medium-level commanders and rank fighters of different armed groups who operated outside of my case-areas. The interviews provided detailed evidence on the ways in which these organizations plan their arrival to new territories, as well as their strategies to control populations. While these interviews cannot serve as a general test, they provide additional empirical validation for my theory.

Conclusion

Research on civil war dynamics requires that we take seriously the differing contexts in which individuals make choices be they combatants, civilians, or public officials. If we are to understand these contexts, we need to pay more attention to the ways in which the war transforms local dynamics, and how it does so differently across war zones. Both the process through which these changes take place and their outcomes entail different and complex reconfigurations of social, political, and economic states of affairs. Exploring the different form that war takes across a national territory will illuminate our understanding of wartime phenomena like recruitment, civilian support to the warring sides, armed groups' ruling strategies, and violence.

While quantitative data and analysis can allow us to identify patterns and test correlations at the sub-national level of analysis, they cannot allow us to assess the causal mechanisms at work. Theorizing the micro-foundations of general causal links, and searching for evidence to explore their validity, is inherent to good explanations. The correct use of qualitative methods—both for gathering new data and analyzing them—offers the possibility of describing and making sense of these processes. It also makes the findings of the quantitative analysis more intelligible, as it moves us from the realm of correlations to that of theoretically specified and empirically validated causal claims.

Notes

¹ There are excellent accounts of the variation that armed groups display in their willingness to govern populations (Weinstein 2007; Mampilly 2007). However, the literature has so far assumed that there is no within-group variation in this respect. This fact leads to overlooking the immense variation that exists within civil wars in the ways in which armed groups and civilians interact. In addition, it leads to underestimating the role that civilians' agency plays.

² This idea of mechanisms as a key component of more complete explanations in the social sciences has been presented in a detailed and illuminating way by Elster in several works. Most recently, for example, he has argued that “[m]any social scientists try to model this relation [between explanans and explanandum] using statistical methods. Statistical explanations are incomplete by themselves, however, since they ultimately have to rely on intuitions about plausible causal mechanisms” (Elster 2007: 8). Several authors stress in a similar way the importance of causal mechanisms as defined by Elster (Achen 2002, for example).

References

- Achen, C. 2002. “Toward a New Political Methodology: Micro-foundations and ART.” *Annual Review of Political Science* 5, 423-50.
- Arjona, Ana and Stathis Kalyvas. 2008. “A Micro-level Approach to the Armed Conflict in Colombia: Results of a Survey with Demobilized Fighters of Guerrilla and Paramilitary Groups.” in *Argumentación, Negociación y Acuerdos*. Freddy Cante, ed. Bogotá, Columbia: Universidad del Rosario.
- Balcells, Laia and Stathis Kalyvas. 2007. “Warfare in Civil Wars.” Presented at the Comparative Politics Workshop. New Haven: Yale University, Department of Political Science (10 October).

- Elster, Jon. 2007. *Explaining Social Behavior: More Nuts and Bolts for the Social Sciences*. Cambridge: Cambridge University Press.
- Elster, Jon. 1998. "A Plea for Mechanisms." in *Social Mechanisms: An Analytical Approach to Social Theory*. Peter Hedström and Richard Swedberg, eds. Cambridge: Cambridge University Press.
- Guevara, Che. 1997. *Che Guevara Reader: Writings by Ernesto Che Guevara on Guerrilla Strategy, Politics and Revolution*. New York: Ocean Press.
- Kalyvas, Stathis. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.
- Mampilly, Zachariah. 2007. "Stationary Bandits: Understanding Rebel Governance." PhD Dissertation. Los Angeles: University of California, Los Angeles, Department of Political Science.
- Mao, Zedong. 1978. *On Guerrilla Warfare*. New York: Anchor Press.
- McCull, Robert. 1969. "The Insurgent State: Territorial Bases of Revolution." *Annals of the Association of American Geographers* 59:4, 613-31.
- Weinstein, Jeremy. 2007. *Inside Rebellion: The Politics of Insurgent Violence*. Cambridge: Cambridge University Press.

Getting the Balance Right: A Mixed Methods Approach to the Study of Post-Civil War Democratization

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Bridging the quantitative-qualitative divide in civil war studies is imperative if we are to improve our understanding of the dynamics of intrastate violence leading up to all-out civil wars. Nicholas Sambanis (2004) pinpointed a list of shortcomings in the quantitative literature on civil wars—measurement error, unit heterogeneity, model misspecification, and unclear specification of causal mechanisms—and called for combining statistical and case study work to address these limitations. Sambanis and Paul Collier (2005) then edited two volumes that identified the causal logic and limitations of the Collier-Hoeffler model of civil war onset and the quantitative methods on which it is based, and expanded it through 16 case studies. In a similar vein, Fearon and Laitin (2005) are conducting a "random narrative" project that essentially combines their statistical model on civil war onset with 25 randomly selected country studies.

This work indicates that multi-method approaches are being taken seriously in civil war research. Yet one commonality of their use of mixed methods is that the in-depth case studies or narratives are typically employed to detect limitations in the statistical models or to improve the original models with new insights. This is certainly one way of triangulating multiple methods to understand better the complicated process of conflict escalation across space and time—but not the only one possible. In this essay, I thus advocate another way of using mixed methods for a different research purpose—identifying and measuring a proposed mechanism.¹ Describing why and how I have combined a large-N analysis with paired case com-

parison in my ongoing research on post-civil war democratization, I call for getting the balance right between quantitative and qualitative techniques—so as to maximize the analytic advantages of each.

Post-Conflict Democratization

My interest in democracy building in post-civil war countries began with the realization that there was a striking imbalance between substantial international efforts at democracy promotion in such settings and the paucity of systematic comparative research on the issue. The quantitative literature in civil war studies has paid little attention to asking under what conditions post-civil war democracy is more likely to emerge and survive. Even in the democratization literature, the question of how democratic governance can be established in war-torn societies is "either wholly neglected or seriously under-theorized" (Bermeo 2003: 159).

Intrigued by these analytic gaps, I started my research by asking what caused the success or failure of democracy building in countries emerging from deadly internal conflicts since the end of World War II. Moreover, I was puzzled that power-sharing arrangements are widely considered the most effective institutional tool for establishing peace and democracy in post-civil war countries, even though they often work against that prospect—the case of post-2003 Iraq comes immediately to mind.

To answer these questions, I began with a theoretical insight drawn from the work of Doyle and Sambanis (2000) and Wantchekon (2004): Peaceful resolution of violent conflicts (or negotiated settlement of civil wars) and a high degree of international commitment to peacekeeping operations should be essential conditions for building a sustainable peace and democracy in post-civil war societies. However, I quickly learned—through a careful examination of the case study work—that negotiated settlement has not necessarily led to durable peace and stable democratic regimes in post-civil war countries. Moreover, strong international involvement in conflict resolution and peace building processes has often produced adverse outcomes, such as repeated peace failure in Lebanon and an extremely delayed process of establishing a functioning central government in Bosnia-Herzegovina. My research question thus became more specific and nuanced: Why does peaceful resolution of civil wars sometimes lead to establishing a stable democratic government, while at other times it does not, despite similar degrees of international commitment?

Differing Time Horizons

I argue there is a trade-off between the short-term interest in ending violence as quickly as possible and the long-term goal of post-civil war democratization, and that this trade-off revolves around the issue of power sharing. The short-term interest in making a peace by signing a peace agreement and the long-term goal of democracy promotion do not always coincide in civil war situations, but in fact often conflict, depending upon the time horizons held by key political actors involved in the transition from war to peace. My proposed mechanism in post-conflict political processes is as follows.

The short-term versus long-term inconsistency in designing post-conflict institutions arises when civil war adversaries have reached a mutually destructive stalemate and entered into peace negotiations. To resolve the stalemate, power sharing is often proposed by international mediators to provide a security guarantee and a strong incentive for warring parties to initiate negotiations, sign a bargain for peace, and implement peace settlement terms. Seemingly indivisible stakes become divisible, to some extent, by balancing the distribution of political powers among warring parties. Therefore, power sharing should contribute to negotiated settlement of civil wars through institutionally guaranteeing the security of warring groups.

However, power sharing, by virtue of its institutional nature, builds wartime cleavages into post-war political structures. It also provides a strong incentive structure under which former warring parties perpetuate those wartime cleavages into post-conflict politics. In turn, this frequently leads to deadlocks in governmental institutions and hinders the development of state capacity necessary for democracy promotion in post-civil war countries. War-induced cleavages are also likely to be deeply entrenched in post-war electoral politics, as power-sharing institutions provide a powerful incentive for former warring parties to garner political support primarily from their own constituencies. Moreover, such institutionalized wartime cleavages help maintain ordinary citizens' support for former warring parties and lower their confidence in central governmental institutions.

The result is a vicious circle—from post-conflict elections deeply entrenched by wartime cleavages to dysfunctional governmental institutions to low levels of public confidence in those central institutions and the public's consistent support for former warring parties. This vicious circle—initiated by power-sharing agreements—delays the establishment of democratic governance in post-conflict societies.

Testing the Theory

The long causal chain in my theoretical approach—nine steps—stems from the fact that conflict resolution and post-conflict reconstruction are as complicated as the escalation process from low-level to high-level violence in civil wars. Thus, the critical issue for my research was how this causal story could be investigated empirically. My overarching strategy was, first, to extract a set of hypotheses amenable to testing by large-N analysis, and then—second—to select two “best” cases, one positive and the other negative, to illuminate the short-term/long-term trade-off by process-tracing post-conflict political events.

The main rationale of a preliminary large-N analysis was to discover the conditions under which a democratic government is more likely to emerge and be sustained in war-torn societies, and to estimate the causal effects of power-sharing arrangements on post-civil war democratization. For the quantitative part of my research, I sorted out testable and non-testable hypotheses. By testable, I mean that hypothesized variables should be measurable by reasonable indicators without suffering large measurement errors. That is, my main crite-

riterion for testable hypotheses was to avoid using proxy variables too far removed from what I sought to measure. For instance, in my analysis, the time horizons of key actors during peace negotiations are theoretically important for establishing post-civil war democracy, as they influence the type of political institutions created as a result of the peace deal. But it is neither feasible nor ideal to measure quantitatively the time horizons of political actors. They should rather be assessed qualitatively in the actual context of the transition from war to peace.

Put specifically, statistical methods cannot take full account of the mechanism at work in the trade-off between short-term peacemaking and long-term democracy building. This is true even though I employ the most process-oriented estimation technique in the quantitative analysis toolkit—event history modeling—to investigate the success or failure of post-civil war democratization and to estimate the effects of power-sharing agreements on democracy promotion during post-conflict peace processes.² Thus, event history analysis must be combined with case studies to illuminate the entire trade-off mechanism. As one of the observable implications in the above causal story is that the trade-off mechanism comes into play if power sharing is imposed by international actors with short-term rationality (i.e., ending civil wars as quickly as possible), I selected two cases, Bosnia-Herzegovina and Mozambique, in which such a triggering event was present and absent, respectively, during peace negotiations. Pairing these two cases also enabled me to assess whether the divergent paths toward post-civil war democracy in Bosnia and Mozambique can be attributed to their distinctive institutional designs arranged during the negotiation process.

In this sense, my case selection strategy may sound close to a “model-testing small-N analysis” following a large-N study, in Lieberman's (2005) nested analysis framework for multi-method approaches. Yet the purpose of the paired comparison of Bosnia and Mozambique is not simply to confirm empirical findings of my large-N analysis (see also Nome, this Symposium). It also allows me to identify and measure the short-term/long-term trade-off dynamic during civil war resolution and post-war reconstruction processes, while illustrating the path-dependent nature of power-sharing arrangements that reinforce the status quo of the initial institutional set-up.

Conclusion

Using mixed methods has been particularly relevant for my research on post-civil war democratization in two respects. First, the decision to use multiple methods flowed from my research questions; it was puzzle before method (see also Symposium 2007: 9-10). Those questions could not be fully addressed without a careful examination of the complicated political processes during post-conflict periods, which is what qualitative case studies can do better than any other method. Second, a single method alone could not illuminate the short-term/long-term trade-off in which I was interested. Therefore, combining event history analysis and paired comparison of the two post-civil war countries was necessary to identify and measure the mechanisms that I theorized to be at work.

For sure, this essay describes just one way of mixing different methods for a particular research purpose. However, it “gets the balance right” as qualitative methods are no longer simply an auxiliary to a model (re-) specification exercise. The challenge now for students of civil war is to develop additional ways of combining multiple methods, with the goal being to connect rigorous empirical investigations of patterns of violence with the mechanisms underlying them.

Notes

¹ Of no less than 24 different and contested definitions of mechanisms (Mahoney 2001: 579-80), mine follows McAdam, Tarrow, and Tilly (2001: 24). They conceptualize mechanisms as “a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations.” For an excellent discussion on how to measure mechanisms, see McAdam et al. (2008).

² In event history analysis, the dependent variable is measured by the timing of a certain political event. Unlike conventional regression approaches, event history modeling thus considers “not only if something happens, but also when something happens” (Box-Steffensmeier and Jones 2004: 1).

References

- Bermeo, Nancy. 2003. “What the Democratization Literature Says—or Doesn’t Say—About Postwar Democratization.” *Global Governance* 9, 159-77.
- Box-Steffensmeier, Janet and Bradford Jones. 2004. *Event History Modeling: A Guide to Social Scientists*. New York: Cambridge University Press.
- Collier, Paul and Nicholas Sambanis, eds. 2005. *Understanding Civil War: Evidence and Analysis*, Vol. 1 and 2. Washington: World Bank.
- Doyle, Michael and Nicholas Sambanis. 2000. “International Peacebuilding: A Theoretical and Quantitative Analysis.” *American Political Science Review* 94:4, 779-802.
- Fearon, James and David Laitin. 2005. “Civil War Narratives.” Stanford University, Department of Political Science.
- Lieberman, Evan S. 2005. “Nested Analysis as a Mixed-Method Strategy for Comparative Research.” *American Political Science Review* 99:3, 435-51.
- Mahoney, James. 2001. “Beyond Correlational Analysis: Recent Innovations in Theory and Method.” *Sociological Forum* 16:3, 575-93.
- McAdam, Doug, Sidney Tarrow and Charles Tilly. 2001. *Dynamics of Contention*. New York: Cambridge University Press.
- McAdam, Doug, Sidney Tarrow and Charles Tilly. 2008. “Methods for Measuring Mechanisms of Contention.” *Qualitative Sociology* (forthcoming).
- Sambanis, Nicholas. 2004. “Using Case Studies to Expand Economic Models of Civil War.” *Perspectives on Politics* 2:2, 257-79.
- Symposium. 2007. “Multi-Method Work, Dispatches from the Front Lines.” *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods* 5:1, 9-27.
- Wantchekon, Leonard. 2004. “The Paradox of ‘Warlord’ Democracy: A Theoretical Investigation.” *American Political Science Review* 98:1, 17-33.

Getting the Pathway Case off the Drawing Board

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Put yourself in my place. I have been raised and socialized in the large-N research programme on civil war. My MA thesis was on third-party interventions in civil conflicts, where I found that interventions are more likely when the intervening state is home to an ethnic group whose kin is involved in the conflict. This confirms existing quantitative research. The time comes when I am to specify a research question for my PhD. An appropriate puzzle is not far off. What are the *mechanisms* connecting third-party military interventions in “ethnic” civil wars with transnational ethnic affinities? Then I learn that the way to develop theory with mechanisms rather than variables is to do process tracing, typically of a single case. The trouble is that I have a data matrix full of interventions from which to choose, and no clear guidance on where to begin.

Along comes nested analysis. I learn that cases can be selected according to their position in a population, having described that population using large-N analysis (Lieberman 2005; Gerring and Seawright 2007). The regression line describes the population and the residual positions the case. I seize the moment, I go for the pathway case selection technique (Gerring and Seawright 2007: 122-31), and two conference panels and one *QM* symposium later (Symposium 2007), I realize that the road from drawing board to satisfactory application is not so straightforward after all. There is a need to explain how the pathway case works, and to anticipate some lessons from its use.

I do two things in this essay. First, I emphasize the value added of the pathway case in terms of its mode of theory building, and show why it is different from the most-likely case or the typical case, to which it has been compared. This is a defence of Gerring and Seawright (2007) of sorts, but also an argument for its potential contribution to the research programme on civil war, as it offers explanation in terms of causal mechanisms as a complement to the growing body of generalizations. Second, I address four issues that arise when applying the pathway case selection technique. I discuss model specification, the role of measurement validity, the indeterminacy of case selection, and generalization. Not unscathed by the treatment, the pathway case re-emerges as less than ideal, but as a research design well worth applying.

