

THE WEAKEST LINK?

SCHOLARSHIP AND POLICY ON INTRASTATE CONFLICT

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Introduction

How do theory and practice in the critical domain of intrastate conflict interact? When events, such as the Syrian civil war, spiral for years, do policy makers consult academics or their ideas? Do academics respond to such events by trying to explain why these intrastate conflicts start or end? Should this interplay between policy and academia occur? If and when it does, how will we know it? When thinking about strengthening the links between the policy and academic words, these are but a few questions to consider.

Our core argument here is that scholarly work has had little influence on policy, a conclusion that is based on several auxiliary arguments and associated with preliminary data analysis. On a general level, the area of intrastate conflict faces challenges similar to other topical areas like human rights or the global environment: there is terrible communication between academics and policymakers, fundamental differences between methodological training and reporting, and precious few individual-level incentives for collaboration.

More specific to intrastate conflict, much of the intrastate conflict occurring in the world has taken place in countries / regions in which policymakers in the U.S. and other Western institutions have historically had little interest. Even during the Cold War, much of the violence in Africa and Asia drew relatively little attention from outside. Second, intrastate conflict has only become more important in the academy in the last 10-15 years and that interest has largely been rooted in the explosion and

analysis of large-N databases. While political science (comparative politics specifically) has a somewhat longer tradition of discussing revolution, few conflict studies groups existed to propel that research into the mainstream of political science, much less the policy world. So it would be optimistic at best to expect much opportunity for the intrastate war literature to have an effect on policy. Third, what little relationship appears to exist comes from the literatures on insurgency and development, though it is unclear just how much scholarly work drives policy as opposed to the two taking place concurrently.

Turning to the question of whether the policy world has had an impact on scholars, we similarly argue that scholarly work has integrated very few insights from the policy community. While scholars are likely influenced by current events and study the conflict processes that are occurring, it is far from clear that they pay any attention to the decision calculus of policymakers in the U.S., other western countries, or international institutions. Tying to our point above about international development, growing numbers of collaborations between academics and individuals in the policy world have led to greater insights in the peacebuilding literature, among others.

A central task in this paper then is to examine the TRIP data to learn what it might reveal about the relationship. Unfortunately, the data provided by the conference organizers are not very telling. They provide small pieces of the puzzle, but as we discuss, most of the puzzle remains incomplete especially if we are to provide credible evidence about the academic-policy relationships. For the evidence we do find, we can only hazard some ideas about why trends exist. Hopefully this encourages refinement of the measures, and some concrete discussions about the identification of possible causal relationships between scholarly work and policy activity.

Indeed, we hasten to add that almost nothing systematic is known, especially about any causal effects. The academic and policy communities would likely prefer (or should prefer) an answer to two specific causal questions: (1) does scholarly research influence policy? (2) do international events and associated policy decisions affect academic research? And, as well, underlying these questions, why and how do these relationships occur?

We want to offer cautions about the inferential minefield we all face in attempting to evaluate the academic-policy connections from the perspective of isolating causal effects or mechanisms. We venture that at this stage it may be impossible to do so in any completely satisfactory way. Instead, we may be limited to identifying patterns of covariation in academic ideas and policy impact both through TRIP data and csae evidence. But beginning the process of addressing the causal questions, even if

piecemeal, is vital, especially given the tremendous pressure on political science to justify its relevance.¹

In what follows, we first lay out the conceptual scope conditions, discussing different types of intrastate conflict and peace processes and also identifying what policy prescriptions or policy individuals are. We then lay out a rough framework for thinking about what a policy influence would be so that we can begin to think about how more rigorous analysis could, perhaps, be conducted. Following, we raise a number of examples that suggest meaningful academic and policy engagement on intrastate conflict is occurring in important domains. We then examine each of the TRIP data sets for what they might have to offer for the study of intrastate conflict. Before concluding, we lay out some ideas on what needs to be done moving forward and also what it might look like to take the identification of causal effects more seriously.

Scope of the Discussion

Before jumping in, let us say a few brief words on scope. First, we consider what are intrastate conflict and peace operations. Second, and perhaps less straightforward, we discuss what is a policymaker vs. say a non-academic practitioner. Finally, and perhaps most importantly, we address a much deeper question that haunts the discussion of academic and policy connections: how would one recognize academic influences on policy if one were to go looking? There are pernicious measurement and identification problems associated with this final exercise.

What is Intrastate Conflict? And What Conflict Resolution Options Exist?

For the past few decades, events, such as the Rwandan genocide, the 9/11 attacks, and the Syrian civil war, have pushed the study of conflict beyond traditional areas of great power disputes, militarized interstate conflicts, and the like. After the end of the cold war, events such as Somalia and the breakdown of Yugoslavia drew scholars away from traditional security issues into studying sub-state processes. This development was further enhanced in the late 1990s by the upsurge of *civil war* research.

So, what is civil war? Why is it different than interstate war? Or more directly, why are these distinct concepts worthy of their own investigation? Standard definitions of war that emanated from the Correlates of War (COW) project essentially defined it as a dispute between two internationally recognized states that lead to battle deaths exceeding a 1000 person threshold.² Early investigators used a similar

¹ See, for example, <https://www.insidehighered.com/news/2013/09/03/political-scientists-consider-strategies-deal-ban-nsf-support>

² See Moore (2006) for a discussion on the “dark side” of this definition.

definition of civil war, which was influenced by COW: civil war is an intrastate conflict between the state and a non-state actor exceeding 1000 death battle threshold. Sambanis (2004) in the most comprehensive discussion of the concept of intrastate war argues for a pragmatic approach to defining and then measuring civil war that takes into account case knowledge, consistent battles over the course of the conflict, unreliable reporting, wars of decolonization and other factors. While his definition is certainly more thorough than COW or the earlier work on the topic, it stills begins with the notion that civil war is a kind of conflict that breaks out and engulfs a state.

Young (2013) suggests a deceptively simple, but critical addendum. Civil war, at its core, is violence between the state and non-state actors, but it is conflict that *builds* above a given threshold. Rather than large-scale violence simply breaking out or immediately crossing a large threshold, civil war is a point on a continuum of conflict processes that is arrived at by state-dissident interaction. This insight leads to differentiating civil war from other forms of violence but it also recognizes that civil war is part of the larger conflict repertoire available to states and non-state actors. This point, then, tries to bridge some of the work in sociology by pioneers, such as Charles Tilly, with civil war studies emanating mostly from political science and economics.

This insight and some of the sociological work suggests that civil war is connected to these other kinds of violence as these forms of conflict build to civil war, they make civil war last longer, or they might even occur after civil war as a residual effect (Sambanis 2008, Findley and Young 2012). Terrorism is another common form of violence, which by contrast, often lacks a death or violence threshold and is even more contentious to define than civil war (Hoffman 2006). For our purposes, we offer a fairly common definition: violence or threats of violence against a target used to persuade a third actor in pursuance of a goal.³ This definition is agnostic to whether this contention occurs within or across states, but empirically we witness most terrorism within states (Lafree and Dugan 2007, Enders et al. 2011).

While the study of civil war, terrorism, and other intrastate conflict has occurred in what has been termed islands of inquiry (Most and Starr 1984), more recent work is connecting these isolates by recognizing the overlap and affinities among the types of violent contention (See, for example, Fortna 2011, Findley and Young 2012, Thomas 2014).⁴

Beyond violent contention, scholars such as Erica Chenoweth, have begun to investigate the link between forms of violent and nonviolent resistance. Chenoweth and Stephan (2011), for example, find that nonviolent resistance is a more

³ See Hoffman (2006), Weinberg et al. (2004), and Young and Findley (2011) for a more complete discussion of defining terrorism.

⁴ Findley et al. (2012) and Conrad (2011) also connect the study of interstate war with transnational terrorism.

successful form of contention as compared to violence. Another important distinction is between purely internal and internationalized civil conflicts. While most conflict today is intrastate, about 27% of current conflicts are internationalized or involve troops from another state (Themnar and Wallensteen 2014).

Some scholars focus primarily on stages of a conflict, and ask questions about onset, duration, recurrence or whether outside actors are involved. A different way of approaching intrastate conflict dynamics is to take an actor-centric approach and consider the reasons why third parties intervene, be it for political or humanitarian reasons (Findley and Teo 2006; Gent 2007). From the perspective of policy influence, the latter approach is more relevant. Indeed, the scholarly literature on civil wars seems to be somewhat less connected to policy discussions relative to the peace studies literature, which has featured more prominently.

