

THE WEAKEST LINK? *Scholarship and Policy on Intrastate Conflict*

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How do theory and practice interact in the critical domain of intrastate conflict? When events, such as the Syrian civil war, spiral for years, do policymakers consult academics for 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? When thinking about strengthening the links between the policy and academic worlds, these are but a few questions to consider. A central task in this chapter is to examine various Teaching, Research, and International Policy (TRIP) datasets to learn about the role of intrastate conflict in the study of international relations (IR) and to relate this to the role scholars have played in the policy process.

Our core argument is that scholarly work has had modest direct influence on policy and likely small, although difficult-to-detect, indirect influences. Much of the intrastate conflict occurring in the world has taken place in geographic areas where policymakers have historically paid little attention. Africa, for example, represents about 15 percent of the global population. In the TRIP article database, however, only 5 percent of all articles are related to Africa, and only 5.5 percent of faculty in the thirty-two countries included in the 2014 TRIP faculty survey select Africa as their primary focus. Although violence in Africa may be of less concern to policymakers than violence in other parts of the world, 17 percent of the intrastate conflict articles in the TRIP database have an Africa focus. That said, Africa has experienced the largest share of the world's intrastate conflict, at least since World War II.

Even during the Cold War, much of the violence in Africa and Asia drew relatively little attention. Intrastate conflict has become more important in the academy over the last ten to fifteen years, and that interest has largely been fueled by an increase in analysis of large-N databases. Political science (comparative politics, specifically) has a somewhat longer tradition of studying revolution, but there have been few conflict studies groups to propel its research into the mainstream of political science, much less the policy world. Thus it would be unrealistic to expect that the intrastate war literature would have a strong effect on policy. Finally, what little covariance appears to exist comes from the literature on insurgency and development, although it is unclear how much scholarly work drives policy as opposed to the two co-occurring with no obvious causal link.

Turning to the question of whether the policy world has had an impact on scholars, we argue that scholarly work has integrated few insights from the policy community. It is far from clear that scholars pay attention to the decision calculus of policymakers in the United States, other western countries, or international organizations when developing their research claims. Growing numbers of collaborations between academics and practitioners, however, have led to greater insights in the peacebuilding and other literatures.

That said, scholars are often influenced by current events and study temporary conflict processes. In the 2014 TRIP faculty survey, 62 percent of respondents answered in the affirmative when asked if they have increased their research related to a critical event, such as September 11, 2001. In fact, 54 percent of faculty answered "yes" when asked if they have responded to a major world event by seeking to make their work more relevant to policy practitioners. Unsurprisingly, of all major world events listed, 9/11 had the biggest impact on scholars' research, with about 42 percent of respondents citing this incident. The Arab Spring and the fall of communism were ranked next highest at 25 percent and 23.45 percent, respectively.

In this chapter we first describe the scope of the intrastate conflict issue area and employ qualitative evidence to describe the evolution of this subfield. Second, we introduce various TRIP datasets and discuss their strengths and weaknesses in further describing the evolution of this subfield and its relationship to the policy community. Third, we evaluate several arguments that are described in chapter 1. We find suggestive evidence that is consistent with the arguments that emerging issues (argument 1a) and more technical issues (argument 1b) provide greater opportunities for IR scholars to influence policy than do more long-standing and less technical issues. The demand for applied scholarship on terrorism and intrastate conflict increased dramatically after 9/11, when policymakers were desperate for more/better evidence and for more sophisticated models that would help them forecast conflict events at the subnational level. We also present evidence that speaks to two additional arguments presented in chapter 1: first, that the opportunities for scholarly engagement and influence will be more limited in issue areas in which another epistemic or disciplinary community already has an established working relationship with practitioners than in issue areas in which no such relationship exists (argument 2a) and, second, that IR scholars are more likely to directly influence policy in issue areas in which policy decisions are made frequently and at relatively low levels (argument 2b).

In the post-9/11 environment, policy implementers sought insights from new multidisciplinary teams of researchers when they lacked in-house expertise. Large contracts were awarded to IR scholars to conduct research on a weekly or daily basis for combatant commanders who were tasked with implementing policy. These were not consultations with the secretary of state but frequent forecasts for practitioners tasked with making tactical decisions or providing intelligence assessments.