The Pathway Case

Why a pathway case research design? The co-variation between transnational ethnic affinities and interventions in civil conflicts is known (Davis and Moore 1997; Saideman 2002; Nome 2007). One can think of a number of stories that make

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Theory Building, Not Theory Testing**

that co-variation plausible—bottom-up mobilization, top-down manipulation, irredentist ideology, ethnic outbidding, strategic framing of conflict cleavages—but there remains an observational and theoretical lag from the probabilistic relationship between independent and dependent variable to the causal mechanisms that generate it. Under such conditions, when a case of intervention is used to explicate the mechanisms linking transnational ethnic affinities with interventions, and when the co-variation of input and output already is confirmed by cross-case analysis, the pathway case is particularly useful (Gerring and Seawright 2007: 122).

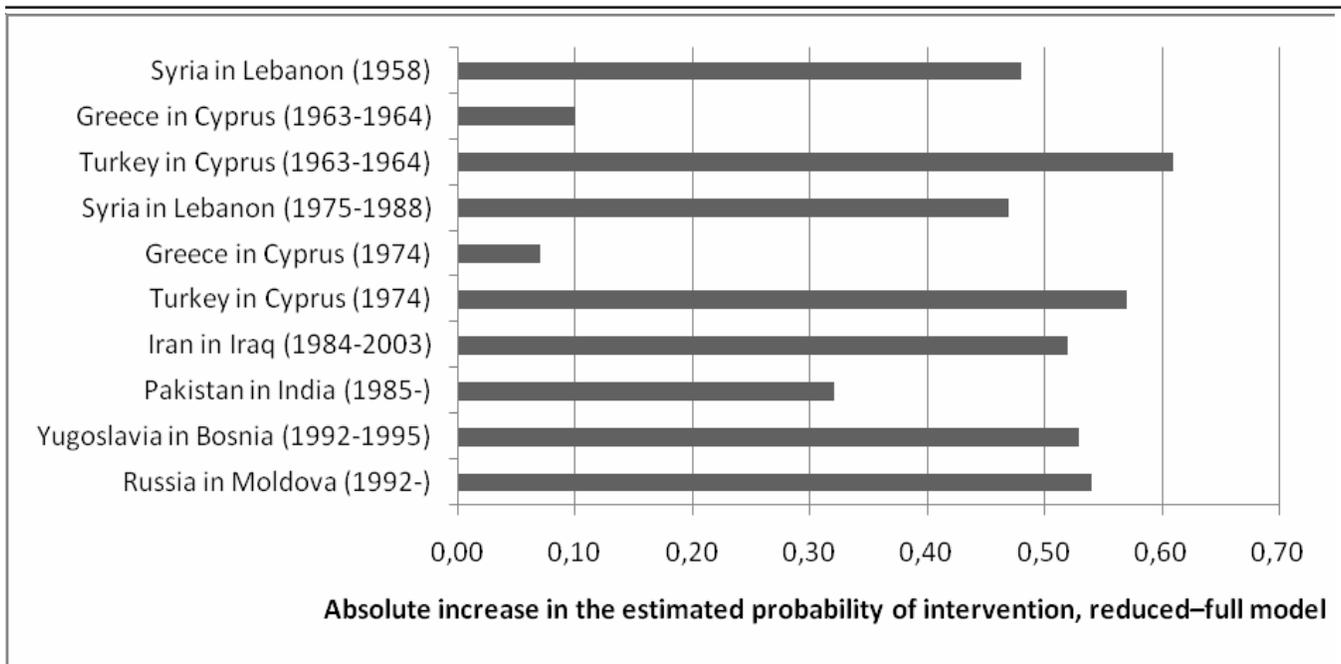
The pathway case isolates the effect of the independent variable of interest and then identifies the case(s) whose prediction is most improved by including the independent variable in the regression model. Consider, for example, Regan’s (2000) data on interventions in civil wars, to which I have appended data on ethnic ties.¹ I first ran a logit regression of interventions including only the most basic control variables—contiguity, intervener-target distance, and intervener-target power ratio—and estimated the probability of intervention for every case (reduced model). I then ran the model again, this time including transnational ethnic affinities among the independent variables, and again estimated the probability of intervention for every case (full model). Figure 1 displays the absolute improvement in the prediction of ten interventions with ethnic ties, when moving from the reduced to the full model. Allowing for some margins of error in prediction, it would seem that there are several pathway case candidates here. Among those, Turkey’s involvement in Cyprus (1963–64) is that whose prediction is most improved by including transnational ethnic affinities in the model. The logic of the pathway case then suggests that Turkey in Cyprus is a good case in which to study the causal mechanisms linking transnational ethnic affinities with intervention.

Why bother with the pathway case? First, it is to my knowledge the only form of nested analysis devised primarily for theorizing causal mechanisms. To appreciate this, it is worth distinguishing mechanisms from variables as modes of explanation. Mechanisms are not simply intervening variables or variables at lower levels of aggregation. Rather, they are empirically grounded, theoretically framed “*recurrent processes linking specified initial conditions and a specific outcome*” (Mayntz 2004: 241, italics in original). In the language of variables, mechanisms are the processes at work when one observes a particular value on the independent variable of interest (Mayntz’s “specified initial condition”) in conjunction with a particular value on the dependent variable (Mayntz’s “specific outcome”). This has important implications for the sort of statements I want to make in my thesis. I do not want to say “transnational ethnic affinities are associated with interventions, probably because of A, B, and C.” This is already known. Rather, I want to say “transnational ethnic affinities cause interventions by mechanism X, Y, and Z under scope condition U, V and W,” and I want theoretical and empirical leverage to back it up (see also Arjona, this symposium). Process tracing, properly done, can provide such leverage, and the pathway case can expand the domain of inference—a point to which I will return below.

One critical voice has asked how the pathway case is any different from the crucial case strategy or the typical case (Lieberman 2007). I have also been asked why a deviant case might not be more fruitful for theory building than a pathway case. Such questions deserve an answer, and the beginnings of a response lie in establishing two dichotomies.

The first dichotomy is that between theory testing and

Figure 1



theory building. Whereas the crucial case is at the theory-testing end of the scale, the pathway case is closer to the theory-building pole. The type of crucial case I have in mind here—a most-likely case *without* the anticipated outcome—would be used to test and weaken a theory. The pathway case is also a most-likely case, but in contrast to the crucial case it comes *with* the anticipated outcome; its purpose is not to test a theory of input and output, but to build theory about the mechanisms connecting cause and effect.

The pathway case has also been compared with the typical case. Yet the purpose of the typical case is more general—exploring causal mechanisms, endogenous relationships, omitted variables—and its primary selection criterion is proximity to the regression line—being typical, so to speak, of a set of relationships (Gerring and Seawright 2007, 91-7). The pathway case is also close to the regression line, but it identifies the independent variable that has contributed most significantly to moving it there. And the purpose of the pathway case is very specific—to develop theory on the mechanisms that explain the contribution of that independent variable.

The second dichotomy is that between two modes of theory building. On one hand, there is theory building as generating hypotheses about new independent variables. If such hypotheses are confirmed, then the image is that of cases being pulled closer to the regression line and explained variance increasing. On the other hand, there is theory building as developing mechanisms that explain the effect of already known independent variables (see also Jung, this symposium). Mechanisms may not be amenable to the kind of standardized observations required for large-N analyses. The deviant case, defined as an outlier in a cross-case relationship, may be excellent for theory development of the first sort, but it is not a substitute for the pathway case, which specifically is for theory development of the second sort.

The distinction between theory-building-as-variables and theory-building-as-mechanisms is also useful to show how the pathway case can expand our repertoire and rationales beyond Lieberman's (2005) widely cited idea of nested analysis. His contribution is to show how particular cases can be explicitly embedded and positioned within their large-N samples, thus offering an operational answer to the call from case study methodologists who insist that cases should be selected with prior knowledge of the universe. This is a lesson to retain. Yet if the objective is to use the case to uncover causal mechanisms, then this is as far as Lieberman takes us. His suggestions on how to connect large-N and case analysis are based on an understanding of theory building as variables. This is apparent in claims such as "the elaboration of concepts and mechanisms can best be accomplished through *comparison*" and "comparison provides an empirical basis for making narrative assessments of counter-factual claims—that is, an event would have happened a different way had the score on a key variable or set of variables been different" (Lieberman 2005: 441, italics in original).

This emphasis on comparison is incompatible with the nature of process tracing—the methodological foundation of

mechanisms-based explanation—and the counter-factual logic indicates a view of cause as co-variate. Lieberman's examples reinforce this impression. When questions of causality arose, cases were selected based on different scores on the independent variables, tracing their varying impact on the dependent variable (Lieberman 2005: 444). Finally, theory building is viewed solely as finding new explanations for deviant cases—new causal factors, or new independent variables—not as uncovering and theorizing new causal mechanisms. Using the image of cases' position relative to the regression line, Lieberman (2005: 445) argues that "only when the scholar has good reason to believe that a particular case is on-the-line for entirely spurious reasons would it be useful to select such a case for [model-building small-N analysis]." If one moves from theory building-as-variables to theory building-as-mechanisms, however, cases on the regression line that are well and *non-spuriously* explained by multivariate models may be very fruitful indeed for theory building (see also Ragin 2006). Such is the pathway case.

Middle-Ground Overtures

The pathway case thus has something unique to offer in terms of nesting the process tracing of a single case within its large-N cross-case sample. Yet, taking it off the drawing board and actually applying it suggests that the real is somewhat less than the ideal. In particular, issues of model specification, measurement validity, and the indeterminacy of case selection need to be addressed.

First, using a regression analysis as the foundation for selecting the pathway case requires the "right" parameter estimates. The effect of the variable of interest ought to be known, at least as it relates to its control variables. Yet the magnitude of estimated effects varies not only as a function of co-variation in the systematized concepts that are measured, but also as a function of the variables that are included in the models, as well as the statistical assumptions that are applied. To build confidence about case selection, both questions of model specification and statistical assumptions must be considered and addressed.

Second, uncertainty is compounded if there are reasons to be sceptical of the measurement validity of key variables (see also Jung, this symposium). If there is doubt that the proxies adequately capture their systematized concepts, if the outcome of interest is not caused by what our measures and their effects lead us to believe, then nested analysis is less than useful.

Third, consideration of model specification and the role of measurement validity, the probabilism of statistical associations, and the margins of error involved when predicting outcomes on the dependent variable, all suggest that the selection of a pathway case as a nesting strategy is inherently indeterminate. Rather than finding the ideal pathway case where the independent variable of interest was sufficient to produce the outcome, one is more likely to find a set of pathway case candidates where the independent variable of interest contributed more to produce the outcome than it did in other cases.

Such issues and concerns suggest that one would be well

advised to moderate the image of the ideal pathway case. Indeed, I doubt that any such case exists. The loss may not be so great, however, considering the consequences the pathway case has for generalization.

In terms of generalization, the pathway case inhabits a middle ground that Gerring and Seawright (2007) overestimate and their critics underestimate (Coppedge 2007; Mahoney 2007). The embeddedness of a case in its population and thus its “representativeness” is essential to Gerring and Seawright (2007: 147). The aim is to infer from case to population. This emphasis obscures the extent to which generalization—particularly from the pathway case—is circumscribed by the more or less restrictive scope conditions placed on the mechanisms. The other position—that it is never safe to generalize from case studies (Coppedge 2007: 3), and that the primary (but ignored) goal of many case studies is solely to explain a particular outcome in a specific case (Mahoney 2007: 7)—errs on the side of case idiosyncrasy. The pathway case is in a middle position because it can produce the conditional generalization characteristic of middle-range theory (Johnston 2005). While the regression-based selection technique maximizes the domain of inference to the extent that the chosen case approaches the ideal pathway case, the focus on mechanisms and their scope conditions limits the inference to a narrow class of phenomena.

My project is part of a move to complement the research programme on civil war with mechanisms-based explanations. There is an imbalance to redress, and a pathway case of intervention in civil war is in a good position to do so, by taking advantage of a vigorous field of quantitative work, and by embedding within it a study of causal processes. In a recent exchange with John Gerring, he warned that the reaction of readers to the above might be of the sort, “OK, so where’s the beef? The case study?” Well, the beef is yet to come. In the meantime, a necessary first step is to justify the pathway case as a research design because of the misapprehensions surrounding it and because of the challenges that arise when moving from theory to practice.

Note

¹ The model and data are available at <http://folk.uio.no/martaus/Papers/>.

References

- Coppedge, Michael. 2007. “Case Studies Are for Intensive Testing and Theory Development, Not Extensive Testing.” *Qualitative Methods: Newsletter of the American Political Science Association Organized Section for Qualitative and Multi-Method Research* 5:2, 2-3.
- Davis, David. R. and Will H. Moore. 1997. “Ethnicity Matters: Transnational Ethnic Alliances and Foreign Policy Behavior.” *International Studies Quarterly* 41:1, 171-84.
- Gerring, John and Jason Seawright. 2007. “Techniques for Choosing Cases.” in *Case Study Research: Principles and Practices*. New York: Cambridge University Press.
- Johnston, Alastair. Iain. 2005. “Conclusions and Extensions: Toward Mid-Range Theorizing and Beyond Europe.” *International Organization* 59:4, 1013-44.

- Lieberman, Evan. S. 2007. “Moving the Doormat to the Main Menu: Case Study Research Methods in the Social Science Toolkit.” *Qualitative Methods: Newsletter of the American Political Science Association Organized Section for Qualitative and Multi-Method Research* 5:2, 3-6.
- Lieberman, Evan S. 2005. “Nested Analysis as a Mixed-Method Strategy for Comparative Research.” *American Political Science Review* 99:3, 435-52.
- Mahoney, James. 2007. “Particularizing Case Studies: A Critical Review of Gerring’s *Case Study Research*.” *Qualitative Methods: Newsletter of the American Political Science Association Organized Section for Qualitative and Multi-Method Research* 5:2, 6-8.
- Mayntz, Renate. 2004. “Mechanisms in the Analysis of Social Macro-Phenomena.” *Philosophy of the Social Sciences* 34:2, 237-59.
- Nome, Martin. A. 2007. “Ties That Decide: Transnational Ethnic Determinants of Foreign Support for Civil War Parties.” University of Oslo, Department of Political Science.
- Ragin, Charles C. 2006. “How to Lure Analytical Social Science Out of the Doldrums: Some Lessons from Comparative Research.” *International Sociology* 21:5, 633-46.
- Regan, Patrick. M. 2000. *Civil Wars and Foreign Powers: Outside Intervention in Intrastate Conflict*. Ann Arbor: University of Michigan Press.
- Saideman, Stephen. M. 2002. “Discrimination in International Relations: Analyzing External Support for Ethnic Groups.” *Journal of Peace Research* 39:1, 27-50.
- Symposium. 2007. “John Gerring, *Case Study Research: Principles and Practices* (Cambridge, 2007).” *Qualitative Methods: Newsletter of the American Political Science Association Organized Section for Qualitative and Multi-Method Research* 5:2, 2-15.

Moving Targets: Mixing Methods to Uncover Dynamics of Displacement in Civil Wars

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It hardly needs stating that civil wars are complex and disorderly. Perhaps that is why for so long, social scientists employed qualitative *or* quantitative methods to address civil war onset or ending, but avoided questions related to warfare and other phenomena during wars (Kalyvas 2003). More recently, though, scholars have brought a combination of qualitative and quantitative methods to bear on variation in wartime dynamics (Wood 2003; Kalyvas 2006). In this essay, I suggest that employing a combination of qualitative and quantitative methods not only improves our understanding of displacement during civil wars, but also enables us to characterize it better and identify the most critical forms of variation to explain.

This clarification is essential. In spite of substantial academic and humanitarian attention to refugee movements and internal displacement, many aspects of migration during war remain puzzling. I define displacement as civilian migration during war that is provoked, directly or indirectly, by the actions of one or several armed groups.¹ This definition implies

the interaction of civilians and armed groups, so what is to be explained is not only the behavior of the household, but the behavior of a household given the behavior of the armed group(s), and vice-versa. The literature on displacement—dominated by either cross-national, large-N or case studies—implies this dynamic, but the predominant hypothesis is that higher levels of violence (however conceived) yield more displacement, proportional or absolute (Schmeidl 1997; Cohen and Deng 1998; Weiner 1992, 1996; Zolberg 1989; Davenport, Moore, and Poe 2003; Moore and Shellman 2006). While sensible, and even useful for humanitarian purposes, this approach does not allow for deeper understanding of the problem, because it obscures important variation in armed group and civilian behavior within and across wars. Household-level data from Indonesia and Colombia provide evidence that while higher levels of violence correlate with higher levels of displacement, variation exists among households about whether or not to displace, and when to do so, given similar levels of violence (Czaika and Kis-Katos 2007; Engel and Ibáñez 2007).