What we are calling the peace studies literature examines a number of dimensions including peacekeeping, peacemaking, peacebuilding, and peace enforcement. All of these peace operations are on the menu of options for countries such as the United States in thinking about how to get involved in intrastate conflicts abroad. Complete discussions of each of these types of peace interventions are beyond the scope of this paper, but we note here that these different types of peace operations require different levels of material and time commitments ranging from short-term peacekeeping to long-term peacebuilding. They could be conducted by international organizations or by states. When conducted by states, however, there is often skepticism about how biased they are. The recent Russian aid convoy into Ukraine is a prime example.

Peace operations have been criticized heavily but there is evidence that they may often be effective (Doyle and Sambanis 2006). And while the organizations that conduct peace operations lag behind standard development organizations, even many UN agencies are interested in learning and trying to adopt the more rigorous evaluation standards that scholars have applied. One of the authors here recently completed an impact evaluation of the UN Peacebuilding Fund's (PBF) operations in Burundi and conducted the PBF's first quasi-experimental evaluation (they have not conducted any randomized trials), and have had follow-up conversations with the PBF's Monitoring & Evaluation specialists who have confirmed that they are now requiring similarly rigorous evaluations (given UN's constraints) moving forward.

For which of these conflict and peace processes are there natural affinities with policymakers? Which already have connections? Internationalized intrastate conflicts such as in Iraq often move scholars and practitioners into the realm of counterinsurgency (COIN). The role of think tanks, especially the American Enterprise Institute, was prominent as Fred and Kimberly Kagan advised General Petraeus directly. Max Boot, Michael O'Hanlon of Brookings and others were directly

engage in COIN strategy.⁵ Unsurprisingly, where direct strategic interests of the US are involved, the US academic community tends to gravitate towards these questions, due in part to the availability of research funding.⁶

What is a Policy Prescription? And Who is a Policymaker?

An anecdote that we fear may generalize: one of us received a revise and resubmit decision on a paper early in our career in which a reviewer, signaling some irritation asked why there were no policy prescriptions in the conclusion of the paper. And then the reviewer continued saying that the prescriptions must be there in order to endorse publication. The reviewer's instructions (paraphrased): two paragraphs would be sufficient, and they should simply communicate to "the policymakers" how they should redirect the country's activities based on the research findings.

The flawed logic of this scholar suggests the following: Write an academic paper, fill it full of jargon, add some stars and bolded numbers, publish it in a gated venue, and put two paragraphs at the end of a long, oddly-structured article that explains how state policies should be changed. And the "policymakers" – presumably a Hillary Clinton or John Kerry – will be paying attention and change things.

The standard academic policy prescription that we caricature is obviously insufficient. But this sort of flippantly provided policy prescription may be what we are seeing in the academic work we share with each other. The notion of offering policy prescriptions in academic articles is curious on multiple levels, which would be worthy of a discussion in itself. We wonder in particular whether the scholars least engaged are the ones that offer blunt policy prescriptions in articles whereas those who are more engaged find much more subtle, yet effective ways to communicate these lessons. If so, attempts to learn about academic-policy connections from policy prescriptions in academic articles could give us precisely the opposite impressions about what is actually happening.

We also wonder how closely the policy prescriptions actually follow the empirical work discussed. Our hunch is that many of these policy prescriptions do not track with authors' actual empirical evidence, and instead are hazarded wildly to elevate the perceived importance of the work. After all, if only a few other academics will read the paper, what does it cost to give some advice to Putin or Merkel? Probably more likely, scholars may not have the appropriate skills or experience to pull out a logically derived policy inference that appropriately speaks to the policy world. If the anecdote described above in the context of intrastate war studies applies more generally, then we have serious concerns about the larger set of policy prescriptions most academics are offering in their papers.

⁵ <http://www.propublica.org/article/meet-the-think-tankers-advising-the-us-military-in-kabul>

⁶ The Minerva Initiative, established in 2008 by then Secretary of Defense, Robert Gates, is an example.

Some academics have taken to blogs, op-eds, policy briefs, and numerous other places to put out lessons on their work. In our own area, the *Political Violence at a Glance* blog, which one of the authors is a contributor, may be the most prominent blog forum for disseminating academic work on intrastate conflict. *The Monkey Cage*, a blog started by political scientists and now connected to the *Washington Post*, has published increasing numbers of posts in this area, though it is not primarily tailored to intrastate conflict. These and other venues may provide different types of policy engagement that are important to understand. But similar questions apply – to what extent does this work suggest policymaking rooted in strong evidence?⁷

Beyond the type of prescription, the question of who is a “policymaker” is also important. In the standard academic paper, scholars seem to assume they are writing to high-level officials – the policy “makers”. Yes, there are policymakers of the Clinton/Kerry variety that may pay some attention to academic work. But it is hyperbole to argue that many, or perhaps even a few, of them do. At best, they may have particularly attuned advisors who help filter some lessons learned. The small number of such policymakers likely pale in comparison to the vast numbers of other government staff, including those that work for intergovernmental organizations such as the World Bank or various UN agencies, who could be termed policy “implementers”. Policy implementers are much more likely, we think, to play a role in applying academic insights into policy.

Moreover, academics appear to ignore another very large group of individuals: there are hundreds of thousands, or millions, of private, for- and not-for-profit organizations that operate in spaces of interest to political scientists, especially in the context of intrastate conflict and peace activities. In our own areas, there is a vast array of conflict resolution and development organizations that we would call policy “practitioners”. These individuals and organizations are perhaps in a much better position to implement change than government policymakers, and perhaps even than policy implementers who may be struck in complex bureaucracies. And yet academics write their policy prescriptions as if these organizations do not exist, haven’t offered similar or contradictory suggestions, or do not matter.⁸

In sum, this workshop hopes to understand academic contributions to policy (and vice versa). But we have doubts about the policy “prescriptions” academics provide as well as about whether academics provide prescriptions with serious intent. Finally, we wonder how much appreciation (knowingly or not) scholars have for what types of people operate in the policy world including who might actually read the work and do something with it.

⁷ And of course a policy prescription does not need to be expressly identified as such. Any work could fall into the hands of the policy community who could draw lessons from this (or not).

⁸ An informative project would code the audience of each policy prescription offered by policymaker vs. implementer, the content of the message, or other relevant information for linking messenger, message, and receiver of the message.

Of course, the response to this is that academics may be clueless in their approach, but the influence could occur anyway. This leads to our next question: how would you recognize influence on policy if you saw it?

How Would We Know Influence on Policy?

Given that many academics might like to influence policy,⁹ how would we think about this influence? Even more, how would we measure this influence?

Clearly, there are some examples in the academy of scholars making tremendous impacts on policies related to intrastate conflict. Obviously, Henry Kissinger had wide ranging influence on policy for decades. As a strict adherent to realism, he brought this theoretical perspective to everything from counterinsurgency in Vietnam to intervention in substate conflicts throughout Latin America. Regardless of how we conceptualize or measure impact, Kissinger would be at the far end of the spectrum.¹⁰

But where are the rest of us? At some level, being an advisor to governments or directly involved, like Kissinger, would be a huge impact. More realistic would be having an indirect¹¹ influence, such as research featured or cited in Congressional testimony or in the operations of a UN peacebuilding operation.¹²

Below we offer a stylized spectrum of influence academics can have on policy.

A Spectrum of Policy Influence

1. **None**—at the basic level and perhaps the modal academic influence is to have absolutely no influence on policy. Some academics do not see this as their mission. Some do not want to have this kind of influence, and probably some ask too arcane a question to have much policy relevance. Moreover, these academics may not be creating any new ideas, even that other scholars are debating. They may be in a position that does not require publishing or their incentives are to teach or administrate but not to publish. To the extent that we construe policy influence at the broadest level, we might consider anyone educating the next generation to have

⁹ It might useful to know what the baseline is for how many academics, what proportion, or what type would like to influence policy.

¹⁰ This discussion is agnostic as to what type of normative impact one can have. One could argue Kissinger's influence was negative across policy domains. Regardless, he did shape policy. The underlying dimension is influence or not.

¹¹ Green and Hale (2014) offer a way to think about influence in global environmental policy that includes direct, indirect, and diffuse. Our approach is quite complementarity to their suggestion but offers a bit more detail.

¹² One example is Christian Davenport's testimony on his paper with co-author Will Moore on causes of torture in democracies. <http://www.c-span.org/person/?christiandavenport>

at least a small indirect influence. Regardless, this group or point on the continuum is a starting point.

2. Papers cited by scholars—Another large group of academics likely influences other academics by publishing high-quality peer-reviewed research that pushes a research program into new territory, spawns dissertations, has major detractors and supporters, and might be called an *-ism* or a *have school* attached to this way of thinking. To the extent that this group influences policy, it might also be an indirect process. Ideas percolate up through long debate and discussion, research papers influence students in masters and doctoral programs who go on to the policy world, and finally the ideas make their way into larger policy discussions.¹³

3. Paper cited by policy makers—As much as we scholars disparage the “Clash of Civilizations” article/book by Samuel Huntington¹⁴ in public and private venues, a fact remains: in meetings with policymakers the article is often referenced.¹⁵ This is partially due to Huntington’s previous relationship with the policy community, but also due to the ease of which the article’s thesis can be digested. This is a critical point Mack (2002) makes when advising academics how to speak to the policy community, and it is a point we will return to at the end.