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scope of the intrastate conflict to describe the evolution of this subfield. We present our datasets and discuss their implications for the evolution of this subfield. Third, we evaluate several arguments and present suggestive evidence that is consistent with the idea that the study of intrastate conflict is an important and distinctive area of IR research. We also present evidence that suggests that the study of intrastate conflict is an important and distinctive area of IR research. We also present evidence that suggests that the study of intrastate conflict is an important and distinctive area of IR research.

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Finally, we discuss the type of data collection efforts and research designs that would be helpful in pursuing a second generation of empirical research on the relationship between IR scholars and policy practitioners. Although the evidence collected for this book provides a strong foundation, and the tentative explanations advanced herein provide a plausible starting point, we all have more work in front of us if we are to take this research agenda on the theory-practice nexus as seriously as we take our individual substantive research agendas.

STATE OF THE FIELD

For the past few decades, events such as the Rwandan genocide, the 9/11 attacks, and the Arab Spring have pushed the study of conflict beyond traditional areas of great power disputes and militarized interstate conflicts. After the end of the Cold War, events such as the crisis in Somalia and the breakdown of Yugoslavia drew scholars into studying substate processes. This development was further enhanced in the late 1990s by the upsurge of quantitative *civil war* research, which encouraged even more work in this area.

A tabulation of the *intrastate conflict* variable in the TRIP article database shows that 602 of the 5,306 articles coded have a substantive focus on intrastate conflict, amounting to about 11 percent of the articles. Figure 14.1 illustrates the percentage of all articles in the database that are devoted to intrastate conflict

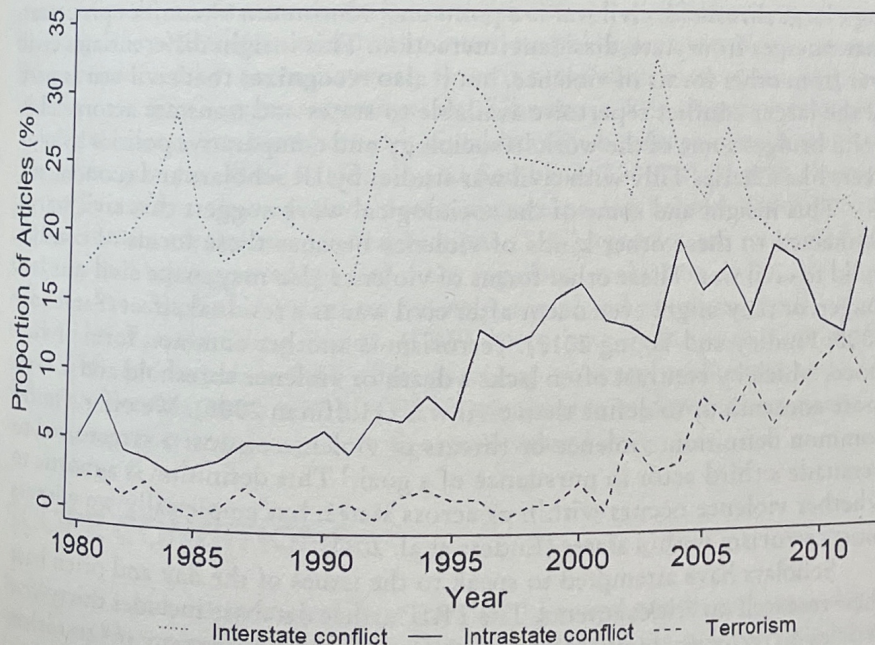


FIGURE 14.1. Plot of the percentage of all articles in the TRIP journal database on the topics of intrastate and interstate conflict and terrorism

the TRIP journal database). A smaller fraction, thirty-two of 5,306 articles, is listed as intrastate conflict *and* terrorism. Even though a majority of terrorist attacks are domestic or intrastate in nature (Sánchez-Cuenca and De la Calle 2009), it seems scholars see terrorism as more of an IR than a comparative politics topic. Whereas civil war, terrorism, and other intrastate conflict have been studied as “islands of inquiry” (Most and Starr 1984), more recent work is connecting these islands by recognizing the overlap and affinities among the types of violent contention (Findley and Young 2012; Thomas 2014; Fortna 2015).⁵

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 takes an actor-centric approach and considers the political or humanitarian reasons why third parties intervene (Findley and Teo 2006; Gent 2007). From the perspective of policy influence, the humanitarian approach is more relevant. Indeed, the scholarly literature on civil wars seems to be less connected to policy discussions relative to the peace studies literature.