My own interviews in Colombia confirm this general pattern. I found that *how* people displace—in terms of where they resettle, for how long, and how they behave in new communities—is an equally important question to whether or not they displace at all. Implications for household well-being and local-level order and development relate not only to how many people resettle in a given place, but where they come from and why. Had I only focused on analyzing the large-N, household-level data that I have on displacement within Colombia over the last 15 years (with over 2 million observations!), it never would have occurred to me to change my research question. In other words, I used my qualitative fieldwork as part of an inductive theory-building process that also helped me identify what I should be building theory about.

Theory

The question I now ask in my dissertation is what explains variation in how civilians displace during civil wars. I argue that how civilians are targeted—for their behavior, identity, or location—explains if they are likely to resettle, where they go, how they integrate in new communities, and how long they stay. At the community level, targeting patterns have implications for the scale of displacement at any given time and cleavage formation within communities.

Behavior: Armed groups target individuals for behavior such as refusing to collaborate, defecting, or supporting a rival armed group. Such a transgression is generally punished severely, if the armed group has good intelligence. In such a case, civilians have limited options to avoid violence to themselves or their family. They can attempt to change their behavior, they can “hide” by seeking anonymity in a new community, or they can seek protection from a rival armed group. However, protection may be difficult to attain if they are targeted by a heavily favored armed group, so an individual’s best bet may be to seek anonymity in a sufficiently large city.

Identity: Identity-based targeting refers to a situation in which a household is part of, or perceived to be part of, a group that is associated with an armed group. Even though a

particular household may have no stake in the conflict between groups, both armed groups and civilians use informational short-cuts to identify potential rivals and sympathizers. “Identity” can range from political ideology to ethnicity to religion to profession. Faced with possible violence because of its identity, under some circumstances, a sensible option for a household would be to move closer to the group of civilians that shares the characteristics for which it is being targeted by an armed group. Moving to a predominantly Shi’a, leftist, or Kikuyu region or neighborhood can reduce the odds that a particular Shiite, leftist, or Kikuyu household will suffer violence based on group-level targeting. This relationship should be stronger as group-level identifiers are more physically apparent. Additionally, the household could seek protection from the armed group it is perceived to be supporting, potentially creating identities endogenous to the conflict, or “hardening” those that already exist.

Location: I call location-based targeting those situations in which civilians are potentially in danger because of where they live. For example, households could face the threat of cross-fire between two armed groups. In such cases, because the threat is linked to their location at that moment, changing location for a relatively short period of time is usually a feasible option. Households threatened in such circumstances are likely to remain close to their communities.

These expectations are based on my sense that households primarily attempt to reduce the odds that they will suffer direct violence. Depending on the type of targeting they face, some displacement strategies improve these odds better than others. The underlying mechanisms—assessing threats, searching for the best option to avoid direct violence—are the building blocks of the theory.² The logic of household-level displacement behavior should operate in many kinds of civil wars, even though it potentially aggregates to substantially different national-level patterns. In the section that follows, I describe how I plan to test the plausibility of the mechanisms.

Research Design

My theory presents numerous challenges to empirical testing: armed groups’ targeting patterns and household-level decision-making processes are impossible to directly observe. Indirect testing is possible, however, by specifying observable implications (Geddes 2003). At the household level, I focus on implications for destination, duration, and how households are likely to behave in the receptor communities.³ To test these implications, I combine econometric analyses, interviews, and case studies at the sub-national level in Colombia.

The Colombian civil war is an apt context in which to test implications of the theory and to assess the plausibility of its mechanisms. Analytically, the war is irregular, implying that armed groups use violence (or its threat) as a resource to shape civilian behavior (Kalyvas 2006). Further, many armed groups exist in Colombia. In addition to the state military forces, two communist-inspired guerrilla groups and a host of right-wing paramilitary groups have contested for control of communities. This diversity will provide confidence that the conclusions of the study are not limited to a particular type of armed

group. Because the war is not based around an identity cleavage, I can more directly assess the relationship between behavior-based targeting and displacement. At the same time, group-level characteristics do exist, and will thus allow me to test hypotheses related to identity-based targeting. On the practical front, relative to other civil wars, data on displacement in Colombia have been collected by various groups for the past 20 years, and the government has registered and collected extensive data on internally displaced households since 1995. Given the micro-foundations of my argument, data at the household level are necessary to evaluate it. In addition, even though the war is ongoing, it is possible to combine interviewing and excellent secondary materials to conduct in-depth case studies of some communities.

To illustrate how I combine methods and data sources to evaluate the theory, I will focus on the implications for the expected destination of civilians in each of the three targeting categories. The data include several large-N, household-level datasets on displacement in Colombia, interviews, and case studies. One large-N dataset is the government registry of internally displaced people (IDPs), which has roughly 2.5 million observations and contains household-level characteristics on a range of variables. I also have survey data collected by Ana María Ibáñez, an economist at the Universidad de los Andes. In 2,322 surveys carried out with a stratified sample in 48 municipalities of the country, Ibáñez gathered detailed data on the circumstances leading to households' decisions to displace.

Finally, I will conduct case studies of six communities to trace patterns of targeting and displacement choices over time.⁴ The case studies enable me to evaluate community-level implications of the theory related to armed group activity, the scale of displacement and resettlement over time, and the composition of community in terms of political preferences. In addition, the cases also allow for gathering household-level data more effectively than if I randomly selected households to interview. In the context of an ongoing war, it is important to gain the trust of subjects, and two effective ways for doing this are introduction through a contact and repeated interviews (Wood 2003). Further, in-depth local knowledge is critical to both signaling credibility and triggering important narratives that subjects might otherwise find irrelevant.

Behavior: With the survey data, I can evaluate implications related to households' destination if they have been targeted for their behavior, as opposed to more general threats. All else equal, I would expect those households explicitly threatened by an armed group to displace to an urban destination. This is likely because anonymity is relatively easier to secure in an urban area than in smaller communities.

Even with such fine-grained, large-N data, there is still room for qualitative data to sharpen the analysis. If the statistical analyses suggest that there is a significant correlation between targeting type and destination type in the way I expect, it would not rule out alternative explanations for such a relationship. The qualitative work is meant to provide more confidence that the correlation is meaningful because of the logics I propose. Rather than assume that those targeted for

behavior went to urban destinations because they tend to offer better chances for anonymity, I plan to study whether such a consideration played a role in the decision-making of those households. If the interviews with IDPs in case study communities indicate that the targeting they experienced did not play a role in their destination decisions, then the theory will not be supported.

Identity: I expect that people who are targeted because of their identity and presumed affiliation with one side or another will be more likely to go to places where people similar to them already live. In the Colombian context, many communities in the Pacific coast department of the Chocó, which are predominantly Afro-Colombian, were under the influence of guerrilla groups more than paramilitaries or even the state until recently. Thus it is possible that given the inability to change or conceal their identity, Chocoanos could be easily targeted in receptor communities for presumed association with guerrillas. According to the logic of the argument, living among others with the same identity will reduce the odds that any one household will suffer direct violence. All else equal, I would expect to observe less dispersion of Chocoanos than other subsets of the Colombian IDP population. With large-N, household-level data on municipalities of origin and resettlement, and ethnicity, I can test this implication.

At the same time, even if the relationship is strong as expected, it will be consistent with many alternative explanations. In particular, it is possible that Afro-Colombians follow traditional migratory trends and rely on social networks to help establish themselves in new communities. In the case study areas, I will interview Afro-Colombians about the decision-making process leading to where they sought to resettle. If I find that security concerns rather than networking predominated, then I will gain confidence in my explanation.

Location: With a dataset on violent events by municipality/day, I can use clashes between armed groups as a proxy for location-based targeting. Because I have municipal-level data on where households were displaced and where they arrived, I can use destination as an indicator for how households respond to the threat. I would expect that around the time of a clash, a relatively greater proportion of the displaced would remain within the municipality, rather than relocate farther away, because they would be more likely to return to their communities once the cross-fire threat subsided. Preliminary tests for events and displacement destination with 2004 data have borne this out. This lends credibility to the hypothesized relationship between location-based threats and short-term, shorter-distance destinations.

An additional validity test of the argument will be to investigate whether there were instances of short-term displacement from the case study communities that did not lead to registration with the government. If the theory is consistent with reality, I would expect to find that at least some people temporarily displace and never appear in the dataset. This sort of activity would also help explain a recent finding in a cross-national study by Melander and Oberg (2007) that intensity of fighting did not correlate to higher refugee flows. It may just be that we are not observing the refugees or IDPs at such a

high level of aggregation.

In sum, the research design combines econometrics to test for correlations that would be consistent with my argument, and interviews and case studies to evaluate whether the mechanisms that I think explain those correlations are plausible. In this way, I will be able to increase confidence that alternative explanations cannot account for the outcomes observed. Additionally, the cases provide a more complete picture of displacement and fill some potentially important gaps in the large-N data. Finally, case studies will provide a check on the large-N data measures to ensure that the statistical models are well specified. Multiple tests of implications at different levels of analysis, on various dimensions of displacement, will add confidence that the theory is capturing important dynamics of displacement.

Conclusion

In the study of civil wars, mixing methods provides the best way to get a sense of what patterns we should focus on understanding, how they emerge, and how they vary over time and across space. The mixed approach outlined here will help us gain analytic leverage over the problem of displacement during wars. Indeed, where and how people settle and constitute communities seems to be an overlooked—and crucial—element of how order is established at the local level, by the state or other actors during civil war. When households displace, they arrive in new communities not just as needy recipients of humanitarian attention (though this is certainly true), but also as actors in a dynamic of ongoing violence, especially given their generally precarious security situation.

If my argument about targeting captures these dynamics, then forms of targeting should generate different community-level outcomes. Civilians avoiding behavior-based targeting are more likely to want to remain “invisible” in their communities, perhaps inhibiting the kinds of interactions necessary to overcome local-level collective action problems. If civilians are targeted for their identity, they will be more likely to displace to areas where others “like them” already live, generating segregation patterns and the possibility of a local-level dynamic resembling in-group solidarity, but also possibly out-group hostility.

These mechanisms overlap with those linked to the diffusion of violence in international contexts (Lischer 2005; Salehyan and Gleditsch 2006), but they also have implications for the development of warfare within one civil war. The endogenous effects that targeting patterns can have on future settlement and security issues could also be critical for post-war cleavages, political behavior, stability, and economic development.

Notes

¹ In this essay, “armed groups” will refer to state and insurgent groups, as well as to militias and paramilitaries; I am agnostic about the political aims.

² As with Arjona (this symposium), I follow Elster’s definition of mechanisms as “frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or

with indeterminate consequences” (Elster 1998). Studying mechanisms is the difference “between a static correlation (‘if X, then Y’) and a ‘process’ (‘X leads to Y through steps A, B, C’)” (George and Bennett 2004: 141).

³ My dissertation also examines community-level implications, but I focus on the household here.

⁴ To select the cases, I rely on in-depth knowledge of communities, and the kind of armed group presence and competition that existed there.

References

- Cohen, Roberta and Francis M. Deng. 1998. “Introduction.” in *Forgotten People: Case Studies of the Internally Displaced*. Roberta Cohen and Francis M. Deng, eds. Washington: Brookings Institution Press.
- Czaika, Mathias and Krisztina Kis-Katos. 2007. “Civil Conflict and Displacement: Village-Level Determinants of Forced Migration in Aceh.” Households in Conflict Network, Working Paper 32 (Available at <http://www.hicn.org/papers/wp32.pdf>).
- Davenport, Christian A., Will H. Moore, and Steven C. Poe. 2003. “Sometimes you Just Have to Leave: Threat and Refugee Movements, 1964-1989.” *International Interactions* 29, 27-55.
- Elster, Jon. 1998. “A Plea for Mechanisms.” in *Social Mechanisms: An Analytical Approach to Social Theory*. Peter Hedström and Richard Swedberg, eds. Cambridge: Cambridge University Press.
- Engel, Stefanie and Ana María Ibañez. 2007. “Displacement due to Violence in Colombia: A Household-Level Analysis.” *Economic Development and Cultural Change* 55:2 (January), 335-65.
- Geddes, Barbara. 2003. *Paradigms and Sand Castles: Theory Building and Research Design in Comparative Politics*. Ann Arbor: University of Michigan Press.
- Kalyvas, Stathis N. 2003. “The Ontology of ‘Political Violence’: Action and Identity in Civil Wars.” *Perspectives on Politics* 1:3, 475-94.
- Kalyvas, Stathis. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.
- Lischer, Sarah Kenyon. 2005. *Dangerous Sanctuaries: Refugee Camps, Civil War, and the Dilemmas of Humanitarian Aid*. Ithaca: Cornell University Press.
- Melander, Erik and Magnus Oberg. 2007. “The Threat of Violence and Forced Migration: Geographical Scope Trumps Intensity of Fighting.” *Civil Wars* 9:2, 156-73.
- Moore, Will H. and Stephen M. Shellman. 2006. “Refugee or Internally Displaced Person? To Where Should One Flee?” *Comparative Political Studies* 39:5, 599-622.
- Salehyan, Idean and Kristian Skrede Gleditsch. 2006. “Refugees and the Spread of Civil War.” *International Organization* 60:2, 335-66.
- Schmeidl, Susanne. 1997. “Exploring the Causes of Forced Migration: A Pooled Time-Series Analysis, 1971-1990.” *Social Science Quarterly* 78, 284-308.
- Weiner, Myron. 1992. “Security, Stability, and International Migration.” *International Security* 17:3, 91-126.
- Weiner, Myron. 1996. “Bad Neighbors, Bad Neighborhoods: An Inquiry into the Causes of Refugee Flows.” *International Security* 21:1, 5-42.
- Wood, Elisabeth. 2003. *Insurgent Collective Action and Civil War in El Salvador*. Cambridge: Cambridge University Press.
- Zolberg, Aristide R. 1989. “The Next Waves: Migration Theory for a Changing World.” *International Migration Review* 23, 403-30.

Mixing It Up: The Role of Theory in Mixed-Methods Research

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Mixing methods is about using both qualitative and quantitative techniques and connecting them in an analytically meaningful way. Such an approach can serve to bridge the two principal civil war research communities, represented by Kaufman (2001) and Tilly (2003) on the qualitative side, and by Collier and Hoeffler (2001), Hegre, et al. (2001) and Fearon and Laitin (2003) on the quantitative side. Like Wood (2003), Kalyvas (2006), and Weinstein (2007), all four of the doctoral dissertations featured in this symposium capture a dynamic aspect of intrastate conflict, and do so by mixing quantitative and qualitative methods.

Ana Arjona examines variation in the relationships between armed groups and civilians in irregular warfare, with a particular focus on their strategic interactions. She combines game theory, statistical analysis of original survey and existing quantitative data, a controlled-comparison study of six cases, and an in-depth analysis of interviews. Jai Kwan Jung studies the conditions under which post-conflict democracy is more likely to emerge and survive. He pairs case studies of Bosnia and Mozambique with large-N analysis, with a focus on the time horizons of political actors.

Martin Austvoll Nome's doctoral dissertation examines the mechanisms connecting transnational ethnic affinities and third-party interventions in civil wars. He extends Gerring and Seawright's (2007) pathway case selection technique through a two-step procedure. Nome first runs a large-N cross-national analysis of third-party intervention to establish a baseline probability. He then adds data on transnational ethnic affinities and identifies the change in baseline probability of intervention. Those countries that experienced the greatest change in the probability of military intervention are then chosen for process-tracing analysis. Abbey Steele's dissertation is about civilian displacement in civil war. She combines statistical analyses, interviews, and process-tracing case studies of six communities. Each of these projects offers a dynamic perspective on some particular aspect of civil conflict that significantly contributes to our understanding of civil war more generally.

My comments are structured as follows. I begin with a general discussion that applies to all four contributions; these comments stress the central role of theory for understanding or interpreting causal relationships. Then, I discuss how mixed methods can help build theory; problems of measurement error and model specification are emphasized. The analytical distinction between theoretical explication and evaluation or testing is a point that I stress.