4. Speaking to NGOs, policy/practitioner community—Academics are at times encouraged to speak to non-governmental organizations, say Amnesty International, or be involved in workshops or meetings at think tanks like the Center for Strategic and International Studies. These types of connections to the policy world may begin to have a more direct influence, yet that impact may be minimal. Indirect *and* direct impacts may be large or small.¹⁶

5. Speaking to government or IGO officials (policy makers or implementers)—While the meetings with think tankers and NGOs deal with policy, actually speaking to policymakers/implementers is the first of the most direct ways to influence policy. In the study of intrastate conflict, this may be speaking to members of the Department of Defense (DoD), the State Department, or the World Bank. Like many scholars, we have been asked on several occasions to speak to DoD, UN, or USAID about our research. We’ve observed direct policy or operational changes that began with these talks.

¹³ This might describe the democratic peace theory, but does not explain something like Huntington’s “Clash of Civilization”, which was roundly discredited among academics, but latched onto by policy makers (Russett et al. 2000, Chiozzo 2002).

¹⁴ We have purposely cherry-picked examples relating to intrastate conflict. This is not meant to be a full accounting of all academic engagement across issues areas.

¹⁵ This is based on our own anecdotal evidence. It seems to be more popular in different parts of the USG.

¹⁶ For example, a classroom teacher may have a large direct influence on the education of a student in the class, but a small indirect influence on overall education at the school. A principal may have a more indirect influence, but that influence could be larger on students in other classes than the original teacher.

6. Advising NGOs, policy/practitioner community—Beyond a simple one-off discussion, academics can be asked to advise and coordinate with these organizations and individuals. For example, Stephen Tankel, an assistant professor at American University, is a nonresident fellow at the Carnegie Endowment for International Peace. When he writes a policy brief, Carnegie supports it and invites scholars and policymakers to engage in the discussion. Jay Ulfelder, a non-affiliated political scientist that used to work as the research director for the *Political Instability Task Force*, works with the Center for the Prevention of Genocide of the United States Holocaust Museum as part of the Early Warning Project, to produce risk assessments globally for onsets of mass atrocities.

7. Advising government—This is the most direct way an academic might have influence in the policy world while retaining their academic affiliations. Jeremy Weinstein, who is currently the chief of staff for the US Ambassador to the UN, Samantha Power is a good current example. While a tenured professor at Stanford, Weinstein also previously worked for Power at the National Security Council where he was the director of development and democracy. Assessing the impact of such advising is quite complicated especially in the realm of international security. As discussed below, many venues such as the Political Instability Task Force for academic input are at least in part private thereby making it difficult to identify what advice is given and therefore what effect there may be.

8. Educating students who go into policy positions—One of the most important, but indirect, ways in which scholars may influence policy is through the education of a large number of students over the course of a career who go on to influence policy. In policy schools especially, but in universities more generally, scholars educate thousands of students over the course of their careers; undoubtedly many of these students go on and make a very large number of decisions that move forward the machinery of public policy.

9. Become a policy maker—As stated at the outset, Kissinger is one of the best examples of someone who has a direct and important impact as he advised presidents, was the Secretary of State and the National Security Advisor in a couple of administrations. Woodrow Wilson is the only political scientist ever to be elected President. Short of these high-impact positions, many more current or former academics are working in the DoD, USAID, or other government or IGO organizations. While it is common for PhDs in economics and other fields to go straight into business or government, this has not historically been the case in political science, though there appears to be a change as an increasingly larger share of political science PhDs go on to work for congressional staffers, the intelligence agencies, or even non-governmental organizations such as Amnesty International.

This spectrum provides us with a heuristic for thinking about relative impact academics might make. How would we then measure this impact? We could count the number of times a person or article is cited in congressional testimony. We could

count and weight the strength and duration of engagement with the policy world by taking into account formal vs. informal advising, number of meetings, how close to the top of the organization the person is involved, and many other ways.

At American University, faculty and leadership have had related discussions as engagement with the policy world is an explicit goal of the university and thus something that figures into merit and promotion. Within the scale described above, there is guidance in thinking about these rankings. Exact measurement is more complicated and would likely need to be worked out more completely. Mention in congressional testimony, for example is better evidence of impact than say a citation in a think tank report. For intelligence analysts, a mention in the presidential daily briefing (PDB) is the highest honor (similar to an APSR article in political science).¹⁷ In the intelligence world, at least, there is a similar parallel system where impact is considered and used for promotion and advancement.

Most importantly, and overlooked until now, how do interactions between academics and policymakers lead to reasonably informed policy? As Mack (2002) argues, policymakers need to understand that a few examples do not falsify a theory¹⁸. Most academic theories are probabilistic and thus need to be supported or refuted based on the weight of evidence.¹⁹ Academics need to do a better job communicating the strength and limitations of their work. For instance, quantitative scholars need to share their material in a way that is digestible and to make clear to policymakers that their work is often complementarity and not just a substitute for area studies knowledge (Mack 2002).

One of the ultimate challenges is that policymakers and the institutions where they operate are results based. Sometimes people make the “right” decisions but the outcome is nonetheless suboptimal. For example, we might have a plan for getting rich. We work hard, go to business school, consult successful and unsuccessful business owners, have a detailed business plan, and we still fail. In contrast, someone buys a lottery ticket, and they become rich. Clearly, in this case, our plan and decisions increased the probability for success as compared to the lottery-ticket buyer. Looking at just the outcome obscures which process would yield a better outcome most of the time.

These issues give rise to the question we began with. Measurement of a policy impact is difficult. If an academic speaks to some policy implementers in DC or Geneva, and then we observe some policy change, can we establish that academic work was responsible? What if, for example, the change was going to occur anyway

¹⁷ We would like to thank Tricia Bacon, an assistant professor at American University and former member of the intelligence community, for this insight.

¹⁸ To be more accurate, a hypothesis from a theory. Whether one is more Kuhnian or Lakatosian, a single refuted hypothesis rarely if ever has the impact to invalidate an entire theory.

¹⁹ Some rigorous qualitative work, such as Skocpol (1979), outlines necessary and sufficient conditions where a single case could undermine the claim. These arguments are more rare among empirical scholars.

and an organization simply brought in some academics to speak to the issue? We could continue this exercise with many possible counterfactuals; suffice it to say here that we need to be extremely cautious about what we conclude.

How Would We Know Policy Influence on Academic Work?

In contrast to the task of tracking influence in the policy community, the task of observing influence on academics is ostensibly simpler. There is a largely known set of academics whose work is tracked via journal articles and conference submissions, and whose opinions can be solicited via surveys. Thus, the TRIP article database and scholar surveys provide some useful tools for us to examine, say, numbers of terrorism articles pre- and post-9/11. Indeed, surveys of this variety exist.

The answers we get to these questions depend on what we look for. In our case, the TRIP journal database codes intrastate conflict articles, but the TRIP survey by construction excludes *ethnic conflict* and asks little about intrastate conflict more generally. The Snap polls relate to a couple of internationalized intrastate conflict, but have a very particular approach to the questions. We are thus left with a limited body of evidence from which we can draw.

Moreover, there is a large censoring problem that makes the task of understanding difficult. Many ideas are spawned but never written, or papers written but never presented, or written/presented but never published, or published but never read, etc. In the realm of teaching, most public course descriptions are brief, many syllabi still gated, most lecture notes not distributed, etc. We will not expand here, but academic incentives, university bureaucracy, and the publication system in the academy provide numerous challenges that would need to be accounted for in order to fully address impact.

And to add another dimension, we need to be careful about what on the policy side should influence academic work. Is it the political events that are occurring? 9/11 clearly influenced how much work was being conducted on terrorism and the end of the Cold War altered scholarship in important ways. But we may also be interested in the policy community's activities? In recent years, we've seen much work in the experimental community devoted to providing better impact evaluation practices. The flurry of randomized controlled trials in development and conflict, among other areas, has in many ways been a reaction to flawed impact evaluation practices in the World Bank, UN, or other such organizations.