What we are calling the *peace studies literature* examines a number of dimensions including peacekeeping, peacemaking, peace building, 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 discussion of each type of peace intervention is beyond the scope of this chapter, 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 Russian aid convoy into Ukraine in 2014 is a prime example.

Peace operations have been criticized heavily, but there is evidence that they often may be effective (Doyle and Sambanis 2006). And even though the organizations that conduct peace operations lag behind development organizations, even many United Nations (UN) agencies are interested in learning and trying to adopt the more rigorous evaluation standards that scholars have applied. One of us recently completed an impact evaluation of the UN Peacebuilding Fund’s (PBF) operations in Burundi (Campbell et al. 2014), conducted the PBF’s first quasi-experimental evaluation, and had follow-up conversations with the PBF’s monitoring and evaluation specialists, who are now requiring more rigorous evaluations.

SCOPE CONDITIONS

In chapter 1, the editors identify a number of scope conditions, including what is a policy prescription, who are the key actors in the policy world, and how one would recognize policy influence. We will not rehash their arguments, but we provide a few additional thoughts here.

We begin with 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. The reviewer said that the prescriptions must be there in order to endorse publication. The reviewer's instructions (paraphrased): two paragraphs would be sufficient, and the authors should simply communicate to "the policymakers" how they should redirect the country's activities based on the research findings.

The flawed logic suggests that scholars 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 explain how policies should be changed. And the "policymakers"—presumably a Hillary Clinton or Mike Pompeo—will be paying attention and change things.⁶ The standard academic policy prescription that we caricature is obviously insufficient. However, this sort of hastily 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 who are least engaged are the ones who offer blunt policy prescriptions in articles, whereas those who are more engaged find much more subtle yet effective ways to communicate lessons learned. This is consistent with Parks and Stern's (2013) finding that IR scholars who take time during their careers to work full-time in a policy position return to academia with a new outlook; they devote more time to publishing in policy journals such as *Foreign Affairs* and *Foreign Policy*. In this case, 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. We surmise, given that academics do not receive training in policy analysis, that policy prescriptions embedded within articles do not track closely with authors' empirical evidence. After all, if only a few other academics will read the paper, it is costless to give some advice to a Putin or a Merkel. Moreover, scholars may not have the appropriate skills or experience to derive a prescription that appropriately speaks to the policy world. A more in-depth qualitative review of policy prescriptions might be an important next step for researchers interested in the policy-academic nexus.

As discussed in chapter 1, some academics have taken to blogs, op-eds, policy briefs, and numerous other media to share lessons from their work. In our own area, *Political Violence at a Glance*, for which one of the authors is a contributor, may be the most prominent blog forum for disseminating academic work on intrastate conflict. Because policymakers, staff, or advocates can weigh in on such discussion through online fora, these approaches may provide different, and potentially more relevant, policy engagement.

However, similar questions about the impact of advice rooted in strong evidence.

Whether academics should be engaged in this way. In chapter 1 the editor argued that although we would point to the impact that the spectrum is so important that academics need to be engaged in this way. Would we count the impact of engagement with the informal advising, number of publications, or the person is involved in the project?

At American University, the School of Public Service and the School of International Studies related discussions as evidence of the university and the impact of the scale described in the rankings. Exact measures of impact to be worked out more carefully. For example, is better evidence of the impact of a report. For intelligence studies, the highest honor (similar to the highest honor in political science).⁸ In the future, where impact is considered.

Most important, an understanding of the relationship between academics and policymakers. (2002) argues, policymakers should not falsify a theory. Most academic work is supported or refuted by evidence. To do a better job communicating the impact of research. For instance, quantitative research is digestible and makes a good case for policy change, but is elementary, not a substitute for a more in-depth analysis.

These issues give rise to questions about the impact of policy impact is difficult to measure. Advocates in Washington, D.C., can we infer that change, the change was brought in some academic work. The exercise with many policymakers. We should be cautious about what we are doing.

We examined the impact of policy impact (2014), as well as the impact of the academic-policy nexus. The impact of policy impact is relatively sparse on impact.