The Importance of Theory

Mixed methods offer a number of advantages, and I applaud these efforts to integrate approaches. Nonetheless, I want to emphasize the limits of any methodology. Theory must take primacy over method. Theory offers the perspective through which we can interpret empirical observation, quantitative or qualitative.

Without strong theory—and I would argue that we lack a solid theoretical basis for understanding civil war—sophisticated statistical analyses become problematic. Complex simultaneous equations, factor analysis, and covariance structure models have become increasingly commonplace in various areas of political science. In most respects, this increasing complexity is well and good; however, one costly side effect is to multiply the number of difficult, and often arbitrary, specification decisions upon which any empirical result depends. Too often, we lack the strong theory necessary to specify clearly how observable indicators are related to latent variables, or which variables in a structural equation are exogenous and which are endogenous (Bartels and Brady 1993: 140-41).

By theory, I mean explanations about how a phenomenon varies in space and time. Furthermore, strong theory is defined by clear and explicit theoretical explanations—indeed, by a “near-obsessive concern with clarity and explicitness” (Elster 2007: 455). Rigorous models, especially formal ones, “force clarity about assumptions and concepts; they ensure logical consistency, and they describe the underlying mechanisms that lead to outcomes” (Granato et al. 2005: 5; see also Powell 1999). I do not think it matters whether the theories come from a rational choice tradition or not, but I do think modelling rigor is important. Sound statistical analysis depends on strong theory.

Substituting methods or mixing methods does not allow us to escape the need for theory (see also Checkel, this symposium). The questions asked in an interview and the interpretation of events in a process-tracing case study are shaped by theory. The conceptualization of the events that compose a civil war and the phenomena related to it are shaped by our theories. Indeed, how we define a civil war stems from theory. We call civil war an event, but it is actually a series of associated events composed of armed combat; recruiting, organizing, and mobilizing armies; establishing and moving bases; occupying and attempting to control territory and associated populations; etc. Moreover, each of these events can be further subdivided.

Theory allows us to impose a structure for demarking whether there is or is not a civil war. How we measure, interpret, and conceptualize stems from our theory, whether we employ quantitative or qualitative methods. Understanding patterns of causation come from our theoretical explanations and not from our method.

In his appeal for “nested analysis as a mixed-method strategy for comparative research,” Lieberman (2005) distinguishes between model-testing and model-building. However, distinguishing between these two approaches in practice is not so easy. All social science to some degree involves a mix of deduction and induction; model-testing and model-building go

hand in hand. Given the state of theories regarding civil war, model-testing inevitably works to build theory. Ultimately, mixed methods serve to develop theory, whether or not the intention was to test a large-N quantitative model.

The Promise of Mixed Methods

Mixing methods offers a wonderful opportunity to examine “dogs that don’t bark.” These are cases where our statistical model predicts that we should see the phenomenon that we are investigating, but we do not see it. Like Sherlock Holmes in “The Hound of the Baskervilles,” these dogs that don’t bark can provide valuable information about causal processes. Take, for example, a cross-national large-N analysis of civil war onset with a dichotomous dependent variable and a statistical technique such as logit. “Dogs that don’t bark” would be the cases for which we expect to see civil war, but do not. Depending on how we define civil conflict, Madagascar may be such a case. Our models predict that we should have civil war in Madagascar, but we do not see it.

Lieberman (2005: 445) discusses something similar in the context of a continuous dependent variable in a regression analysis when he invokes an “extreme case,” one which is more than two standard deviations from the predicted value. Using qualitative methods to explore such outliers may reveal much about our causal explanation. It might also reveal a good deal about the way our parameters have been measured and operationalized. Indeed, model specification is another problem that can be mitigated through qualitative investigations of outliers, as these may reveal critical missing variables.

Qualitative analyses of cases that are more or less predicted by the large-N analysis are also valuable. Measurement error is a significant problem in quantitative research—one that qualitative methods can help to bound and reduce. The interviews employed by Steele and Arjona, for example, serve as a useful way to check the data collected from large-N surveys. The process-tracing technique employed by Nome and the comparative case studies of Arjona, Jung, and Steele also provide the means for evaluating the measurement and operationalization of quantitative variables.

Mixed methods can also serve to better differentiate between parameters that shape the baseline probability of an event and those parameters that precipitate its occurrence. Most large-N analysis of civil war has been good at identifying those variables highly correlated with civil war. However, our understanding of the role of precipitating factors or shocks—elections, natural disasters, or price shocks—is more limited. Process tracing, in particular, offers significant opportunities for examining how certain events can serve as triggers.

Missing variables, especially missing key variables, is a problem of model misspecification. As Jung demonstrates, certain parameters in civil war—such as the time horizons of political actors—are not measurable; qualitative analysis provides an alternative way to assess them.

A mixed-method approach offers a way to develop and enhance theory. Our understanding of civil war remains underdeveloped. This is partly because only recently has it received significant attention from the academic community, but it is

primarily due to the complexity of the phenomenon. The iterative nature of theorizing, evaluation, and theory refinement, entailing both deductive and inductive procedures, is the state of the field now. Whether we refer to the process as model-testing or model-building, both contribute to the development of theories on civil war. The fact that all the contributors seek to explain only certain aspects of civil war is also to be welcomed; indeed, it reflects a new level of maturity for this area of study.

The micro-foundational aspects of theory and data found in Arjona and Steele’s dissertations help with regard to the mixed method approaches they adopt. Working within the rational choice tradition allows them to build upon a large and extensive body of research. Mixed methods allow them to evaluate the assumptions and predictions of their models (Morten 1999; see also Checkel, this symposium).

Research on civil wars has tended to examine how they begin, how they are sustained and what it takes to end them. Lately, more and more effort has been directed to understanding the dynamics of the wars themselves and the processes that lead to the development of a long-lasting civil peace. These four dissertations—through their self-conscious and disciplined use of mixed methods—are contributing to these cutting-edge areas of theory development.

References

- Bartels, Larry M. and Henry E. Brady. 1993. “The State of Quantitative Political Methodology.” in *Political Science: The State of the Discipline II*. Ada W. Finifter, ed. Washington: American Political Science Association.
- Collier, Paul and Anke Hoeffler. 2001. “Greed and Grievance in Civil War.” World Bank Policy Research Working Paper 2355. Washington: World Bank.
- Elster, Jon. 2007. *Explaining Social Behavior: More Nuts and Bolts for the Social Sciences*. Cambridge: Cambridge University Press.
- Fearon, James D. and David D. Laitin. 2003. “Ethnicity, Insurgency, and Civil War.” *American Political Science Review* 97:1, 175-186.
- Gerring, John and Jason Seawright. 2007. “Techniques for Choosing Cases.” in *Case Study Research: Principles and Practices*. New York: Cambridge University Press.
- Granato, Jim, Melody Lo, and M.C. Sunny Wong. 2005. “Not Even Wrong: Using the Empirical Implications of Theoretical Models (EITM) to Maximize Falsification.” Paper presented at the Annual Meeting of the Southern Political Science Association, New Orleans, LA (January).
- Hegre, Håvard, Tanja Ellingsen, Scott Gates, and Nils Petter Gleditsch. 2001. “Toward a Democratic Civil Peace? Democracy, Political Change, and Civil War, 1816-1992.” *American Political Science Review* 95:1, 33-48.
- Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.
- Kaufman, Stuart J. 2001. *Modern Hatreds: The Symbolic Politics of Ethnic War*. Ithaca: Cornell University Press.
- Lieberman, Evan S. 2005. “Nested Analysis as a Mixed-Method Strategy for Comparative Research.” *American Political Science Review* 99:3, 435-51.
- Morten, Rebecca B. 1999. *Methods and Models: A Guide to the Empirical Analysis of Formal Models in Political Science*. Cambridge: Cambridge University Press.
- Powell, Robert. 1999. *In the Shadow of Power: States & Strategies in*

- International Politics*. Princeton: Princeton University Press.
- Tilly, Charles. 2003. *The Politics of Collective Violence*. Cambridge: Cambridge University Press.
- Weinstein, Jeremy. 2007. *Inside Rebellion: The Politics of Insurgent Violence*. Cambridge: Cambridge University Press.
- Wood, Elisabeth Jean. 2003. *Insurgent Collective Action and Civil War in El Salvador*. Cambridge: Cambridge University Press.

Co-Variation and Causal Process Tracing Revisited: Clarifying New Directions for Causal Inference and Generalization in Case Study Methodology

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Alexander George and Andrew Bennett's *Case Studies and Theory Development in the Social Sciences* (2005) and John Gerring's *Case Study Research: Principles and Practices* (2007) are among the best examples of the dramatic increase in methodological reflection that we have witnessed in recent years in case study research. In our elaboration, we will concentrate on what we perceive as problems and weaknesses of the two books. It is important to stress our conviction that both have contributed tremendously to the improvement of the understanding of case study research (not only for the development of the typology which we will present below) and deserve a central place in every case study methodology course (which they actually have in the courses we teach). The review itself is set up in a manner that reflects our own work on doing (comparative) case study research. We first sketch our own typology of styles of case study research, and then discuss the two books with reference to this typology.

We believe that it makes sense to differentiate between three styles of case study research. We labeled the three styles according to their main technique to generate "inference" from concrete empirical observations to abstract explanatory propositions for the *cases under study*: The first style is accounting for *co-variance* among independent and dependent variables. The second one is *tracing processes* in order to identify causal mechanisms on a lower level of analysis, or in order to identify causal configurations based on complex in-teractions and/or necessary context factors. Process tracing involves stressing the temporal unfolding of causality, and it is based on a holistic ontology in which the basic unit of analysis is not an individual variable, but a multi-level model or a configuration of densely linked causal factors. The third style is intensively reflecting on the (non) *congruence* between a broad range of concrete expectations, which can be deduced from coherent abstract concepts (theories) and empirical findings. The latter

is based on an understanding of theory as an interpretative framework (without the radical constructivist assumption that theory fully determines our empirical findings). The relevance of theoretical frameworks can be tested empirically by checking how many coherent meanings they can generate for understanding and explaining specific cases.

The three styles are not exclusive categories. Especially the last style has areas where it overlaps with the first two. Nevertheless, they vary strongly in the emphasis on what the necessary preconditions are to draw valid conclusions for the cases under study (see Table 1).

The differences are even more pronounced if we consider the understanding and direction of *generalization*—understood as drawing conclusions *beyond the cases under study*—which logically corresponds to the three techniques of drawing inferences (see Table 2). The co-variance style strives for "statistical generalization" (Yin 2003: 10): drawing conclusions from the findings of cases studied about the average contribution of a causal variable in explaining an outcome within a wider population of similar cases. The findings from process tracing are not used to draw conclusions for a population of cases but for a set of potential causal configurations or for multi-level causal models. The function of case studies here is mainly to show exactly whether and how a specific configuration of causal factors (potentially including context factors) or whether and how a causal mechanism leads to a specific outcome. Additional case studies would not strive to prove that this causal configuration also works within other cases, but they would try to find out whether other combinations of causal factors can also lead to the same or a similar outcome. We followed George and Bennett's labeling and called it "contingent generalization." We call the logic of generalization which corresponds to the congruence analysis "*abstraction*." Here, our emphasis is not on drawing conclusions from the "specific" (co-variation within the studied cases) to the "universal" (covering lawlike proposition for a specified population), but from the reality of "concretes" (observations) to the relevance of "abstracts" (concepts, theories and paradigms). The orientation is not horizontal (as in the co-variational approach), but towards "the vertical organization of knowledge" (Sartori 1984: 44). The various understandings and directions of generalization lead to different preconditions and consequences for case and/or theory selection (see Table 2; further elaborations can be found in Blatter and Blume 2008).

Based on this brief sketch of our typology specifying three styles of case study methodology, we now turn to the two books which have already become the authoritative sources for doing and teaching case studies in Political Science and beyond.

Although it is the more recent book, we turn first to John Gerring's *Case Study Research: Principles and Practices* because it is much easier to categorize, describe and evaluate. It represents a highly sophisticated and impressively clear-cut and consistent co-variational approach to case study research. Gerring develops his definitions and suggestions for making inferences within case study research on the basis of an "ex-

Table 1: Different Ways to Draw Inferences

Inference	Description	Preconditions
Co-Variation	<p>Concrete Observations: Co-variation (over time or space) among indicators of the dependent variable (Y) and indicators of an independent variable (X)</p> <p>Abstract Conclusions: X has a causal effect on Y</p>	<ol style="list-style-type: none"> 1. Control of other variables 2. Theoretically deduced hypothesis for causal direction
Causal Process Tracing	<p>Concrete Observations: Temporal unfolding of actions and events, traces of “motivations” (or other lower-level mechanisms), evidence of (complex) interactions between causal factors, and/or information about restricting/catalyzing contexts, and detailed features of a specific outcome</p> <p>Abstract Conclusions: Actual working of a causal mechanism/ actual interaction between the elements of a causal configuration</p>	<ol style="list-style-type: none"> 1. “Smoking-gun” observations 2. A full “storyline” with “density” and “depth” and a fine-grained picture of events within their contexts
Congruence Analysis	<p>Concrete Observations: (Mis)matches between empirical findings and concrete expectations deduced from core elements of theories: e.g., central actors and structures, traces of motivational foundation of (inter)action, specific features of X and Y, co-variance among indicators of X and Y</p> <p>Abstract Conclusions: Relevance/relative strength of theories to explain/understand the case(s)</p>	<ol style="list-style-type: none"> 1. Plurality of full-fledged and coherent theories from which concrete expectations can be deduced 2. Plurality and diversity of available observations

perimental template.” The logical structure of the book and the stringent line of argumentation make it easily accessible and lead to systematic definitions, discussions of major issues and recommendations. We would like to point to what we see as an inconsistency within this book and provide a first argument for why we think that most case study researchers might not whole-heartedly embrace Gerring’s co-variational style (a second one is given at the end of the review).

The insight which we can draw from the largest and most impressive chapter in the book, the one on case selection (co-authored by Jason Seawright), is somewhat contradictory to other parts of the book in respect to the relevance of cross-case comparison in the case study proper. Gerring and Seawright base their chapter on case selection on the assumption that the selected cases in case study research represent samples of the overall population of cases. From this background they discuss the following nine case study types: typical, diverse, extreme, deviant, influential, crucial, pathway, most-similar, and most-different. Selecting cases on the basis of prior statistical analysis is clearly preferred. This goes so far that in discussing the selection of most-similar cases, the authors ignore prior literature on “most similar systems designs” (Przeworski and Teune 1970), which does not correspond to this approach because the logic of case selection is not based on the prior location of the cases within a population. In giving the term “most-similar” an entirely new twist, Gerring and Seawright devote most of their attention to statistical “matching” techniques to select those pairs of cases which are “most-similar” within the population.

At the end, however, it is not clear how much of the causal

inference drawn from the in-depth analysis of the two cases—which were selected on the basis of these matching techniques—is based on cross-case comparison or on within-case analysis.¹ If the causal inferences drawn for the cases under study rely only on within-case analysis, cross-case comparison within the case study proper ceases to play a substantial role within the co-variational template. This is due to the fact that the authors dismiss the most-different cases technique and that all other techniques are geared to select only *one* case. In consequence, Gerring and Seawright in fact challenge an assumption that has become (implicitly or explicitly) commonsensical, especially in comparative politics: the assumption that analyzing and comparing a few cases in-depth is better than analyzing one case. This leaves us with the following puzzle: If we want to generalize towards a population and if—given this goal—the cross-case comparison must include (a representative sample of) the entire population, how does this correspond to the position presented in the book that “spatial comparison” (comparison of patterns of co-variation among a few cases) is a major element of generating internal validity? The part on “spatial comparison” is the shortest section within Chapter 6 (pp. 165-6). Whether a matter of accident or not, we take the fact as support for our following conclusion.