State of Academic and Policy Engagement

There are many reasons to believe that in the context of intrastate conflict the academic-policy links are tenuous. First and foremost, over the past 50 years most intrastate violence has occurred in areas of Africa and Asia (Uppsala 2014) where

the U.S. and many other countries have had little strategic interest. Thus, even if academics and policymakers were speaking openly and consistently, there has been little policy interest in the areas experiencing intrastate conflict. Brief forays into non-strategically important areas such as Somalia demonstrated how few benefits there were for third-party states to get involved. Even UN peace operations during the Cold War and immediate post-Cold War years were still learning valuable, though painful, lessons about intervention in these areas.

Second, at the same time policy interest in intrastate conflict increased, academic interest in the topic also increased. And yet, unfortunately, the two progressed on quite different paths. Scholars turned towards increasingly sophisticated methods to analyze large databases (for average effects) of various causes on war onset or dynamics. Few scholars expressed interest in serious engagement about specific cases with policymakers or practitioners. For their part, policy-minded individuals made little attempt to understand the pitfalls or potential promise of quantitative approaches.

There is thus little reason to expect that academics and policymakers should have had a healthy dialogue that produces real-world impact. And yet, there appears to be at least some influential initiatives in which policy impact has occurred. Before turning to the TRIP data, here we informally discuss some of them that are active on topics related to intrastate conflict.

From Academy → Policy

From the 1960s to the 1980s, the study of intrastate conflict was carried out primarily as examinations of revolution and rebellion. The work in these years is most commonly associated with the scholars such as Davies (1962), Moore (1966), Gurr (1970), Tilly (1978), and Skocpol (1979). A related area, though not exclusively focused on intrastate war, is the study of counterinsurgency strategy, much of this associated with practitioner-scholars such as Galula (1964) who was also a military officer prior to publishing his scholarly work and Leites and Wolf (1970) at the RAND corporation. Neither the revolutions literature nor the counterinsurgency literature was particularly pervasive in public policy discussions, though the latter featured more prominently at least indirectly on U.S. strategy in Vietnam. Gurr's work was noticed and led to a 1968 appointment to the National Commission on the Causes and Prevention of Violence, a short-lived (by design) commission put into place following the assassinations of Martin Luther King, Jr. and Robert F. Kennedy.

More prominently, Gurr helped found the State Failure Task Force (later renamed the Political Instability Task Force (PITF) in the mid-1990s). The PITF, which began under the Clinton administration and housed within the CIA has routinely engaged academics for help providing estimates of instability. Academics, such as Jay Ulfelder, Erica Chenoweth, Monty Marshall, and Jeremy Weinstein have been

involved at varying degrees. Other academics with regional or topical expertise have been invited on a more ad hoc basis. The Task Force's discussions are secret making it difficult to assess just how much of the advice offered is actually implemented in some way by policy makers in the intelligence or security industry.

The Integrated Crisis Early Warning System (ICEWS) is another example of scholarly input into policy arenas, specifically the military. Academics have been involved with the program for years. In 2007, the Defense Advanced Research Projects Agency (DARPA) started ICEWS as a tournament to see which models could provide some of the best forecasts of crises around the globe. Since 2011, ICEWS has been an Office of Naval Research project. In comparison to PITF, ICEWS uses subnational event data, at a granular temporal and spatial level. Mike Ward at Duke, Phil Shrodt formerly at Penn State, Ian Lustick at Penn, and Steve Shellman formerly at the University of Georgia are all academics that are involved in the project. Again, much of the activities in the ICEWS project is not public thus making it difficult to assess any of the impact.

The Good Judgment project, which works with the U.S. Intelligence Advanced Research Projects Activity (IARPA), is another initiative that has turned to crowdsourcing information to forecast world events, including intrastate conflict. They claim to be surprisingly successful at prediction – better even than intelligence analysts with classified information – and the question will now be to what extent the predictions they make transfer over to the policy community.

Academics have worked directly within various organizations either on a temporary or permanent basis; whole centers or institutes at places such as the World Bank or USAID are devoted to policy-relevant scholarly research. In fact, some influential work from scholars such as Paul Collier, Nicholas Sambanis, Anke Hoeffler, Michael Doyle, began or advanced while in policy organizations such as the UN or World Bank. In the area of intrastate conflict, one of the most visible impacts of much academic work from numerous scholars was the 2011 World Development Report devoted to the connection between armed conflict and international development. A number of noted development and conflict scholars contributed background papers and otherwise helped author the report.

Related, one of the fastest growing trends in recent years is policy-relevant experimental work. The Jameel Poverty Action Lab (JPAL), Innovations for Poverty Action (IPA), Experiments in Governance and Politics (EGAP), and others have all actively conducted randomized controlled trials in a variety of developing countries on topics ranging from sex education interventions in schools to the administration of hundreds of millions of dollars for food distribution in violence affected communities. Many of these experiments are formal academic-policy partnerships where implementing partners explicitly commit to following academic design mandates.

Over the past fifteen years many scholars and policymakers have slowly converged on examining and addressing intrastate conflict through the lens of development and governance policy. The United Nations Peacebuilding Architecture was established in the past decade with the mandate to work through other UN implementers to build peace through development and humanitarian type assistance. USAID has devoted many resources to implement development projects in the context of war-to-peace transitions. The World Bank, UK's Department for International Development, and others are also in on this business. In a large variety of direct and indirect ways, scholars have contributed to the implementation of development strategies in these contexts.

The point being that academic, policy-relevant work is occurring. But it is unclear that we have a solid understanding of just how much, in what form, or how consequential it is. And we may never know; as we discuss below we may be wiser to try to understand the precise conditions under which policy (makers, implementers, practitioners) pay attention to academic research even if in narrow settings but where we can establish sufficient control to identify such effects.

From Policy → Academy

As mentioned earlier, the task of identifying the influence of current events or policy trends on the academy is complicated. But again some published work and other anecdotal evidence suggests that the impact does occur. In just two examples, Diehl (2002) wrote that scholars of war often “chase the headlines” and Young and Findley (2011) demonstrate this trend in the terrorism literature as well.

Certainly funding availability has shaped the agendas of many researchers. In the field of international security, the Department of Defense Minerva initiative has spawned large and important research activities in a wide variety of contexts and has attracted some of the most promising security scholars to use the funding. The United States Institute of Peace, Smith Richardson foundation, and other funders have similarly motivated research.

Although we like to think of influence as positive, that is not always the case. Indeed, the impact evaluation improvements in recent years were motivated by policy practices that were not sound. Thus the policy world influenced the academy, but not necessarily by design. Scholars observed subpar practices and then began conducting research that would improve such practices. If the result is improved outcomes, however, the motivation for influence may be less consequential.

Academy to Policy and Back Again

We are aware of some academics that have left their academic posts to work in governments, IGOs, or NGOs. Jeremy Weinstein (Stanford), as previously mentioned,

is currently working as chief of staff for Samantha Power who is the US Ambassador to the United Nations but will presumably return to Stanford University afterwards. Michael Doyle left his academic posts to serve as Assistant Secretary General and Special Advisor to Kofi Annan. Michael Horowitz recently returned from a year working in the Department of Defense. Scholar-practitioners including Stephen Stedman and William Zartman also spent considerable time in high-level conflict resolution activities while also producing scholarly research at academic institutions. Other examples could be provided. That said, the vast majority of political scientists that begin work in the academy simply stay in the academy. And those that begin in the policy world typically stay there.

Academics and policymakers/practitioners work together in other ways, however. Experimental field research, much of it on development and conflict, relies on close academic-policy collaborations. Non-governmental organizations, or inter-governmental organizations, seek to carry out large-scale projects but need assistance from scholars who have expertise in designing proper interventions. Thus, numerous partnerships are borne with examples ranging from USAID to the International Rescue Committee to small NGOs such as Deniva in Uganda. Thus, there are most likely many different models that would facilitate cross-fertilization of ideas.

Using TRIP (and Other Methods) to Assess the Links

For purposes of this conference, we examined the various TRIP surveys provided by the conference organizers (Maliniak et al 2011; Peterson et al 2013). In comparison to other issue domains, the data are sparse on intrastate conflict. We thus preface this section by mentioning the tentative nature of the discussion. As intrastate articles make up a reasonable portion of the data (see below), we expect that future analyses in this domain will expand on this initial treatment.

The TRIP Journal Data

We conducted several sets of analyses with the journal data. First, we examined patterns using the intrastate conflict substantive focus variable. Second, we used the terrorism substantive focus variable. Third, we combined the two and also conducted other subgroup analyses on whether these literatures incorporate policy prescriptions.

A tabulation of the “Intrastate Conflict” variable shows that 602 of the 5,306 articles coded have a substantive focus on intrastate conflict, amounting to about 11% of the articles. Figure 1 illustrates the percentage of all articles in the database that are devoted to intrastate conflict over time. The graph illustrates the dramatic growth in studies of intrastate conflict in the wake of the end of the Cold War, consistent with

what is often argued anecdotally. It is remarkable that from about 2000, nearly 20 percent of the articles published in the major IR journals were on intrastate conflict.

