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

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### 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.

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## *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.<sup>1</sup>

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."<sup>2</sup>

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

<sup>1</sup> 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.

<sup>2</sup> 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).

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## *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.<sup>1</sup> 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.<sup>2</sup> 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 a 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

<sup>1</sup> 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).

<sup>2</sup> 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).

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## 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

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.<sup>1</sup> 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.

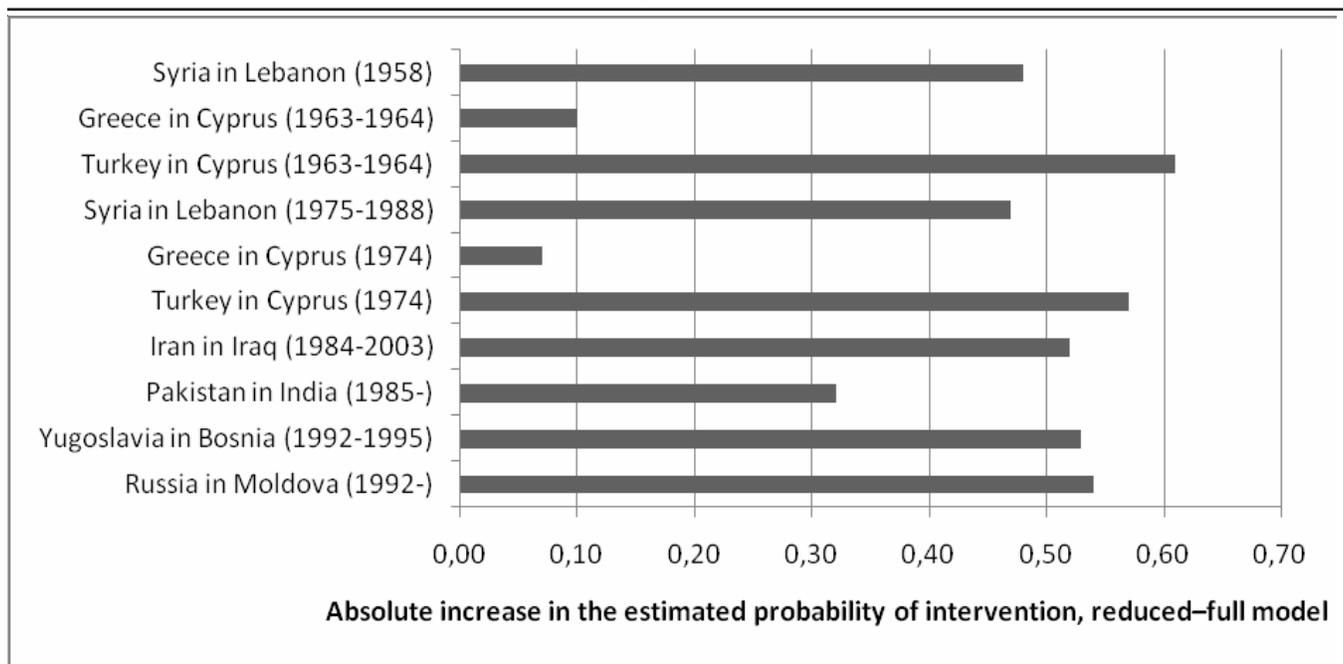
### Mechanisms, Not Variables; Theory Building, Not Theory Testing

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

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

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## *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.<sup>1</sup> 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.<sup>2</sup> 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.<sup>3</sup> 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.<sup>4</sup> 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

<sup>1</sup> 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.

<sup>2</sup> 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).

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

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

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## 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.

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## 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-