For us, the core message from Gerring’s book (and this corresponds to George and Bennett as well)² is the insight that the comparison of a few (from two until about six) cases does not provide much leverage for drawing causal inferences. Instead, the combination of a large-N-study and in-depth analysis within single cases, which are selected on the basis

Table 2: Different Understandings and Directions of Generalization

Understanding	Direction of Generalization	Preconditions and Consequences for Case/Theory Selection
Statistical Generalization	Drawing conclusions about the strength of a causal variable from specific cases to the wider population	<ol style="list-style-type: none"> 1. Specification and justification of the boundary of the wider population of similar cases 2. Selecting cases according to their statistical location within the wider population (e.g., typical, diverse)
Contingent Generalization	Drawing conclusions from identified causal configuration (with evidence for the links and interactions between causal factors) to the wider set of potentially possible configurations OR Drawing conclusions from traces of causal mechanisms for the accuracy and consistency of multi-level causal models	<ol style="list-style-type: none"> 1. Specification and justification of the wider set of potential causal configurations 2. Selecting cases according to their preliminary classification into theoretically interesting types of causal configurations OR <ol style="list-style-type: none"> 1. Specification and justification of multi-level models with generic causal mechanisms 2. Selecting cases according to “accessibility” and “familiarity” in order to get as close as possible to the unobservable causal mechanisms
Abstraction	Drawing conclusions from adequacy for the empirical case(s) to the relevance/relative strength of theories within the broader scientific discourse	<ol style="list-style-type: none"> 1. Specification and justification of the range of theories which are applied 2. Selecting cases according to their “likeliness” for the dominant theory

of this large-N-study, is much more productive. Accordingly, the strength of Gerring’s book lies less in providing helpful advice for doing case studies proper but more in his profound discussion on how to embed case studies in large-N-studies.³

Overall, we predict that Gerring’s book will be received by case study researchers with some skepticism. Conceptually, the co-variational style represents what Peter Hall (2006: 26) aptly described as “the statistical method writ small” and practically, it confines case studies to a secondary place: beyond theory development, case studies are only seen as conducive for gaining causal inference in combination with, and after, large-N studies. The combination of large-N and small-N studies might well be a fruitful endeavor, although there are some first cautious voices (e.g., Rohlfing 2008). But there is the danger that we miss the real qualities of case studies and do not develop the adequate methodological advice if we conceive case study research only within the confines of co-variational analysis.

In comparison to Gerring’s book, Alexander George and Andrew Bennett’s *Case Studies and Theory Development in the Social Sciences* is much broader in its understanding of what case studies are, and it puts a strong emphasis on causal process tracing as the heart of the case study endeavor. The richness of the book in terms of conceptual breadth and philosophical depth comes with a price, though. First, the structure of the book is surprising: Advice on how to do case studies is given before basic foundations (scientific realism as epistemo-

logical base and the goal to generate policy-relevant theory) and techniques (controlled comparison, congruence method, and process tracing) are fully spelled out. Second, their openness to different styles and techniques makes the line of argument and the practical advice less stringent and clear-cut in comparison to Gerring. We would like to emphasize three points: First, it is important to realize that by embedding within-case analysis into “typological theorizing,” George and Bennett also advocate a specific kind of multi-method approach, one which can be made even more productive if we combine it with (fs)QCA. Second, we think that in order to get a more precise understanding of what the goals of process tracing are, we would need to differentiate more clearly between causal mechanisms and causal configurations. Finally, we argue that the congruence method which has been introduced by George and Bennett holds more potential if we take it seriously as a technique of “causal interpretation.”

In a similar way, as Gerring argues for the selection of cases in accordance with their location in a larger population, George and Bennett (2005: 255-57) describe the selection of cases according to their (assumed) location in a preliminary set of possible causal configurations. They call such a set of causal configurations a “typological theory.” George and Bennett first present an inductive approach of using the insights of process tracing within a few cases as a foundation for creating typological theories. Detailed descriptive analysis and process tracing within a few cases can help scholars to arrive at a differen-

tiated typology of the dependent variable and/or to different combinations of causal factors which lead to specific outcomes. These findings can be used to create building blocks of a typological theory (pp. 241-43). Nevertheless, the subsequent elaboration on the deductive use of typological theories—illustrated by the extended example of Andrew Bennett’s work (pp. 255-60)—show that in-depth case studies are especially valuable for their testing and refinement. In this deductive style, case studies are conducted after theoretical knowledge has been used to create a set of possible causal configurations and first empirical investigations have provided preliminary evidence for the location of specific cases in this set.

We believe that in order to understand more clearly what we are searching for when we are tracing causal processes, we should clearly and conceptually differentiate between causal configurations and causal mechanisms. The term causal configuration should be used if we start from the assumption that there exist intense links and/or complex interactions between various factors in the production process of an outcome (Ragin 2000: 64-119). Individual elements of a causal configuration are conceptualized on the same level of analysis. Such an ontological starting point has a strong affinity and can easily be integrated with the methodological reflections on necessary and sufficient conditions and set-theoretical logic (see Goertz and Starr 2003; Ragin 2008). In consequence, the concept of causal configuration includes at least the three following ideas: First, interaction effects, in the sense that causal factors do not simply add up together to reach the causal power to produce an outcome, but that their co-existence modifies (e.g., accelerates) their individual causal power. Second, specific causal factors (sometimes misleadingly called causal mechanisms) work only within specific contexts.⁴ A third idea is the relevance of causal chains (Goertz and Levy 2007: 23-29). Note that the latter two ideas translate “dense links” into the asymmetrically deterministic concepts of sufficient and necessary conditions, whereas the first does not.

In their chapter on typological theorizing, George and Bennett see process-tracing as an instrument to identify the interaction effects between the elements of a causal configuration (pp. 244-55). Surprisingly, George and Bennett do not connect their “typological theories” closer to the work of Charles Ragin, although the ontological assumptions are the same (configurational causality and the importance of equifinality). (fs)QCA, however, could complement the purely theory-driven creation and reduction of the “property spaces” of typologies. In a first empirical step, the co-existence of specific values of variables can be identified and could provide preliminary evidence of the relevance of specific causal configurations based on a more superficial study of a middle-range number of cases. Functionally, (fs)QCA is an addition to the purely theoretical creation and reduction of the set of possible causal configurations. In-depth case studies would still be necessary to verify empirically whether and how two or more co-existing independent factors interact for the production of a specific outcome.

George and Bennett define “causal mechanisms” explicitly “as ultimately unobservable physical, social, or psycho-

logical processes through which agents with causal capacities operate, but only in specific contexts or conditions, to transfer energy, information, or matter to other entities” (p. 137). This definition reflects the attempts of the authors to be compatible with very different theoretical orientations (expressed in the enumerations like “physical, social, or psychological” and “energy, information, or matter”). Nevertheless, it clearly signals a preference for micro-level theories (because of the term “agents”). Furthermore, and much more problematic from our point of view, it stresses that contextual factors act as necessary conditions for the actual working of causal mechanisms. Bennett sheds more light on his understanding of causal mechanisms in a later contribution and provides two quite different characteristics of causal mechanisms by comparing an explanation by reference to a law with an explanation by reference to a mechanism: First, mechanisms work in a more specified context, a narrower scope, than laws. Second, “explanations via law more readily admit ‘as if’ assumptions about processes or mechanisms, whereas mechanism-based explanations admit to being subject to refutation if it can be demonstrated, often at a lower level of analysis or finer degree of detail, that the posited mechanism was not in operation” (Bennett 2006: 47). Unfortunately, this conflates again the difference between causal configurations and causal mechanisms, and is still undetermined (“often”) in respect to what we see as the core of causal mechanisms—the need to go on to a lower level of analysis for tracing these mechanisms.

We believe we get to a more exclusionary and therefore more precise definition of causal mechanisms if we do not confound them with causal configurations, and stress one core aspect: In order to trace causal mechanisms, we have to include lower levels of analysis in comparison to the level of analysis on which we measure inputs and outcomes of a causal process. Good co-variational work sheds light on the “black box” between the independent and the dependent variable by deducing and explicating (formal) models which logically connect the involved variables in such a way that we can infer from “dataset observations” whether the theoretical mechanisms at the heart of the model were working or not. The most sophisticated of these models are based on the combination of “situational mechanisms,” “action-formation mechanisms,” and “transformational mechanisms” (Hedström and Swedberg 1998: 22). The second type of these mechanisms is based on behavioral micro-foundations (not necessarily the ones of rational choice theory); the first and the third types represent mechanisms linking different levels of analysis. For us, the fundamental difference between a co-variational approach and a process-tracing approach is that the latter attempts to find traces of these mechanisms below the original level of analysis. Usually, this means to search for information about the perceptions and motivations of actors. Such a search for empirical traces of mechanisms beyond “dataset observations” is especially warranted if the models and mechanisms are not fully deterministic (e.g., Elster 1998).

Such a differentiation between causal configurations and causal mechanisms allows us to separate two goals of causal process tracing: revealing the complex interactions and deter-

ministic dependencies which produce “dense” links among causal factors, and taking a “deep”⁵ look into the “machinery” of causal connections on a lower level of analysis.⁶ We would like to stress that a process-tracing approach is looking for evidence about the working of causal configurations and causal mechanisms over time—in contrast to (fs)QCA, for example. If we take the definition of terms seriously, we should use “process tracing” or “process observations” only when the observations are documenting temporal unfolding of actions and events. This is often overlooked by those who want to press all alternatives to the co-variational template under the heading of “process tracing.” The term “process” refers to the object of the observation and not to the process through which the scholar tries to arrive at valid conclusions (through some kind of Bayesian updating, for example).

This leads us to our third and final point on George and Bennett’s book. Since the goal of this essay is to review two eminent books, and not to present our own take on case study methodology, we cannot fully lay out our understanding of what we call “congruence analysis” (Blatter and Blume 2008). The labeling and development of this type were triggered by George and Bennett’s chapter on the “congruence method”: “[T]he method of controlled comparison requires the investigator to find two similar cases in every respect but one. Since this requirement is difficult to meet, an alternative approach is often needed—one that does not attempt...to achieve the functional equivalent of an experiment. The alternative we propose is the within-case method of causal interpretation, which may include congruence, process-tracing, or both” (2005: 181). Unfortunately, in the following, they restrict their congruence method to an endeavor for assessing the ability of a theory “to explain or predict the outcome in a particular case” (2005: 181), and they never take up again the term “causal interpretation.” In line with Peter Hall (2006), we think that a theory is more than a hypothesis and that we are able to deduce more predictions (expectations) from a good theory than just the outcome. Beyond Hall, we pretend that the “thickness” of case-studies (the plurality and diversity of observations, plus the intensity of interpretative reflections of the relationship between concrete observations and abstract concepts) makes it not only possible to reduce the “degrees of freedom” problem, but allows for discussion of the relevance of more diverse and/or more abstract theories in respect to their contribution to the understanding/explanation of the case(s) under study (Blatter and Blume 2008). The findings within the case(s) under investigation, in turn, can be used as evidence in a broader spectrum of theoretical discourses.

“Thick” case studies are therefore very conducive for drawing conclusions towards the “broader” theoretical discourse. The first element of our definition of “thickness” makes case studies also the best research design for getting “deeper” insight into the social world and to move the border “between the observable world and the unobservable ontological level where causal mechanisms reside” (George and Bennett 2005: 143). Only, if we strive for generalizations towards the “wider” population of similar cases, then case studies will have to play a secondary role and we run into the “commonsensical...exten-

sity/intensity trade-off” (Gerring 2007: 48).

The philosophical depth and conceptual richness of George and Bennett’s book has been a great inspiration for the discipline’s and our thinking about case study research. John Gerring’s book shows convincingly that a rather narrow but focused perspective is a precondition for clear, consistent, and therefore practically helpful advice for actually doing case-study research. There is still a lot to do for those who believe that it is necessary and possible to come up with similar clear-cut advice for those kinds of case study research going beyond the co-variational template.

Notes

¹ The same applies to their discussion of “diverse cases” (98-99).

² Bennett (2006: 46) stresses that “one goal of our book was to correct for earlier qualitative literature’s overemphasis on comparative methods relative to within-case methods of analysis.” And when Bennett acknowledges the value of comparative methods, he stresses the combination of within-case analysis and cross-case analysis in “typological theorizing” which is something quite different from the classic comparative (small-N) method.

³ The short chapter on “causal process tracing” and the separate chapter on “single-outcome studies” at the very end of the book only moderately mitigate such an impression and rather contribute to it.

⁴ Logically, such a context factor (sometimes also referred to as scope condition) is nothing else than a necessary condition.

⁵ Our understanding of “depth” has nothing to do with what George and Bennett (2005: 185-86) call “causal depth.” George and Bennett understand it to be an earlier factor in a causal chain, a factor which has more causal depth than the later. In line with Goertz and Levy (2007: 29), we think it is more adequate to call such an earlier factor “fundamental” (but only within a sufficient-condition causal chain).

⁶ Neither causal configurations nor causal mechanisms are referring to explanations which take more factors (variables) into account. Adding further (autonomous) variables reduces the scope for generalization, but it is not based on the assumption that there exists a dense or deep link between causal factors. Therefore, notions like “finer degree of detail” (Bennett 2006: 47) or “intervening variables” (Seawright and Collier 2004: 277) are more confusing than helpful if one wants to get a clearer understanding of causal mechanisms.

References

- Bennett, Andrew. 2006. “Advancing the Dialogue on Qualitative Methods.” *Qualitative Methods. Newsletter of the American Political Science Association Organized Section for Qualitative Research* 4:1 (Spring), 45-8.
- Blatter, Joachim and Till Blume. 2008. “In Search of Co-variance, Causal Mechanisms or Congruence? Towards a Plural Understanding of Case Studies.” *Swiss Political Science Review* 14:2, 315-55.
- Elster, Jon. 1998. “A Plea for Mechanisms.” in *Social Mechanisms. An Analytical Approach to Social Theory*. Peter Hedström and Richard Swedberg, eds. Cambridge: Cambridge University Press.
- George, Alexander L. and Andrew Bennett. 2005. *Case Studies and Theory Development in the Social Sciences*. Cambridge: MIT Press.
- Gerring, John. 2007. *Case Study Research: Principles and Practices*. Cambridge: Cambridge University Press.
- Goertz, Gary, and Jack S. Levy. 2007. “Causal Explanation, Necessary Conditions, and Case Studies.” in *Explaining War and*

- Peace: Case Studies and Necessary Condition Counterfactuals.* Gary Goertz and Jack S. Levy, eds. London: Routledge.
- Goertz, Gary and Harvey Starr, eds. 2003. *Necessary Conditions: Theory, Methodology, and Applications.* Lanham, MD: Rowman & Littlefield.
- Hall, Peter A. 2006. "Systematic Process Analysis: When and How to Use it." *European Management Review* 3: 24-31.
- Hedström, Peter and R. Swedberg. 1998. "Social Mechanisms: An Introductory Essay." in *Social Mechanisms. An Analytical Approach to Social Theory.* Peter Hedström and Richard Swedberg, eds. Cambridge: Cambridge University Press.
- Przeworski, Adam and Henry Teune. 1970. *The Logic of Comparative Social Inquiry.* New York: Wiley InterScience.
- Ragin, Charles C. 2000. *Fuzzy-Set Social Science.* Chicago: Chicago University Press.
- Ragin, Charles C. 2008. *Redesigning Social Inquiry: Fuzzy Sets and Beyond.* Chicago: University of Chicago Press.
- Rohlfing, Ingo. 2008. "What You See and What You Get: Pitfalls and Principles of Nested Analysis in Comparative Research." *Comparative Political Studies* (forthcoming).
- Sartori, Giovanni. 1984. *Social Science Concepts. A Systematic Analysis.* Beverly Hills: Sage.
- Seawright, Jason and David Collier. 2004. "Glossary." in *Rethinking Social Inquiry: Diverse Tools, Shared Standards.* Henry E. Brady and David Collier, eds. Lanham, MD: Rowman & Littlefield.
- Yin, Robert K. 2003. *Case Study Research: Design and Methods.* London: Sage.

Reflection on the Methods of Political Science on Both Sides of the Atlantic

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For its Ninth Congress in Toulouse (5–7 September 2007), the French Political Science Association (AFSP) invited the American Political Science Association (APSA) to hold a joint "table ronde"¹ comparing methods on both sides of the Atlantic. It took the form of three consecutive panels, devoted to qualitative and quantitative approaches, to the dimension of time, and to contextual and inference problems. During three days, 18 papers were presented, over 60 participants attended, and contrasting ways to validate theories and models were discussed at length, illustrated by concrete research examples. The objective here is less to sum up all that was said than to outline the main differences and convergences of our methodologies.