[Figure 1 about here]

We next identified one other variable, “terrorism”, which could be picking up a type of intrastate conflict. A tabulation of the “terrorism” variable shows that 213 of the 5,306 articles coded are substantively focus on terrorism, amounting to about 4% of the articles. Figure 2 illustrates the percentage plotted over time. Consistent with what many expect, there appears to be a significant increase in studies of terrorism post 9/11 (Young and Findley 2013).²⁰ We note that the articles coded for terrorism could be picking up terrorism internationally or domestically. Thus, in order to separate out types of violence for the scope of this paper, further work would need to be conducted to learn whether the terrorism captured in these articles refers to intrastate conflict. A quick cross-tabulation shows that 32 of the 213 terrorism articles are also coded as intrastate conflict, perhaps reflecting the disciplines emphasis on transnational terrorism until recently.

[Figure 2 about here]

Given that terrorism is often a strategy employed in intrastate conflict (Findley and Young 2012), we also pooled the two together and show the percentage of all articles in the database on terrorism and intrastate conflict. Strikingly, in 2011 over 25% of all articles are on one of these two topics. Whether the academic studies translate into policy influence is another matter, but at very least there is significant emphasis on these topics that should provide opportunity for policy impact.

[Figure 3 about here]

To provide another point of comparison, we plotted intrastate conflict studies and terrorism studies relative to studies of interstate war. Figure 4 shows the results for academic studies over time. As expected, studies of interstate war dominate over intrastate conflict and terrorism (separately). While this is somewhat expected, it is curious that studies of interstate conflict continue to dominate until at least 2011 when so much emphasis in recent years is placed on the empirical regularity that interstate wars rarely occur whereas interstate conflict is rife throughout the world. If the increased attention to intrastate conflict has led to an increase in studies, which the graph shows, there nonetheless appears to be a substantial lag in academic studies (assuming the percentage of studies should be relatively proportional, which very well may not be the case).

[Figure 4 about here]

²⁰ Many of these are in the quantitative tradition (Young 2014).

We now turn to a consideration of different methods in the study of intrastate conflict and terrorism. Figures 5 and 6 illustrate which methods have been used over time, limiting the methods just to the categories quantitative, qualitative, and formal modeling. While there is substantial variation prior to the year 2000, quantitative methods are now used much more extensively to study intrastate conflict. The trends for terrorism are less pronounced, though quantitative studies also appear to dominate especially recently.

[Figure 5 about here]

[Figure 6 about here]

Finally, we examined what percentage of intrastate conflict and terrorism studies offer policy prescriptions. Figures 7 and 8 show that the percentage varies widely; they do not provide any weighting for the number of articles per year, however. Thus the low percentages in the late 2000s represent far more articles with policy prescriptions than the higher percentages in the early 1990s.

[Figure 7 about here]

[Figure 8 about here]

Moving beyond the substantive focus variables, the TRIP journal data has an issue area variable. This variable captures a higher level of aggregation with the most relevant alternative for us being “International Security” and thus does not allow us to distinguish intrastate from interstate or other types of conflict.

The TRIP Survey Data

The survey data is not as helpful as the journal data for analyzing our issue area. The survey data codebook (Malianiak et al 2011) offers some insight as to why. Under the “Research Interests Non-Qualifiers”, the category “Ethnic Conflict” is excluded from consideration. While ethnic conflict and intrastate conflict are not exact substitutes, they overlap considerably in the ways scholars identify research in this area.²¹

²¹ The exclusion, however, is curiously inconsistent. Also the category of “Human Rights” is listed, just above Ethnic Conflict, as one that does not qualify. And yet Human Rights is listed as an option for research interests in questions such as “fsqg_708: *What are your secondary areas of research within IR? Check all that apply. [One possibility is:] fsqg_708_402: Human Rights.*”

Despite excluding much of the substantive focus on intrastate conflict, the survey data contain some questions that are suggestive for our area. As with the journal article data, the area of terrorism receives significant attention in the survey. It is a possible category for teaching and research interests; as well, scholars are asked whether 9/11 influenced their teaching and research interests.

According to the 2006 survey, 9/11 changed the issue areas covered in class quite dramatically; less so, it also changed the geographical focus and theoretical approaches. 85% of the respondents said that 9/11 significantly or somewhat changed the issue area of the course. 57% report significantly or somewhat changing geographic focus and only 40% report significantly or somewhat changing the theoretical focus.

According to the 2004 survey, 9/11 affected research agendas in non-trivial ways, and the pattern is similar to that of teaching interest changes. 56% report somewhat or significantly changing the issue areas covered in their research agenda, 37% report somewhat or significantly changing the geographic focus, and 30% report somewhat or significantly changing the theoretical approaches.

In addition to the questions about terrorism, the survey also asks about the most pressing foreign policy issues facing a given country. In these questions, a number of possibilities relate to intrastate conflicts in other countries such as “Ethnic Conflict”, “Genocide in Sudan”, “Failed States”, and “Arab Spring”. Interestingly for 2011 (fsqg_879), the most pressing foreign policy issues ranged from the Global Debt Crisis to Global Climate Change, but with Arab Spring (largely an internationalized intrastate conflict situation) ranking second of all the issues. Other traditionally intrastate issues such as ethnic conflict ranked much lower.

We considered the descriptive data on which scholars most influenced others’ scholarly research (fsqg_1092). Of the list, the only scholars who have worked a considerable amount of time on intrastate conflict that garnered more than 10 votes were: David Lake, Charles Tilly, J. David Singer, and James Fearon. And while at least three of these have worked extensively on intrastate conflict, they are better known for their contributions to the interstate war literature.

We also considered the individuals listed as having the greatest influence on US foreign policy in the past 20 years (fsqg_1109). The list is similarly short for intrastate war scholars, with Michael Doyle moving up the list and the others mentioned above moving down. It is thus quite possible that intrastate war scholars are not influential in the academy or in policy arenas. But it could also be the case that the area has only garnered more attention in the last 20 years. It is probably some combination of these and other factors. Unfortunately, the survey instrument is framed in a way that it excludes a close consideration of these issues.

The 2011 Policymaker Survey

We next examined the 2011 policymaker survey and find that very few of the questions speak to the topic of intrastate conflict. By construction, the survey privileges interstate issues and is therefore not as helpful for our purposes. The primary exception relates to the questions on counterinsurgency, a topic that is often put under the umbrella intrastate conflict category.

Question 12 of the survey asks whether respondents are familiar with “population-centric counter-insurgency” theory. According to Avey and Desch (2013), 70% of respondents were familiar with the concept (the 3rd highest behind mutual assured destruction and clash of civilizations) and many thought it was a sound and useful concept (Questions 12b, 12c, 12d).

In most of the questions, the response proportions for COIN were not all that different from the responses to the other categories. Perhaps the most telling statistic for the COIN question is how useful the theory is for the work the respondent does for the U.S. Government – 65% agreed that it influences their work, which is the highest proportion of any of the possibilities. This could be a function of many factors, but without demographic information about respondents it is difficult to know how to contextualize this figure.

All said, the policymaker survey is interesting and the level of access difficult to have otherwise. But the reach does not allow us to speak confidently about policymaker/practitioner views on the topic of intrastate conflict. Additionally, the works suggested to the policymakers are fairly dated. There is no mention, for example, of bargaining theories of war, which arguably dominate the study of international security at least since the early 2000s but perhaps back to 1995 (Fearon 1995). On one level, this makes sense as it is useful to probe which of the big academic studies has had a lasting impact. On another level, however, one of the promises of the strengthening the link initiative is to allow academics and policymakers to learn from each other in the short-term in ways that wisely harness academic insights for policy response.

The TRIP Snap Poll Data

Curiously, whereas the TRIP journal database contains very little coded information on intrastate conflict, and the survey likewise asks very little, the Snap polls are almost entirely about international involvement in intrastate conflict. Of course larger dynamics are in play; namely, U.S.-Russian relations in both cases. Even so, arguably the locus of interest remains the political and humanitarian conflicts in these countries.

The Snap polls provide a potentially interesting link between academics and policymakers. Namely, they allow the aggregation of academic opinions almost

immediately as events unfold, along with the transfer of these ideas to willing policymakers who could act on the information. In both Snap polls, a good number of the questions focus on predicting what will happen in Ukraine and Syria. A much smaller proportion focus on harnessing academic insights about what the U.S. and international community should do. The emphasis on prediction in many of the questions raises the issue: what it would mean for a policymaker to view the responses and then change policy.