However, similar questions apply: to what extent does this work offer policy advice rooted in strong evidence?⁷

Whether academics are clueless or savvy, influence could occur either way. In chapter 1 the editors identify a useful spectrum of policy influence, although we would point out (and do not think the editors would disagree) that the spectrum is something more of a heuristic for identifying relative impact that academics might make. How would we then measure this impact? Would we count the number of times a person or article is cited in congressional testimony? Would we count and weight the strength and duration of engagement with the policy world by taking into account formal versus informal advising, number of meetings, how close to the top of the organization the person is involved, or any number of other ways?

At American University, especially within the School for International Service and the School of Public Affairs, 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. The scale described previously provides guidance in thinking about these rankings. Exact measurement is more complicated and would likely need to be worked out more completely. A 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 President's Daily Brief is the highest honor (similar to an *American Political Science Review* article in political science).⁸ In the intelligence world, at least, there is a parallel system where impact is considered and used for promotion and advancement.

Most important, 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 theory. Most academic theories are probabilistic and thus need to be supported or refuted based on the weight of evidence.⁹ Academics need to do a better job communicating the strengths and limitations of their work. For instance, quantitative scholars need to share their material in a way that is digestible and makes clear to policymakers that their work is often complementary, not a substitute, for area studies knowledge (Mack 2002).

These issues give rise to the one with which we began: measurement of policy impact is difficult. If an academic speaks to some policy staff or advocates in Washington, DC, or Geneva, and then we observe some policy change, can we infer that academic work was responsible? What if, for example, the change was going to occur anyway, 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 that we need to be cautious about what we conclude.

We examined the 2011 and 2014 TRIP surveys (Maliniak et al. 2012, 2014), as well as the TRIP journal data, to understand various dimensions of the academic-policy nexus. Compared to other issue domains, the data are relatively sparse on intrastate conflict.

THE TRIP JOURNAL DATA

We conducted several analyses with the journal data. Figure 14.1 examined patterns across articles for which the substantive focus variable has been coded as intrastate conflict, as well as the terrorism substantive focus variable to which we now turn. We also combined the two and conducted other subgroup analyses on whether the literature incorporates policy prescriptions.

The category of substantive focus, terrorism, could pick up a type of intrastate conflict. A tabulation of the *terrorism* variable shows that 213, or about 4 percent, of the 5,306 articles coded are substantively focused on terrorism. Figure 14.1 also illustrates the percentage plotted over time. Consistent with what many expect, there appears to be a significant increase in studies of terrorism after 9/11 (Young and Findley 2011).¹⁰ We note that the articles coded for terrorism could be picking up terrorism internationally or domestically. Thus, to separate types of violence for the scope of this chapter, further work would need to explore whether the terrorism captured in these articles refers to intrastate conflict. A quick cross-tabulation shows that thirty-two of the 213 terrorism articles are also coded as intrastate conflict, perhaps reflecting the discipline's emphasis on transnational terrorism until recently.

Because terrorism is often a strategy used in intrastate conflict (Findley and Young 2012), we also pooled the two to show the percentage of all articles in the database on terrorism and intrastate conflict. Strikingly, in 2011 more than 25 percent of all articles are on one of these two topics. Whether the academic studies translate into policy influence is another matter, but at least there is sufficient emphasis on these topics to provide opportunity for policy impact.

To provide another point of comparison, we plotted intrastate conflict studies and terrorism studies relative to studies of interstate war. Figure 14.1 shows the results for academic studies over time. As expected, studies of interstate war dominate intrastate conflict and terrorism (separately). Although this is expected, it is curious that studies of interstate conflict continued to dominate until at least 2011, when so much emphasis in recent years has been placed on the empirical regularity that interstate wars rarely occur whereas intrastate conflict is rife. 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.