The Quali-Quanti Debate

It is a fact that in France quantitative approaches are less developed than in the States, where even qualitativists have received a basic foundation in statistics and know how to read an equation, a regression line, and a factor analysis. In France one tends to give more importance to the historical and philosophical positioning of a problem, training in statistical methods is offered by fewer institutions, rational choice

models are not popular (Billordo 2005b; Billordo and Dumitru 2006), and quantitative analysis forms a small minority of the articles published in the main reviews (one third of all articles published in *French Political Science Review* between 1970 and 2004, according to Billordo 2005a). The borders between quali and quanti approaches was the issue addressed by the first panel. Where the Americans tended to see distinct epistemologies, different conceptions of causality, "two cultures" (Mahoney and Goertz 2006), the French on the contrary insisted on the necessity to go beyond this opposition, questioning what basically differentiates the two approaches. Is it the fact of counting, opposing those who count to those who give account—in French "ceux qui comptent" vs. "ceux qui racontent"? Is it a problem of arithmetic, mathematics, statistics? Is it the number of cases studied, small or big—n? Are survey research and comprehensive interviews, case- and variable-oriented approaches so antagonistic? Where should one put the QCA (Qualitative Comparative Approach) developed by Charles Ragin, based on Boolean logics, which does not actually count but puts a phenomenon into an equation according to the presence or absence of certain elements and the way they combine?

On the whole, the divide between qualitative and quantitative methods seems far more institutionalized in the States, where it is embodied in distinct academic departments and recruitment procedures, and is represented by two different methodological standing groups in APSA. But precisely because the separation is less rigid in France, it seems more natural to combine the two approaches, as shown by most of the French papers for the table ronde. This could be an asset, at a time when all over Europe, mixed-methods designs, triangulation, and combining comprehensive and explicative approaches are becoming fashionable (Moses, Rihoux, and Kittel 2005).²

Assessing Time

The second panel explored the time dimension. The papers apprehended it in many different ways: time as period, as process, as event, as series of sequences, as interval, time as the present moment and time as the past and its memories. The advantages and limitations of several methods were compared with sophisticated models such as survival analysis, optimal matching analysis, and protest event analysis. But time is also the specific time of the interview or of the observation, when it takes place, how long it lasts, what relationship settles between interviewer and interviewee. Most participants insisted on the limits of the "one-shot" interview to grasp the subjects with their contradictions, their evolutions, and their interactions, for quantitative as well as for qualitative approaches.

Assessing Context

The third session enlarged the notion of context. At first we had in mind ecological analysis and the classical problems of inference. But some understood it also as the subjective context, the way people interviewed feel about their surroundings. Others dwelt on how experimentation can manipulate

the context in order to test the effect of the variables, in or out of the laboratory. Context was also taken in the sense of the scale of analysis selected, and the multiple levels—in this case local, national and European—at which on can grasp the relations between actors and the dynamic of their opinions, both in the instant and in the long run. Finally, the debate focused on the new types of context in constant transformation generated by the development of the Internet (blogs, mailing lists) and the challenge they represent to the traditional quali and quanti methods.

A Common Space of Discussion

Many questions were asked, many research tracks opened during these three days. If obvious differences appeared in the methods discussed, yet there also was a common space of discussion between qualitativists and quantitativists, which Mathieu Brugidou, chair of the last session, attempted to map in the following graph based on the six papers he discussed.

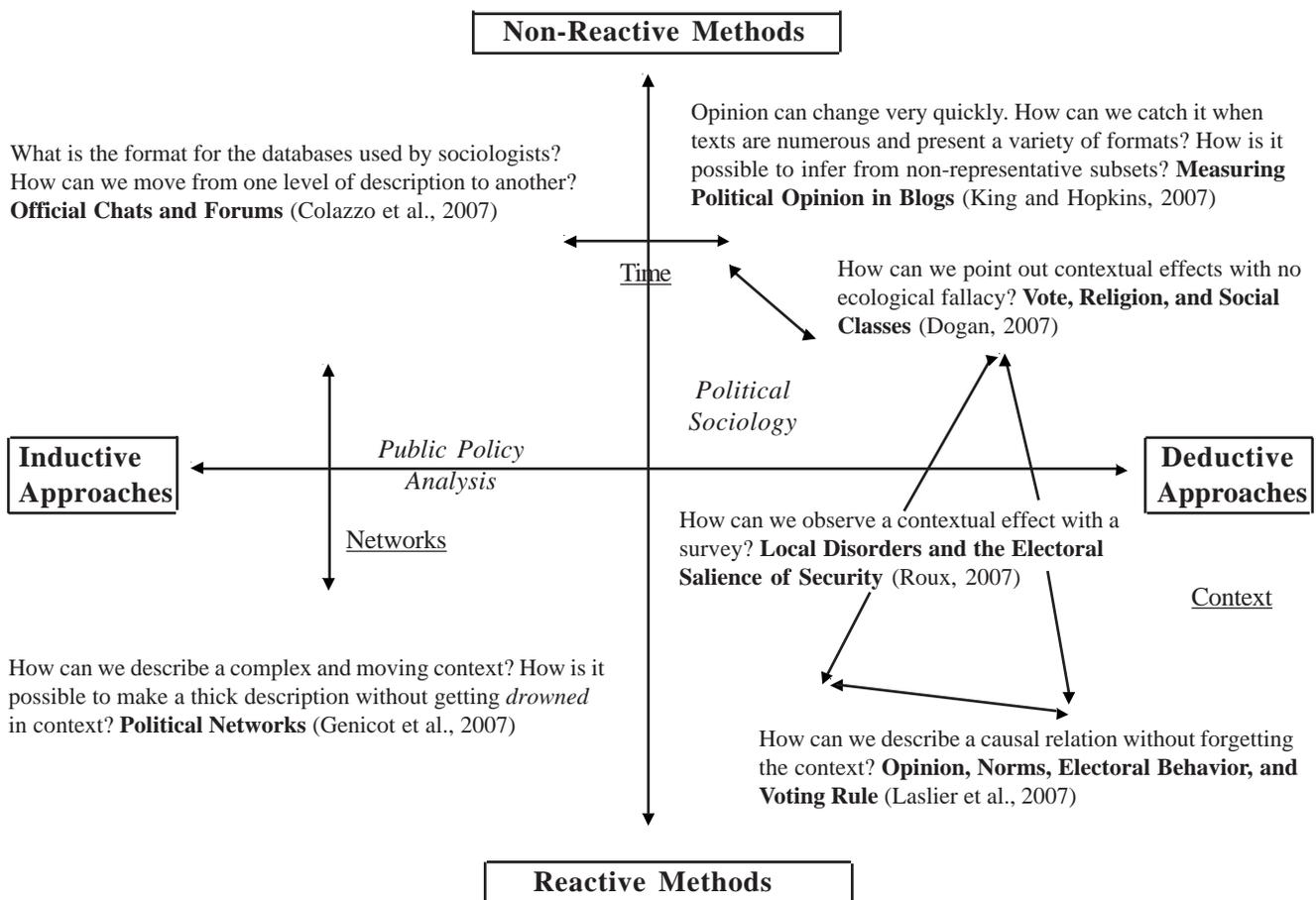
The vertical axis opposes inductive and deductive approaches, those which move from theories and hypotheses to their empirical validation and those which, by contrast, prefer to start by observation and immersion in the field and move up from there. The second axis opposes reactive to non-reactive methods. The former deal with tests, surveys, interviews, get

ting a reaction from the actors observed; the latter deal with a given object already there that they do not influence. For each paper is specified (in boldface) the topic and the methodological issue. The arrows show the possible lines of discussion connecting papers, the objects they have in common are underlined, and in italics appears the sub-discipline concerned.

To fully understand the graph one must go back to the papers, available on the AFSP's website. Yet even without doing so, it shows that the quali/quanti methodological divide is not the only, nor necessarily the most relevant, one.

The paper by Genicot et al., about public policy actors in Europe, is positioned in the reactive/inductive quadrant, lower left. It shares with Colazzo et al. (upper left), who study companies' forums and chats, a same object: networks, and a similar inductive approach, considering that the configuration of the network is not given before hand, it will emerge from the analysis. Yet Genicot and her colleagues have opted for a purely qualitative approach by interviews, while Colazzo et al. offer a quantitative approach of non-reactive data, email lists, to make sense of the evolving configuration of the networks. King and Hopkins, who follow the evolution of political opinions expressed in millions of blogs, share with Colazzo and his colleagues a common moving object, the Net, and the use of sophisticated statistical models. But they are in the upper

Figure 1: Inference, Context, New Approaches: A Common Space of Discussion?



right quadrant because they give preference to deductive methods, starting with a predefined categorisation of the political opinions they code. Dogan's paper, which offers an ecological analysis of votes, is in the same quadrant and faces with King and Hopkins the common problem of inference. But Dogan also shares a common preoccupation, the effect of context, with Roux, who is interested in the subjective perceptions of context by the voters, and Laslier and his colleagues, who perform electoral experimentations, artificially manipulating context, both situated in the lower right quadrant (deductive-reactive).

The Franco-American table ronde was but a first step to confront and exchange our methodological know-how, see how close and how different we are, and overcome the gap between so-called qualitative and quantitative research. We hope it will be followed by many others.

Notes

¹ Co-organized by Nonna Mayer (AFSP) and Andrew Appleton (APSA/French Politics Group). A special thanks to the chairs of APSA's two methodological sections, Janet Box-Steffensmeier and James Mahoney, who greeted me at their business meeting in APSA's 2006 Congress in Philadelphia and enthusiastically supported this project, and to the French Politics group whose mediation was essential. The table ronde's programme and paper summaries (in French and English) are available on AFSP's website, <http://www.congress-afsp.fr/>.

² See for instance the dynamic standing group on Political Methodology at ECPR (European Consortium for Political Research), chaired by Benoît Rihoux, Jonathon Moses, and Bernhard Kittel, and the workshop they propose at the coming ECPR session on "Methodological Pluralism? Consolidating Political Science Methodology" (Rennes, 11–16 April 2008).

References

- Billordo Libia. 2005a. "Publishing in French Political Science Journals: An Inventory of Methods and Sub-fields." *French Politics* 3:2, 178-86.
- Billordo Libia. 2005b. "Methods Training in French Political Science." *French Politics* 3:3, 352-57.
- Billordo, Libia and Adina Dumitru. 2006. "French Political Science: Institutional Structures in Teaching and Research." *French Politics* 4:1, 124-34.
- Colazzo, Dario, Francois-Xavier Dudouet, Iona Manolescu, Benjamin Nguyen, Pierre Senellart, and Antoine Vion. 2007. "Analysing Web Data-Bases: Towards New AI Inquiries." Presented at the Ninth Congress of the French Political Science Association, Toulouse, France.
- Dogan, Mattei. 2007. "France in Ten Sociological Slices: The Decilisation Method." Presented at the Ninth Congress of the French Political Science Association, Toulouse, France.
- Genicot, Genevieve, Delphine Deschaux-Beaume, and Severine Germain. 2007. "Multi-Level Studies: Quantity or Quality? The Rigorous Recipes of Three Dissertations in Progress." Presented at the Ninth Congress of the French Political Science Association, Toulouse, France.
- King, Gary and David Hopkins. 2007. "Extracting Systematic Social Science Meaning from Text." Presented at the Ninth Congress of the French Political Science Association, Toulouse, France.

- Laslier, Jean-Francois, Nicolas Sauger, and Karine Van der Straeten. 2007. "Experimental Methods in Political Science." Presented at the Ninth Congress of the French Political Science Association, Toulouse, France.
- Mahoney, James and Gary Goertz. 2006. "A Tale of Two Cultures: Contrasting Quantitative and Qualitative Research." *Political Analysis* 14:3, 227-49.
- Moses, Jonathon, Benoît Rihoux, and Bernhard Kittel. 2005. "Mapping Political Methodology: Reflections on a European Perspective." *European Political Science* 4, 55-68.
- Roux, Guillaume. 2007. "Local Context, Insecurity, Xenophobia and the Le Pen Vote: A Small Scale Quantitative Approach." Presented at the Ninth Congress of the French Political Science Association, Toulouse, France.

Book Notes

Book descriptions are excerpted from publisher's websites. If you would like to recommend a book to be included in this section, email Joshua C. Yesnowitz, the assistant editor of QM, at jcyesnow@bu.edu.

- Goertz, Gary and Amy G. Mazur, eds. 2008. *Politics, Gender, and Concepts: Theory and Methodology*. Cambridge: Cambridge University Press.

A critique of concepts has been central to feminist scholarship since its inception. However, while gender scholars have identified the analytical gaps in existing social science concepts, few have systematically mapped out a gendered approach to issues in political analysis and theory development. This volume addresses this important gap in the literature by exploring the methodology of concept construction and critique, which is a crucial step to disciplined empirical analysis, research design, causal explanations, and testing hypotheses. Leading gender and politics scholars use a common framework to discuss methodological issues in some of the core concepts of feminist research in political science, including representation, democracy, welfare state governance, and political participation. This is an invaluable work for researchers and students in women's studies and political science.

- Gschwend, Thomas and Frank Schimmelfennig, eds. 2007. *Research Design in Political Science: How to Practice what they Preach*. London: Palgrave.

While research designs can be distinguished along various dimensions, we all face the same set of core research design issues. What is a relevant research problem? How can I improve concepts and measurements? Which and how many variables and cases should I select? How can I evaluate rival explanations and which theoretical conclusions can I draw from my research? Which evidence would lead me to reject and reformulate my initial theory? This volume was written to help advanced students of political science and their neighboring fields to think about these issues and come up with practical solutions for their own research. It turns out that the distinction between qualitative and quantitative research is often inadequate. Some solutions to research design problems are common to both types of research; others cross-cut the traditional qualitative-quantitative divide. Moreover, every solution has its strengths and weaknesses and, therefore, involves substantial trade-offs along the way.

Klotz, Audie and Deepa Prakash, eds. 2008. *Qualitative Methods in International Relations: A Pluralist Guide*. London: Palgrave.

The main terrain of methodological disputes in the social sciences is empirical research, including the delineation of legitimate research questions, allocation of funding for projects, and employment in the profession. Yet we still lack practical answers to one of the most basic questions: How should researchers interpret meanings? The contributors take seriously the goals of both post-modernist and positivist researchers, as they offer detailed guidance on how to apply specific tools of analysis and how to circumvent their inherent limitations. Readers will understand what is at stake in selecting from discourse, speech acts, and semiotics—or even content analysis. Researchers will be able to decide when to combine tools drawn from different analytical traditions—perhaps discourse analysis to inform the construction of a dictionary for context-sensitive computerized coding. The results will be deeper interdisciplinary understanding and better research.

Ragin, Charles C. 2008. *Redesigning Social Inquiry: Fuzzy Sets and Beyond*. Chicago: University of Chicago Press.

In what has become normal social science, researchers view their central task as one of assessing the relative importance of causal variables drawn from competing theories, where each variable is thought to have an autonomous or independent capacity to influence the level or probability of the outcome. This way of conducting social research is the default procedure in the social sciences today—one that researchers fall back on time and time again, often for lack of knowledge of any alternative. In *Redesigning Social Inquiry: Fuzzy Sets and Beyond*, Charles Ragin challenges major aspects of the conventional template for social research. Instead of using variables to characterize cases, he argues that social scientists should use well-calibrated fuzzy sets. Instead of grounding analyses of cross-case patterns in the examination of correlations between variables, researchers should study set-theoretic relationships. Instead of calculating the net effects of independent variables, researchers should think in terms of causal recipes, and assess the impact of combinations of causal conditions. Instead of assuming causal linearity and additivity, researchers should compare cases as configurations and thereby allow maximum causal complexity. Through a series of contrasts between fuzzy set analysis and conventional quantitative research, Charles Ragin shows how researchers can use set-theoretic methods to strengthen the connection between case-oriented knowledge and the study of cross-case patterns. This book goes beyond a critical analysis of conventional quantitative research to offer a groundbreaking alternative that will revitalize social inquiry.

Article Notes

Capoccia, Giovanni and R. Daniel Kelemen. 2007. “The Study of Critical Junctures: Theory, Narrative, and Counterfactuals in Historical Institutionalism.” *World Politics* 59:3 (April), 341-69.

The causal logic behind many arguments in historical institutionalism emphasizes the enduring impact of choices made during critical junctures in history. These choices close off alternative options and lead to the establishment of institutions that generate self-reinforcing path-dependent processes. Despite the theoretical and practical importance of critical junctures, however, analyses of path dependence

often devote little attention to them. The article reconstructs the concept of critical junctures, delimits its range of application, and provides methodological guidance for its use in historical institutional analyses. Contingency is the key characteristic of critical junctures, and counterfactual reasoning and narrative methods are necessary to analyze contingent factors and their impact. Finally, the authors address specific issues relevant to both cross-sectional and longitudinal comparisons of critical junctures.

Kurki, Milja. 2006. “Causes of a Divided Discipline: Rethinking the Concept of Cause in International Relations Theory.” *Review of International Studies* 32:2 (April), 189-216.