First, to address the issue of whether academic insights affect policymaking, we have to assume that scholars' beliefs represent academic insights. Many of the Snap poll items ask questions such as "If X, *do you believe* Y". The polls appear to make the assumption that academics form their beliefs based on scholarly insights and express precisely the beliefs that represent the academic line of thinking on a topic. But this is a strong assumption. Research in psychology and related disciplines demonstrates how beliefs are complicated in a multitude of ways. If a group of scholars think the U.S. will intervene, it could be for all sorts of reasons, including ideology or availability bias (Tversky and Kahneman 1973), perhaps unrelated to scholarly theories and evidence.

Second, if we do assume that beliefs represent the theoretical expectations from the academy, we then have to ask which insights. On the question of whether the U.S. would use military force against Syria if Syria fails to comply, there could be a variety of theoretical models that make different predictions. Which of those theoretical models has the most validity, and is accepted as such? Explicitly tying questions to scholarly theories might make the link clearer for the academics connecting to their beliefs, but also to policymakers who might make a decision based on the information and desire some sort of explanation for the mechanism underlying scholarly beliefs.

The questions that ask respondents to comment on what *should* be done provide more actionable information for policymakers, but they still leave unclear why certain actions should be taken. This is not to say that those reasons need to be included; we are simply making the point that any academic influence, which could be real, may be only thinly based on academic theories. In contrast, if scholarly work is disseminated and acted upon, the theoretical and empirical underpinnings may be more apparent to policymakers and practitioners who adopt those insights.

Finally, while the Snap polls were undoubtedly well vetted before going into the field, some of the questions are nonetheless confusing and wording could account for different response. For instance, on the question of whether the Russian military would intervene in Ukraine, the term "intervention" admits so many possibilities ranging from sending some covert economic assistance to having the Russian Army roll in with tanks. This ambiguity confused at least one of the authors of this paper.

Discussion of the TRIP data

Taking a step back, what roles should the TRIP datasets play? Clearly they cannot cover questions of interest to everyone. And they do not (cannot) speak definitively to the issue of whether academics are influencing policy. But they offer some insights into some possible associations between scholarly and policy activity in some domains. For intrastate conflict, the takeaways are likely not as clear as they may be in other issue areas. What they do offer is an important starting point for the larger conversation.

Moving forward, a few challenges are apparent. There are some notable omissions related to intrastate conflict in the TRIP studies. Some may be partially recoverable through keyword searching, but may merit greater attention moving forward. Namely, almost no attention is given to the peace-(making, keeping, building) literature. If one were to think about policy decisions the U.S. and other countries consider on a regular basis, many (most?) of them involve making decisions about peace operations. Libya, Syria, Ukraine, and recently Iraq, to name a few, all had peace operations at stake. Indeed, for most developed countries interstate wars are highly uncommon; and yet many of these countries are active in peace operations worldwide.

Perhaps the greatest challenge of all is to connect the various types of data in ways that allow credible identification of the impact of scholarly work on policymaking. This will remain a pernicious problem but one worth considering closely so that unique approaches can be developed to address shortcomings.

Conclusion

A larger examination of the TRIP data and discussion of the policy and scholarly nexus leads us to a few points. First, and to summarize this paper, data on trends in scholarly and policy work for intrastate conflict is spotty. Thus, we have introduced many more ideas than we've been able to provide any closure on. Much remains to be done and will require substantial attention from the academic and policy communities. We feel the data and this paper are an opening salvo, but we do not think that sufficient progress has been made to make credible recommendations.

Second, we understand that lines have to be drawn somewhere and that for this conference those lines surround political scientists. But to fully understand the relationship between scholarship and policy on intrastate conflict, one would need to travel across the borders and examine what others are doing. Other academics, such as economists, lawyers, anthropologists, global health scholars, sociologists, mathematicians, and physicists, are all working on intrastate conflict and are contributing to policy discussions. One of the most famous, John Yu, who worked in the US Department of Justice, authored some of the detention and interrogation

policies in the Bush administration while being on leave from his law professor position at the University of California, Berkeley.

Third, one observation that has become clear in this paper is that although intrastate conflict is clearly the dominant form of conflict in the world, and has been for a long time, scholars and policymakers remain predominantly focused on interstate war. The empirical observation about trends in intrastate and interstate conflict is not novel (See Moore 2006). But we were nonetheless surprised that so little of the TRIP survey, policymaker survey, and more general attention continues to lag far behind the empirical dynamics of conflict in the world.

Fourth, in our race to strengthen the links, it may be important to ask ourselves whether such strengthening could do damage. Clearly there is tremendous variation in the quality of scholarship that makes it into the public domain. Do we need to set standards of what constitutes a credible evidence-base for policy-making? Such standards may not be possible in today's climate, but the practicalities do not invalidate the question. We, for two, would be worried about much (most?) academic work making it into the policy sphere without passing significant validity, reliability, and identification checks.

Finally, we wish to draw attention to the sticky issues of demonstrating a causal effect of scholarly work on policy activity. There are many ways that the observational analyses produced for this conference could understate or overstate the impacts, and it is conceivable that the inferences could be completely wrong. We discuss some possible concerns and approaches in the Appendix and more generally encourage any follow-on work to take causal identification seriously.

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Figures

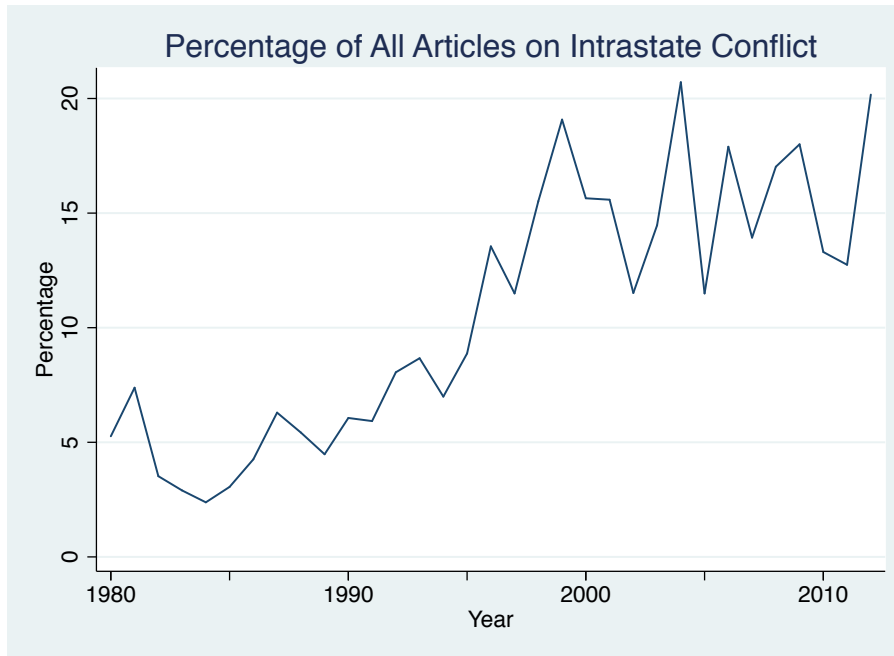


Figure 1: Plot of the percentage of all articles in the TRIP Journal Database (Peterson et al 2013) on the topic of intrastate conflict.

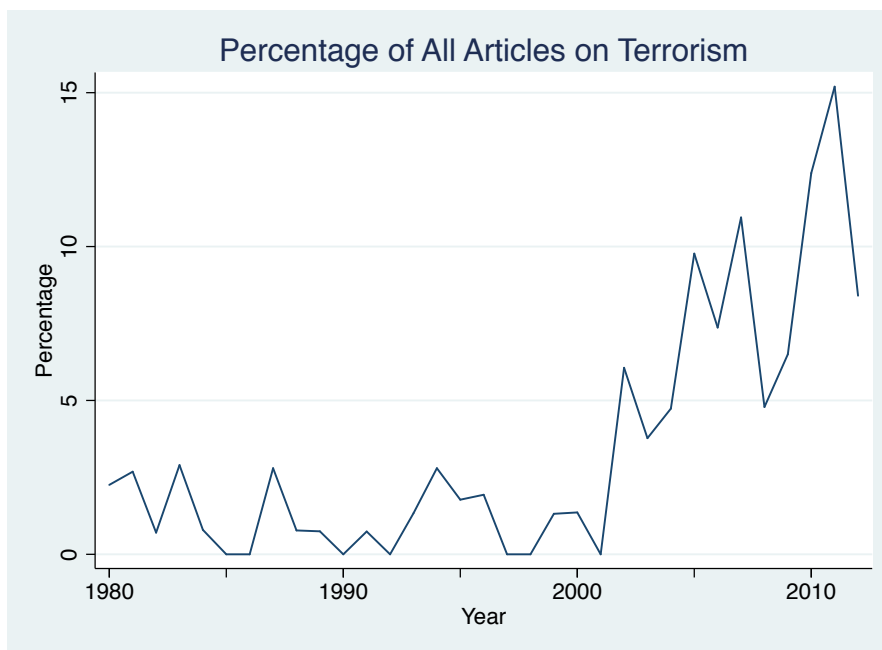


Figure 2: Plot of the percentage of all articles in the TRIP Journal Database (Peterson et al 2013) on the topic of terrorism.