During the last decades “causation” has been a deeply divisive concept in International Relations (IR) theory. While the positivist mainstream has extolled the virtues of causal analysis, many post-positivist theorists have rejected the aims and methods of causal explanation in favour of “constitutive” theorising. It is argued here that the debates on causation in IR have been misleading in that they have been premised on, and have helped to reify, a rather narrow empiricist understanding of causal analysis. It is suggested that in order to move IR theorising forward we need to *deepen* and *broaden* our understandings of the concept of cause. Thereby, we can radically reinterpret the causal-constitutive theory divide in IR, as well as redirect the study of world politics towards more constructive multi-causal and complexity-sensitive analyses.

Levy, Jack S. 2008. “Case Studies: Types, Designs, and Logics of Inference.” *Conflict Management and Peace Science* 25:1 (March), 1-18.

I focus on the role of case studies in developing causal explanations. I distinguish between the theoretical purposes of case studies and the case selection strategies or research designs used to advance those objectives. I construct a typology of case studies based on their purposes: idiographic (inductive and theory-guided), hypothesis-generating, hypothesis-testing, and plausibility probe case studies. I then examine different case study research designs, including comparable cases, most and least likely cases, deviant cases, and process tracing, with attention to their different purposes and logics of inference. I address the issue of selection bias and the “single logic” debate, and I emphasize the utility of multi-method research.

Mahoney, James. 2008. “Toward a Unified Theory of Causality.” *Comparative Political Studies* 41:4/5 (April/May), 412-36.

In comparative research, analysts conceptualize causation in contrasting ways when they pursue explanation in particular cases (case-oriented research) versus large populations (population-oriented research). With case-oriented research, they understand causation in terms of necessary, sufficient, INUS, and SUIN causes. With population-oriented research, by contrast, they understand causation as mean causal effects. This article explores whether it is possible to translate the kind of causal language that is used in case-oriented research into the kind of causal language that is used in population-oriented research (and vice versa). The article suggests that such translation is possible, because certain types of INUS causes manifest themselves as variables that exhibit partial effects when studied in population-oriented research. The article concludes that the conception of causation adopted in case-oriented research is appropriate for the population level, whereas the conception of causation used in population-oriented research is valuable for making predictions in the face of uncertainty.

Rothman, Steven B. 2007. "Understanding Data Quality through Reliability: A Comparison of Data Reliability Assessment in Three International Relations Datasets." *International Studies Review* 9:3 (Fall), 437-56.

Although recent data creation efforts in international relations have begun to focus on issues of reliability and validity more explicitly than previously, current efforts still contain significant problems. This essay focuses on three recent data generation projects that study international relations (the ICOW, ATOP, and River Treaty datasets) and shows the successes and failures of each in assessing reliability when generating data from qualitative evidence. All three datasets attempt to generate reliable data, document the procedures used, and present indications of data reliability. However, their efforts face problems when assessing the reliability of their case selection variables, in the development of reliability indicators, and in the presentation of reliability statistics. In addition to evaluating these recent efforts to generate large-N databases, this essay clarifies the difference between generating data from qualitative and quantitative evidence, explains the importance of reliability when coding qualitative evidence, and provides ways to improve the assessment of the quality of one's data.

Announcements

Institute for Qualitative and Multi-Method Research to be co-hosted by the Moynihan Institute of Global Affairs and the Maxwell School at Syracuse University

Since 2002, the Institute for Qualitative and Multi-Method Research (IQMR) has been held at Arizona State University. During that time, the institutes have collectively trained 630 graduate students and junior faculty in qualitative research methods. Beginning with the 2009 iteration of the institute, IQMR will be moving to Syracuse University, and shifting from January to June–July.

We anticipate that this move will bring several advantages. First, the Moynihan Institute and the Maxwell School are providing substantial long-term support for further broadening IQMR's activities. Second, by moving to a June–July slot, we will be avoiding the January scheduling difficulties faced by attendees from universities on the quarter system. Third, IQMR was previously bracketed by New Year's Day and the beginning of the ASU spring semester, limiting us to two weeks. By moving to the summer, we will be able to have a more flexible timetable. Fourth, in recent years we have seen a very substantial increase in participation by attendees from European universities, and this move dramatically reduces the travel distance for those students. Finally, the move to a venue with the standing of the Maxwell School is further recognition of IQMR's growing national and international prominence.

Although our location and timing are changing, some things will be staying the same. There will still be two ways to attend the institute: either through nomination by a member of the Consortium for Qualitative Research Methods, or by successfully competing for an open-pool slot. We expect to issue the call for open-pool applicants to the 2009 institute in September 2008. Other aspects of the institute—including the high quality of its curriculum and faculty—will carry over to the new venue.

Over the years, IQMR has been very fortunate in being able to rely on the active support of several constituencies. Departments and research centers have invested considerable resources in membership in CQRM, and in nominating attendees to the institutes. IQMR faculty have freely given their time and expertise, often for several

days in a row. ASU has been a generous co-host. The National Science Foundation has supported the institute with multiple grants. And, of course, the 600-plus attendees have worked extraordinarily hard to make the most of their time at IQMR. We are looking forward to the next phase, IQMR co-hosted by Moynihan and Maxwell, with great excitement.

Andrew Bennett
David Collier
Colin Elman

APSA Short Courses Created (or Co-Organized) by Division 46: Qualitative Methods Wednesday, August 27, 2008, Boston, MA

Short Course 1: Multi-Method Research

Co-Sponsors: Qualitative and Multi-Method Research and International History and Politics Sections

Contact Person: Professor Colin Elman, CQRM, Moynihan Institute of Global Affairs, Maxwell School, 346 Eggers Hall, Syracuse University, Syracuse, New York 13244-1020

Email: consortium@asu.edu

Registration: Faculty: \$10; Graduate students: \$5. Check should be made out to CQRM and sent to Professor Colin Elman

Time: 9:00am–1:00pm

Location: On the premises of the APSA Annual Meeting

Instructors: David Collier, Henry E. Brady, and Neal Richardson, University of California, Berkeley; Jason Seawright, Northwestern University, Thad Dunning, Yale University

Attention has increasingly focused on how qualitative methods can be linked to other analytic tools, including large-N quantitative analysis and field experiments. To this end, methodologists have urged scholars to "nest" their case studies within a larger quantitative analysis (Lieberman; also Laitin), and to link quantitative dataset observations with qualitative causal-process observations (Brady, Collier, and Seawright).

Given that many political scientists are now convinced that good research necessarily employs multiple methodologies, how can different approaches be combined to maximize analytic leverage? How useful are the multi-method techniques noted above? What other procedures are available to political scientists? What are the pitfalls associated with multi-method approaches? Do they sometimes involve using multiple methodological tools poorly, rather than one tool with greater skill?

This short course will include discussions of most of the following topics: (a) The contribution of qualitative evidence to specifying statistical models; (b) Conceptual confusion and causal inference; (c) Regression matching designs and regression discontinuity designs; (d) Contextualized Comparison; (e) Conceptions of causation: common ground for qualitative and quantitative researchers; (f) Case selection in qualitative research; (g) Natural Experiments, instrumental variables, and the critical role of qualitative evidence.

Background Readings

Brady, Henry E., and David Collier, eds. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, MD: Rowman & Littlefield. See, for example, Brady, Collier, and Seawright, "Sources of Leverage in Causal Inference" (Ch. 13); and Tarrow, "Bridging the Quantitative-Qualitative Divide" (Ch. 10).
Brady, Henry E. 2008. "Causation and Explanation in Social Sciences." in *Oxford Handbook of Political Methodology*. Janet Box-

- Steffensmeier, Henry E. Brady, and David Collier, eds. Oxford: Oxford University Press, forthcoming.
- Collier, David and Colin Elman. 2008. "Qualitative and Multi-Method Research: Organizations, Publication, and Reflections on Integration." in *Oxford Handbook of Political Methodology*, forthcoming.
- Fearon, James D. and David D. Laitin. 2008. "Integrating Qualitative and Quantitative Methods." in *Oxford Handbook of Political Methodology*, forthcoming.
- Gerring, John. 2008. "Case Selection for Case Study Analysis: Qualitative and Quantitative Techniques." in *Oxford Handbook of Political Methodology*, forthcoming.
- Laitin, David D. 2002. "Comparative Politics: The State of the Discipline." in *Political Science: State of the Discipline*. Ira Katznelson and Helen V. Milner, eds. New York: Norton.
- Lieberman, Evan. 2005. "Nested Analysis as a Mixed-Method Strategy for Comparative Research." *American Political Science Review* 99:3 (August), 435-52.

Short Course 2: Designing and Conducting Field Research

Contact Person: Professor Colin Elman, CQR, Moynihan Institute of Global Affairs, Maxwell School, 346 Eggers Hall, Syracuse University, Syracuse, New York 13244-1020
Email: consortium@asu.edu
Registration: Faculty: \$10; Graduate students: \$5. Check should be made out to CQR and sent to Professor Colin Elman
Time: 2:00pm–6:00pm
Location: On the premises of the APSA Annual Meeting
Instructors: Diana Kapiszewski, University of California, Irvine

This short course addresses a variety of field methods and data collection techniques to help analysts make the most of their empirical research. Topics include preparing for and conducting research involving the direct observation of political actors, institutions, and processes; unstructured and structured interviews; archival sources; ethnographic study; and managing, analyzing, and evaluating data. Although field methods are usually associated with "studying politics abroad," we discuss techniques that may be applied both inside and outside the U.S. A foundational premise of the course is that planning for the effective use of field methods and the efficient collection of data are a crucial part of overall research design.

Analysts typically initiate their projects by mapping out their analytic questions and identifying the data they will need to investigate and to support their claims. Yet even if the research is well planned and adequately funded, obstacles can arise. Key respondents may be unhelpful or unavailable. Valuable archives and other collections of primary materials may be accessible only on a limited basis or may be poorly organized. Data necessary for constructing sampling frames for formal or informal interviewing may simply not exist. Time or money may run out before essential data have been collected.

This short course will help analysts to anticipate, identify, and resolve many of the challenges involved in designing and conducting field research. We discuss strategies that will allow the analyst to: (1) convert the research design into a "to get" list; (2) identify and begin to investigate data sources before leaving the home institution; (3) make optimal use of relevant technologies (email, web, cell phones, portable photocopying equipment, scanners, digital cameras, and voice and video recorders); (4) respond to the availability of data not anticipated in the original research design, and to the inaccessibility of data that were originally to be collected; (5) organize and manage the potentially vast quantities of information gathered; (6) establish key contacts and interact constructively with politicians, administrators, and scholars in the host community; (7) cope with professionally,

politically, and personally uncomfortable situations; and (8) make the transition from data collection to data analysis and writing in a timely manner.

Participants will be provided with document templates that may be useful when carrying out field research, including sample correspondence. The course is valuable for students planning dissertation projects, for scholars who would like to develop or improve their data collection skills, and for those who teach classes on research methods.

APSA Working Group on Methodology: New Perspectives on Qualitative and Quantitative Tools August 28–August 31, 2008, Boston, MA

This group considers the alternative qualitative methods that are available for political analysis. How does one choose among alternative methods? How can they be used most effectively? When and in what way is it valuable to integrate qualitative and quantitative tools in (a) addressing specific substantive questions and (b) doing innovative research on methodology per se? Working-group participants will attend short courses and panels on qualitative methods and meet for discussion about their application. Participation in the working group requires attendance at a Friday noon–2pm meeting, and (a) two short courses and five panels; (b) one short course and seven panels; or (c) nine panels.

The list of panels and roundtables eligible to satisfy the attendance requirements will be posted at <http://www.apsanet.org/content/16440.cfm>.

Coordinators: Colin Elman, Arizona State University; James Mahoney, Northwestern University; David Collier, University of California, Berkeley; and Andrew Bennett, Georgetown University.

APSA Panels/Roundtables Created (or Co-Organized) by Division 46: Qualitative and Multi-Method Research August 28–August 31, 2008, Boston, MA

Explaining Institutional Change: Contributions from Historical Institutionalism

Chair: Peter A. Hall, Harvard University

Participants:

Tulia G. Falleti, University of Pennsylvania: "Infiltrating the State: Health Reforms Under Authoritarianism and Democracy in Brazil."

Alan M. Jacobs, University of British Columbia: "Policymaking as Political Constraint: Institutional Development in the U.S. Social Security Program."

Adam Sheingate, Johns Hopkins University: "Rethinking Rules: Complexity and Creativity in the House of Representatives."

James Mahoney and Kathleen Thelen, Northwestern University: "How Historical Institutionalists Explain Change."

Discussants: Peter A. Hall, Harvard University; Paul Pierson, University of California, Berkeley

Asking and Answering Questions about Law and Courts

Chair: Julie L. Novkov, SUNY, Albany

Participants: Bradley D. Hays, University of Nevada, Las Vegas; Ryan J. Owens, Washington University in St. Louis; Jeffrey A. Segal, SUNY, Stony Brook; Eileen Braman, Indiana University
Discussants: Lawrence Baum, Ohio State University; Pamela Brandwein, University of Michigan

Multi-Method Tools I: Using Qualitative Evidence with Matching Designs and Instrumental Variables

Chair: Jasjeet Singh Sekhon, University of California, Berkeley
Participants:
Jason Seawright, Northwestern University: "Fine-Tuning Quantitative Matching Methods: The Contribution of Case Studies."
Fernando Daniel Hidalgo and Neal Richardson, University of California, Berkeley: "A Perfect Match? Case Knowledge and Statistical Assumptions in Matching Designs."
Thad Dunning, Yale University: "Model Specification in Instrumental-Variables Regression: The Role of Qualitative Evidence."
Discussant: Jasjeet Singh Sekhon, University of California, Berkeley

Multi-Method Tools II: Concepts, Indicators, and Equivalence

Chair: David Collier, University of California, Berkeley
Participants:
Naomi Levy, University of California, Berkeley: "Measuring Identity: A Structural Equation Approach."
Marcus J. Kurtz and Andrew Schrank, Ohio State University: "Promises and Perils of Cross-National Datasets: Comparative Leverage versus Case Knowledge."
Jody Marie LaPorte and Danielle Lussier, University of California, Berkeley: "Revisiting the Leninist Legacy: Conceptualization and Measurement for Meaningful Comparison."
Zachary Elkins, University of Illinois, Urbana-Champaign and John Sides, George Washington University: "Evaluating Measurement Equivalence Across Contexts: The Case of Democracy."
Discussant: Colin Elman, Arizona State University

The Methods Café

Chairs: Dvora Yanow, Vrije Universiteit, Amsterdam, and Peregrine Schwartz-Shea, University of Utah
Participants:
Farah Godrej, University of California, Riverside and Vicky Hattam, The New School: "Contesting the Political Theory/Empirical Research Divide."
Joe Soss, University of Minnesota, and Frederic Charles Schaffer, University of Massachusetts, Amherst: "Conversational and Ordinary Language Interviewing."
Kamal Sadiq, University of California, Irvine: "'Counting': 'Measuring' Phenomena that Bypass the State."
Douglas C. Dow, University of Texas, Dallas: "Critical Concept Analysis."
Raymond Duvall, University of Minnesota: "Critical Constructivist Analysis."
Lisa Wedeen, University of Chicago: "Discourse Analysis."
Mary Hawkesworth, Rutgers University: "Feminist Methods."
Katherine Cramer Walsh, University of Wisconsin, Madison, and Dorian Warren, Columbia University: "Field Research I (Participant Observation; Political Ethnography): United States."
Jan Kubik, Rutgers University: "Field Research II (Political Ethnography, Participant Observation): Overseas."
Lloyd Rudolph, University of Chicago: "First-Person Narratives and Subjective Knowledge: The Place of Diaries, Autobiographies and Memoirs in Constituting Political Knowledge."
Peregrine Schwartz-Shea, University of Utah: "Generalizing? Validity? Reliability?"

Ange-Marie Hancock, Yale University: "Intersectionality Research."
Julie Novkov, SUNY, Albany: "Legal Archeology."
Kevin Bruyneel, Babson College: "Post-Colonial Analysis."
Ernie Zirakzadeh, University of Connecticut: "Recasting Methods in Light of Experience: Theorizing Before, During, and After Fieldwork."
Ido Oren, University of Florida, and Robert Adcock, George Washington University: "Reflexive Historical Analysis."
Dvora Yanow, Vrije Universiteit, Amsterdam: "Studying Space."
Emily Hauptmann, Western Michigan University: "Teaching Qualitative-Interpretive Methods."
Ron Schmidt, California State University, Long Beach: "Value-Critical Policy Analysis."