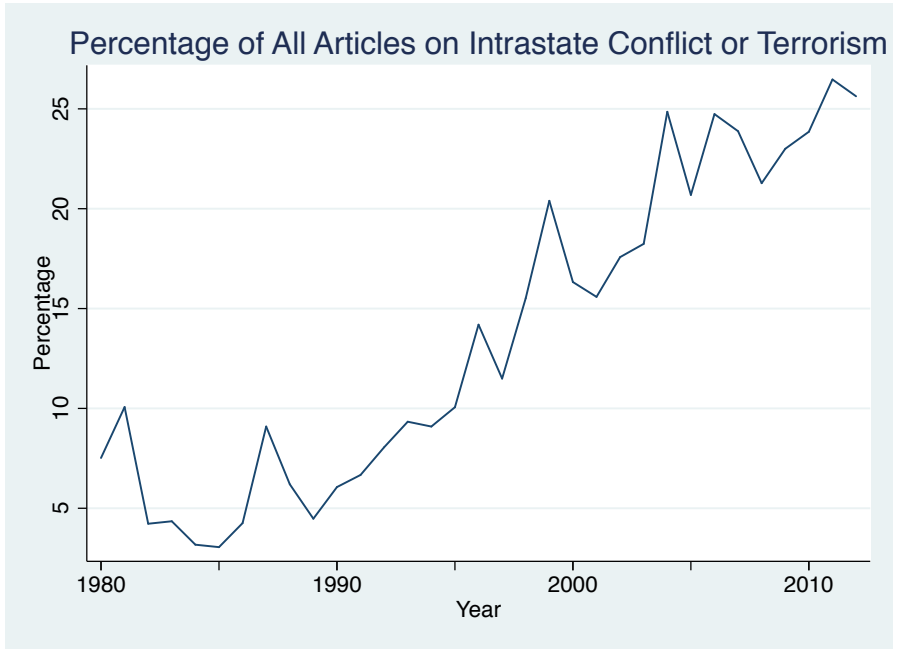


Figure 3: Plot of the percentage of all articles in the TRIP Journal Database (Peterson et al 2013) on the topics of intrastate conflict and terrorism combined.

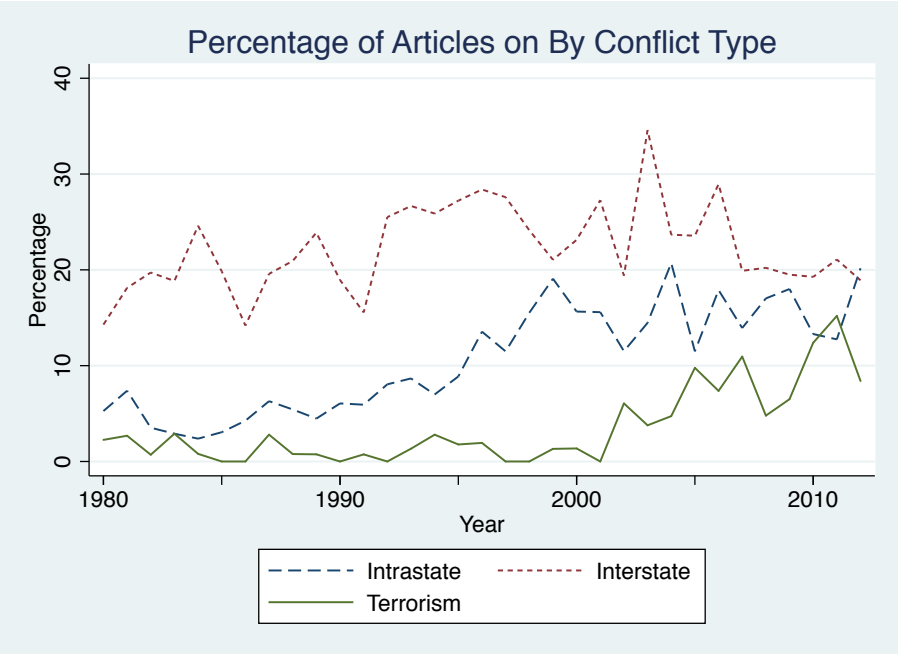


Figure 4: Plot of the percentage of all articles in the TRIP Journal Database (Peterson et al 2013) on the topic of intrastate conflict compared to interstate conflict and terrorism.

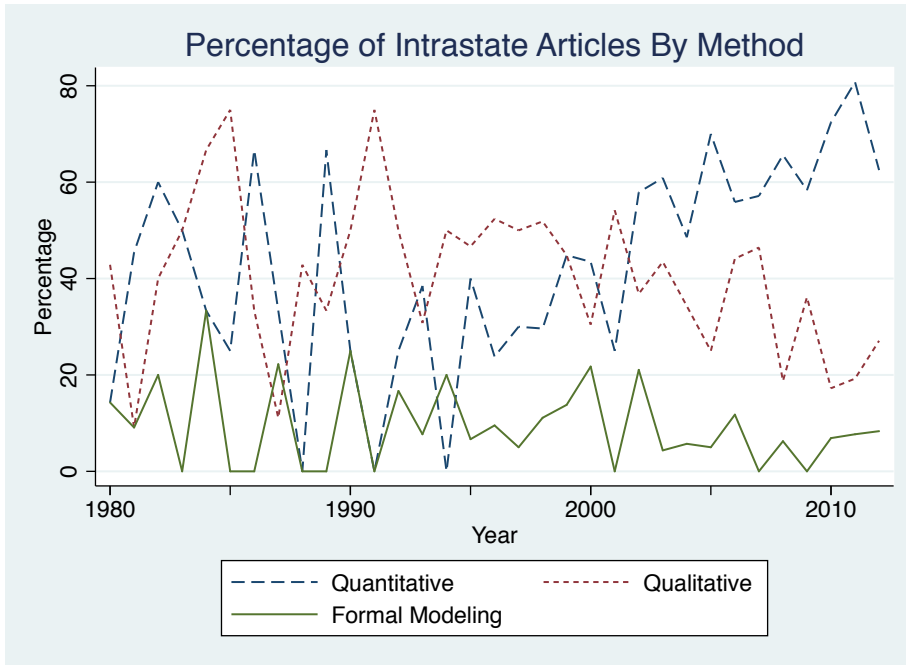


Figure 5: Plot of the percentage of all intrastate conflict articles in the TRIP Journal Database (Peterson et al 2013) by three methods.

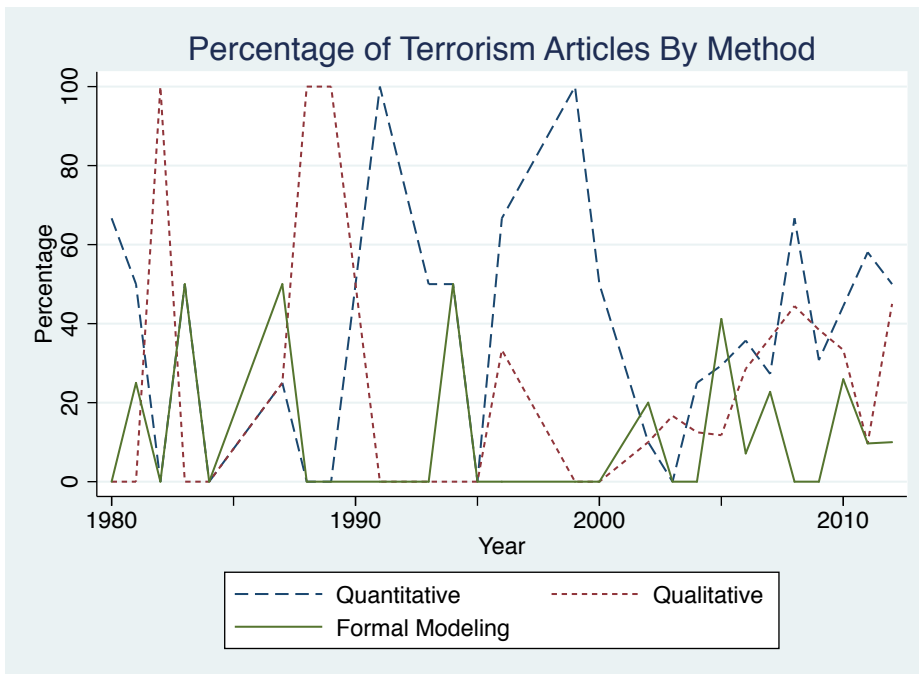


Figure 6: Plot of the percentage of all terrorism articles in the TRIP Journal Database (Peterson et al 2013) by three methods.

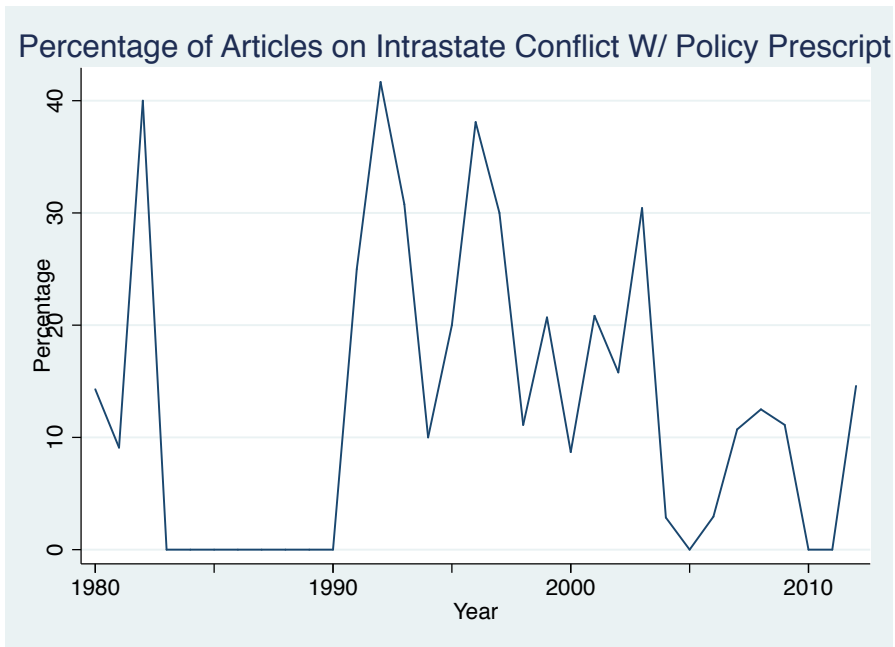


Figure 7: Plot of the percentage of all intrastate conflict articles in the TRIP Journal Database (Peterson et al 2013) that offer a policy prescription.

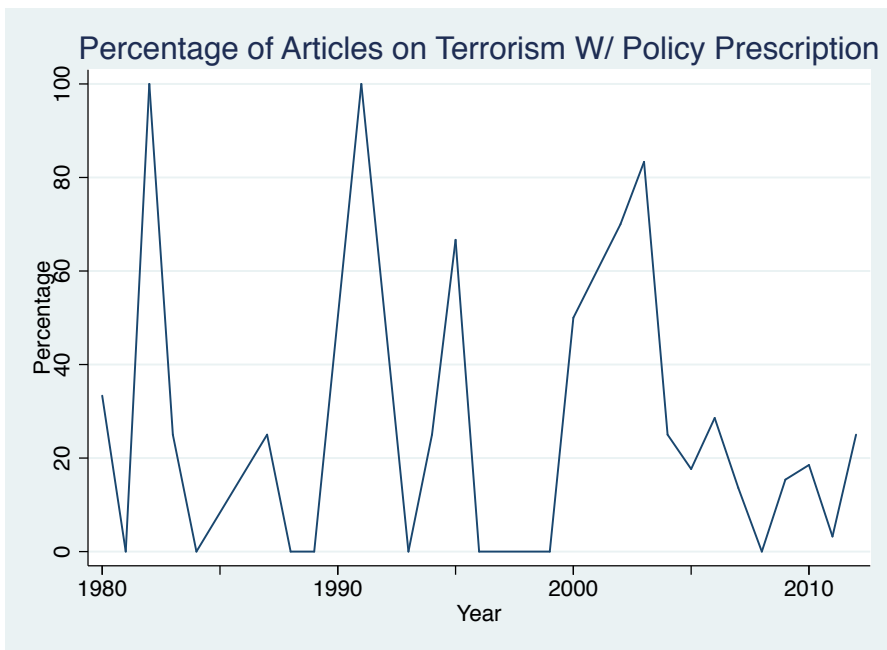


Figure 8: Plot of the percentage of all terrorism articles in the TRIP Journal Database (Peterson et al 2013) that offer a policy prescription.

Appendix

The TRIP data – on journals, scholars, policymakers – are an excellent start. There is no better, or even comparable, source of data currently available. They should provide some interesting insights into broad patterns in publishing trends and individual's attitudes. But as with most observational data there are some important limitations that will always be difficult to surmount, and that could threaten valid causal inference. Using these data, the conference organizers have opted to pursue an observational approach emphasizing broader trends, challenges, and so forth.

We still wish to call attention to the extreme difficulties of inference from this approach. Not only could the contributors to this volume miss important nuance, but they could actually reach precisely the wrong conclusions due to the problems of observability, selection, and endogeneity. The challenges are myriad, and it would be improper to assume we could solve all of them. With this in mind, we ask why not supplement the study of large-N correlations and anecdotes with randomized experiments in much narrower, but perhaps more manageable domains? If carefully designed, an experiment might allow more careful comparison as well as specifically designed interventions and outcomes, which could facilitate the identification of causal effects. And with some careful consideration, interventions could be designed that could uncover causal mechanisms as well. In the event that the experimental evidence is consistent with the larger observational trends, it would add further validity to the findings herein.

Before turning to a type of intervention, we consider a couple of issues. Who would be the subjects? While it would be nearly impossible, and probably not desirable, to experiment on cabinet-level political officials or military leaders, there are many individuals who fall into the category of policy implementers or practitioners. Is randomization of these subjects into different conditions really possible? Why not? If there are enough individuals, there is no reason why a given intervention could be assigned to some subjects while a different intervention is assigned to another. One could perhaps make an argument rooted in the ethics of withholding important information, but we cannot imagine how that could be compelling in this case.

So what would be the interventions? There are many possibilities. We discuss one possibility in the context of an experiment that one of us conducted (Brigham et al. 2013) recently. We wanted to know whether microfinance institutions (MFIs) would update their beliefs based on viewing scientific findings. Like many non-governmental NGOs, we suspect that many MFIs “believe” their work matters thus and operate on a commitment to that belief rather than based on evidence of impact.

What would happen if we presented some MFIs with rigorous scientific evidence showing that MFIs are successful? And what if we presented other MFIs rigorous scientific evidence showing the opposite? We conjectured that those receiving the

negative findings would be less inclined to update when compared to those receiving the positive findings.

As it turns out Dean Karlan and colleagues at Innovations for Poverty Action (IPA) published two separate papers, one showing positive results and the other showing negative results. Fortunately, the experiments they conducted used the exact same design, but applied the design in different locations. And both were published in reputable academic journals. Thus we had an intervention with most details held constant except the opposite findings.

In this particular study, we randomly assigned over 1,400 MFIs globally to the positive or negative condition and then observed whether they responded to our emails about a possible partnership to evaluate their program. We could not easily observe their programs to see what changes they implemented, which would have been a useful outcome. Instead, we simply asked them to take a costly action that could seriously affect their operations moving forward.

Consistent with what we expected, when MFIs received negative information they were far less likely (statistically) to take steps to pursue additional collaboration and evaluation than those that received positive information. In this study, we did not provide evidence that scholarly work changes policy; but nonetheless the results speak to the conditions under which academic research may be discounted or adopted in some way.

In another project still in the design stage (Findley et al. 2014), we have collected data, including contact information, on members of parliament in about 60 countries. With those data, we will approach members of parliament with various types of information about development to observe how they will react. More details to come on this experiment, but the point is that we have a (non-random) sample of over 15,000 members of parliament to whom we can randomly assign interventions to learn about their behavior.

One could imagine still other projects that collect data on large numbers of policymakers, implementers, or practitioners and introduce manipulations randomly in order to learn more. Such an exercise would not yield data amenable to large-N correlational analysis, but could provide much more precision in the measurement of key variables and identification of causal effects.

The road goes both ways. There is not reason why experiments could not be used to study scholars. As we understand from the DC conference, the TRIP project has at least considered embedding experiments in their surveys though they did not admit to as much. If they are not being used in rigorous ways, we propose that the organizers consider doing so in the next rounds.

In the interest of time and space, we will not expound too much further. We simply want to make the point that the existing TRIP efforts will likely lead to some useful

descriptive trends for comparison, but they may also be misleading and perhaps just wrong. Ultimately it will be difficult to know for certain without more rigorous methods. We may thus need to consider alternative methods such as experiments if we are to learn better whether academics influence policy or vice versa.