Teaching Interpretive Research Methods

Chair: Peregrine Schwartz-Shea, University of Utah
Participants: Emily Hauptman, Western Michigan University; Jan Kubik, Rutgers University, New Brunswick; Peregrine Schwartz-Shea, University of Utah; Robert Kaufman Adcock, George Washington University
Discussant: Lisa Wedeen, University of Chicago

Causal Mechanisms and the Science of Politics

Chair: TBA
Participants: Bear F. Braumoeller, Ohio State University; Kevin A. Clarke, University of Rochester; Christopher H. Achen, Princeton University; Jake Bowers, University of Illinois, Urbana-Champaign; John Gerring, Boston University

Reviewing Comparative Datasets: Democracy and Elections

Chair: Cas Mudde, University of Antwerp
Participants:
Cas Mudde, University of Antwerp, and Andreas Schedler, Centro de Investigacion y Docencia Economicas (CIDE), Mexico: "The Quantitative Skeleton of Comparative Politics."
John Gerring, Boston University: "Measuring Democracy: A Survey and Proposal."
Jennifer Gandhi, Emory University: "Studying Elections in Democracies and Non-Democracies."
Renske Doorenspleet, University of Warwick: "Cross-National Public Opinion on Democratization: Comparing Apples and Oranges or Making Fruit Salad?"
Discussant: Michael J. Coppedge, University of Notre Dame

Process Tracing, Causal Mechanisms, and Theory

Chair: Alan M. Jacobs, University of British Columbia
Participants:
Pascal Vennesson, European University Institute: "Process Tracing in Action: Bridging the Positivist-Interpretivist Divide?"
Deborah Welch Larson, University of California, Los Angeles: "Indeterminacy and Causal Mechanisms in International Relations Theory."
James D. Johnson, University of Rochester: "What Rationality Assumption? Or, How 'Positive Political Theory' Rests on a Mistake and Why It Matters."
Discussant: Alan M. Jacobs, University of British Columbia

Using Data Appropriately

Chair: Craig W. Thomas, University of Washington

Participants:

Eleanor Neff Powell and Gary King, Harvard University: "How Not to Lie Without Statistics."

Gary Goertz, University of Arizona, Tony Hak, Erasmus University, Rotterdam, and Jan Dul, Erasmus University, Rotterdam: "Ceilings and Floors: Theoretical and Statistical Considerations when the Goal is to Draw Boundaries of Data, not Lines Through the Middle."

James Honaker, University California, Los Angeles: "Measuring Metaphor: A Quantitative Approach to Judging the Similarity of Mental Models in Unstructured Interviews."

Discussant: Robert K. Adcock, George Washington University

The Prospects and Pitfalls of Multi-Method Research

Chair: Bear F. Braumoeller, Ohio State University

Participants:

Amel F. Ahmed and Rudra Sil, University of Pennsylvania: "The Logic(s) of Inquiry: Reconsidering Multimethod Approaches."

Ariel Ahram, Georgetown University: "Why is This Region Different From All Others?: A Mixed-Method Approach to Region-Specific Research."

Sebastian Karcher, Northwestern University: "Mixing Misaligned Methods: Promises and Pitfalls in Combining Cross-Country Panels with Case Studies."

Ingo Rohlfing, Jacobs University, Germany, and David Kuehn, University of Cologne: "Empirical Properties of Theoretically Relevant Variables and their Identification in Small-N, Large-N, and Mixed-Method Designs."

Discussants: Bear F. Braumoeller, Ohio State University; Daniel J. Hopkins, Yale University

Concept Formation in Practice

Chair: Fiona Barker, European University Institute

Participants:

Gary Goertz, University of Arizona, Kathy Powers, University of New Mexico, and Douglas Gibler, University of Alabama: "Reconceptualizing Alliances: How Contextual and Theoretical Linkages can Fundamentally Change a Concept."

Matthias vom Hau, University of Manchester: "How to Identify Nationalism? Analytical Tools for Conceptualization and Measurement."

Kendra L. Koivu and Erin Kimball, Northwestern University: "Contested Concepts as Causal Conditions: A Systematic Method for Using the Constitutive Elements of a Concept in Theory Building and Hypothesis Testing."

Payam Mohseni and Leah Gilbert, Georgetown University: "Contested Concepts: Mapping the Boundaries of Hybrid Regimes."

Discussant: Fiona Barker, European University Institute

Comparative Methods: Past and Present

Chair: James Mahoney, Northwestern University

Participants:

Robert Kaufman Adcock, George Washington University: "The Curious Career of 'the Comparative Method.'"

Svend-Erik Skaaning, University of Aarhus: "Assessing the Robustness of QCA Results: Guidelines and Replications."

Carolyn M. Warner, Arizona State University: "The Possibility Principle and the Methodology of Comparative Case Studies of

Corruption."

Etel L. Solingen, University of California, Irvine: "Theory and Method in the Study of Nuclear Proliferation."

Discussant: James Mahoney, Northwestern University

Gendered Campaigns, Gendered Media Coverage: Female Presidential Candidates in Comparative Perspective

Chair: Barbara Kellerman, Harvard University

Participants:

Susan Franceschet, University of Calgary: "Evita vs. Hillary: Media Framing of Cristina Fernández de Kirchner in Argentina's 2007 Presidential Campaign."

Dianne G. Bystrom, Iowa State University: "Gender and U.S. Presidential Politics: Media Coverage of Hillary Clinton's Bid for the White House."

Rainbow Murray, Queen Mary, University of London, and Sheila Perry, University of Nottingham: "A Right Royal Mess: Why did the French say 'non' to the Opportunity to Have a Woman President?"

Magda Hinojosa, Arizona State University: "Mediating Gender: Female Political Candidates and the News Media in Latin America."

Melody Rose and Regina Lawrence, Portland State University: "Hillary Rodham Clinton's Quest for the White House: Dealing with the Media as a Female 'First.'"

Discussant: Barbara Kellerman, Harvard University

Re-Conceptualizing Majorities, Minorities and the State

Chair: Alain G. Gagnon, Université du Québec à Montréal

Participants:

Fiona Barker, European University Institute: "Embracing the State: Identity Shift and the Evolving Concept of the State among Sub-State Nationalists."

Jaime Gerardo Lluch, European University Institute: "National Identity and Political Identity: The Impact of Majority-Nation Nationalism on Stateless Nations' National Movements."

Rinku Lamba, European University Institute: "Non-Domination and the State: A Response to the Subaltern Critique."

Discussant: Alain G. Gagnon, University du Quebec a Montreal

Weberian Methods for Contemporary Comparative Politics

Chair: David Waldner, University of Virginia

Participants:

Lawrence A. Scaff, Wayne State University: "What is Weberian Theory?"

Stephen E. Hanson, University of Washington: "Weberian Methodological Individualism."

David Woodruff, London School of Economics: "Concepts for a Non-Naturalistic Social Science: Understanding Weber's 'Ideal Types' and their Contemporary Relevance."

Discussant: Deborah A. Boucoyannis, Harvard University

History and Politics: Legacies, Sequence, and Method

Chair: Daniel F. Ziblatt, Harvard University

Participants:

Jason Wittenberg, University of California, Berkeley: "Historical Legacies: A Reappraisal."

Jason Kaufman, Harvard University: "History Repeats Itself, Until It Doesn't: The 'Re-Accomplishment of Place' in 20th Century Vermont and New Hampshire."

Jennifer K. Smith, University of Wisconsin, Milwaukee, and Julia

Azari, Marquette University: "Qualitative Historical Methods and the Study of Informal Institutions."

Giselle Datz, Virginia Polytechnic Institute and State University: "Politics in Short Time Frames: Stressing Timing and Sequence in Analyses of Distributive Outcomes from Debt Crises."

Discussant: Daniel F. Ziblatt, Harvard University

**Don't Know Much about History:
Confronting the Possibilities and Perils of
Using Historical Material in Political Science**

Chair: TBA

Participants: Ja Ian Chong, Princeton University; Christopher Darnton, Princeton University; Thomas M. Dolan, Jr., Ohio State University; Pierre F. Landry, Yale University; Min Ye, Boston University; Leonard C. Sebastian, Nanyang Technological University

**Credit to the People: Historical-Institutional, Econometric,
and Mixed Methods for the Politics of Money**

Chair: Leslie Elliott Armijo, Portland State University

Participants:

John Echeverri-Gent, University of Virginia: "Micro-Mechanisms Versus Structural Variables: Political Economy of India's Financial Market Reforms."

Leslie Elliott Armijo, Portland State University: "Financial Reform as a Qualitative Dependent Variable (over Time, Regime Type, Income Level, and Cultural Distance)."

Regina M. Baker, University of Oregon: "Bringing the Third World Back In: The Use of Tobit Models to Address Censoring in International Finance Data."

Herman Schwartz, University of Virginia: "Homes Alone? Corporatism, Economic Growth and Housing Finance Systems."

Discussants: Kathryn Lavelle, Case Western Reserve University; J.M. Chwieroth, London School of Economics

The Nuts and Bolts of Challenging Fieldwork

Chair: Dorian T. Warren, Columbia University

Participants:

Lee Ann Fujii, George Washington University: "The Value of Informal Relationships in the Field."

Roselyn Y. Hsueh, University of California, Berkeley: "Conducting Research in Varying Contexts in China."

Maiah A. Jaskoski, Naval Postgraduate School: "Interviewing Mid- and Low-Ranking Officers in the Peruvian and Ecuadorian Militaries."

Robin L. Turner, University of California, Berkeley: "Embodiment, Positionality and Self-Presentation: Informant Perceptions and Fieldwork Strategies in Southern Africa."

Discussants: Dorian T. Warren, Columbia University; Timothy Pachirat, New School University

**Human Informants: Interviews,
Focus Groups, and Ethnography**

Chair: Joe Soss, University of Minnesota

Participants:

Paula Espírito Santo, Instituto Superior de Ciências Sociais e Políticas: "Focus Group as a Qualitative Research Technique to In-Depth Analysis of Politics and Community."

Maria Fernanda Boidi and Luis Gonzalez, Vanderbilt University: "Elite Interviews and Ordinal-Level Data Analysis: A Pro-

posal."

Michael G. Schatzberg, University of Wisconsin, Madison: "Seeing the Invisible, Hearing Silence, Thinking the Unthinkable: The Advantages of Ethnographic Immersion."

Discussant: Joe Soss, University of Minnesota

Taking Qualitative Comparative Analysis Seriously

Chair: Amy G. Mazur, Washington State University

Participants:

Dorothy E. McBride, Florida Atlantic University: "You've Done QCA; Now What Does it Mean? Interpreting QCA Results in the RINGS Project."

Isabelle Engeli, Université de Genève: "Controversies, Public Policies and Reproductive Issues: A QCA Analysis of Public Policies in Abortion and ART."

Edward P. Weber and Claire Metelits, Washington State University: "Saving Nature and Growing Crops in Uzbekistan: Applying QCA to Explain Policy Outcomes."

Kendra L Koivu, Northwestern University: "Pathways to Perdition: The Many Routes to the Emergence of Organized Crime."

Anna Kalbhenn, Swiss Federal Institute of Technology, Zurich, Torben Heinze, Freie Universität, Berlin, and Christoph Knill, University of Konstanz: "Compliance in the European Union: What do Configurational Comparative Methods Tell us?"

Discussant: Mona Lena Krook, Washington University, St. Louis

Mechanisms and the Dynamics of Civil War

Chair: Scott Gates, International Peace Research Institute

Participants:

Kristin Marie Bakke, Harvard University: "Explaining the Turn to Violence in Center-Region Conflicts."

Martin Austvoll Nome, University of Oslo: "Mechanisms and Transnational Violent Mobilization: Turkish Involvement in Cyprus, 1963-75."

Jeffrey T. Checkel, University of Oslo: "Causal Mechanisms and Civil War."

Discussant: Jason M.K. Lyall, Princeton University

**Context, Method and Analysis: Interpretive Techniques for
Understanding Other Political Systems**

Chair: Dvora Yanow, Vrije Universiteit, Amsterdam

Participants:

Robert Press, University of Southern Mississippi: "The Importance of Interpretive Methods in Detecting and Analyzing Social Movements in Authoritarian Countries: Contemporary Human Rights Activism in Liberia and Kenya."

Farah Godrej, University of California, Riverside: "Methodological Lessons from Comparative Political Theory."

Anthony Peter Spanakos, Montclair State University: "Taking Methods to the Street(s): Interpreting Venezuela's Two Poles."

Kamal Sadiq, University of California, Irvine: TBA.

Discussant: Dvora Yanow, Vrije Universiteit, Amsterdam

We Think Ideas Matter, But How Do We Know?

Chair: Judith A. Layzer, Massachusetts Institute of Technology

Participants: Deborah Stone, Dartmouth College; James A.

Morone, Brown University; Judith A. Layzer, Massachusetts Institute of Technology; Laura S. Jensen, Virginia Polytechnic Institute and State University; Leigh S. Raymond, Purdue University

Haggard and Kaufman, *Development, Democracy, and Welfare States*

Chair: David Collier, University of California, Berkeley
Participants: Anna M. Grzymala-Busse, University of Michigan;
Evelyn Huber, University of North Carolina, Chapel Hill; Jonas
Pontusson, Princeton University; Tun-jen Cheng, College of
William & Mary
Discussants: Stephan Haggard, University of California, San Diego,
and Robert R. Kaufman, Rutgers University.

**New Approaches to the Measurement of Ethnicity:
Identities, Institutions, and Power**

Chair: Evan S. Lieberman, Princeton University
Participants:
Kanchan Chandra, New York University: "Which Institutions, if
any, Ensure Stability in Multi-Ethnic Democracies?"
Taeku Lee, University of California, Berkeley: "Through the
Looking Glass, Darkly: External Ascription, Self-Identification,
and Competing Approaches to the Measurement of 'Race.'"
Evan S. Lieberman and Prerna Singh, Princeton University: "Insti-
tutionalized Ethnicity and Ethnic Conflict (1900-2000)."
Andreas Wimmer, University of California, Los Angeles, and Lars-
Erik Cederman, ETH Zurich, Switzerland: "Getting Ethnicity
Right: A Global Dataset on Ethnic Power Relations, 1945-2005."
Discussant: Ashutosh Varshney, University of Michigan

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Vincent Boutet-Lehouillier, University of Wisconsin: "When
Social Networks Undermine Democracy: Nazi Paramilitaries
and the Fall of the Weimar Republic."
Eric M. Blanchard, University of Southern California: "Con-
structing Critical Qualitative Research: A 'Methodological
Turn' in Post-Positivist and Interpretivist Political Science?"
David V. Edwards, University of Texas, Austin: "Constructivism:
Categories and Their Consequences for Social Theorizing."
Daniel Kinderman, Cornell University: "The Strength or the
Weakness of Institutions? On the Ambivalent Meaning of
Corporate Responsibility and Industry Self-Regulation in
Different Countries."
Jack W. Meek, University of La Verne: "The Role of the Re-
searcher in Collaborative Action Research."
Jürgen Petersen, Johann Wolfgang Goethe-Universität, Frankfurt:
"Start Making Sense: Actors' Conceptual Theories of Repre-
sentation in Cross-Contextual Comparison."
M.J. Peterson, University of Massachusetts: "Ontology, Episte-
mology, and Inter-Paradigm Debates."
Shawn W. Rosenberg, University of California, Irvine: "A New
Measure of Political Communication."
Jenny Wustenberg, University of Maryland: "At Home in the
Field: Doing Political Science Research in Your Country of
Origin."
Jonathan Chausovsky, SUNY, Fredonia: "Path Dependency and
Stare Decisis."
Mihaela Ristei, Western Michigan University: "Informal Institu-
tional Change: Towards Developing Comprehensive Measures."
Marc P. Berenson, Princeton University: "Categorizing Trust
After Transition: Why Does Trust in the State Vary So
Dramatically Across Poland, Russia and Ukraine?"
Kimberly Rae Carter, University of Toronto: "Dominance, Grace,
and Everything in Between: What Mexican Communities Can
Teach Political Scientists About Power."

Oyebade Oyerinde, Indiana Wesleyan University: "Inequality and
Contextual Multilayered Political Institutions in Nigeria."
Jean-Yves Dormagen, Université de Montpellier, France: "Indi-
vidual Voting Histories: How Much Faith should we Place in
the Memory of Survey Respondents?"
Jennifer W. Howk, Harvard University: "Losing Ground: Climate
Change, Political Uncertainty, and Mobilization in Four
Alaskan Communities."
Mark Axelrod, Michigan State University: "Modeling Path
Dependence with Variation."

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