We now turn to a consideration of different methods in the study of intrastate conflict and terrorism. Figure 14.2 illustrates methods used over time, limiting the methods just to the categories, quantitative, qualitative, and formal modeling. Although there is substantial variation prior to the year 2000, with qualitative methods sometimes the most common, quantitative methods are now used much more extensively than other methods to study intrastate conflict. The trends for terrorism are less pronounced,

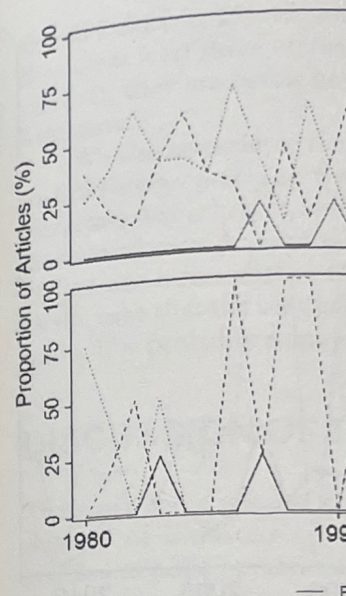


FIGURE 14.2. Plot of the percentage of articles in the TRIP journal database by three methods.

although quantitative studies are increasing. This reflects larger trends in political science.

Finally, we examined whether terrorism studies offer policy prescriptions. This varies widely; however, it does vary over time. The number of articles per year with policy prescriptions is higher in the early 1990s. The spikes in the data reflect the Cold War. Although there may be a decline in policy prescriptions in the data on methods, this suggests that quantitative approaches are becoming more common than qualitative approaches.

THE TRIP SURVEY

Despite excluding much of the TRIP survey data contained in the journal article database, it is a possible category; it is also asked whether 9/11

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in addition, we plotted intrastate conflict and terrorism studies of interstate war. Figure 14.1 shows the percentage of all articles on intrastate conflict. As expected, studies of intrastate terrorism (separately). Although the emphasis on interstate conflict continued to increase, the emphasis in recent years has been on intrastate wars rarely occur whereas attention to intrastate conflict has increased. The graph shows, there nonetheless applies.

Different methods in the study of terrorism. Figure 14.2 illustrates methods used over time. Categories, quantitative, qualitative, quantitative, substantial variation prior to the 1990s. Quantitative is the most common, quantitative is used extensively than other methods. The emphasis on terrorism are less pronounced,

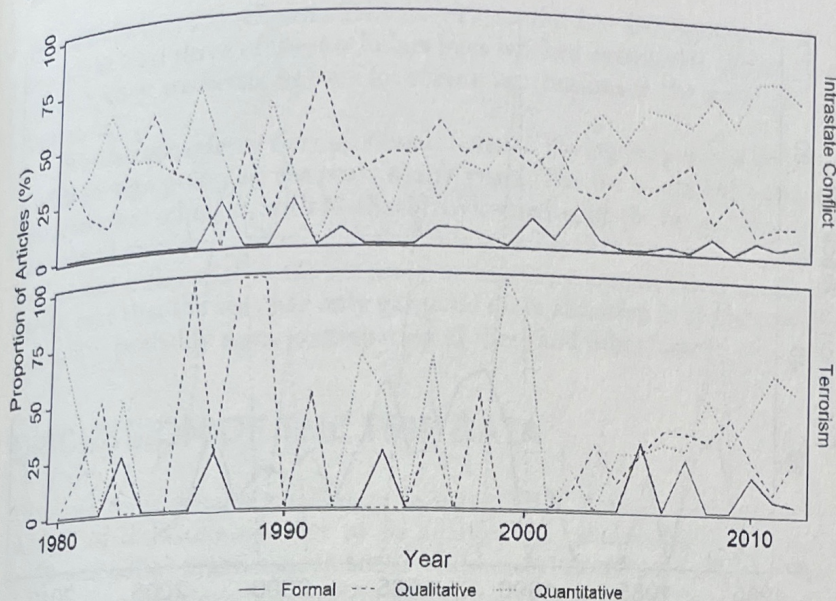


FIGURE 14.2. Plot of the percentage of all intrastate conflict and terrorism articles in the TRIP journal database by three methods

although quantitative studies also appear to dominate especially recently. This reflects larger trends toward the increasing use of quantitative methods in political science.

Finally, we examined what percentage of intrastate conflict and terrorism studies offer policy prescriptions. Figure 14.3 shows that the percentage varies widely; however, it does not provide any weighting for the number of articles per year. Thus, the low percentages in the late 2000s represent far more articles with policy prescriptions than the higher percentages in the early 1990s. The spikes in the early 1990s were likely driven by the end of the Cold War. Although the pattern is somewhat erratic, in raw percentages there may be a decline in policy prescriptions offered. Combined with the data on methods, this suggests that qualitative studies may be more amenable than quantitative approaches to policy prescriptions.

THE TRIP SURVEY DATA

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

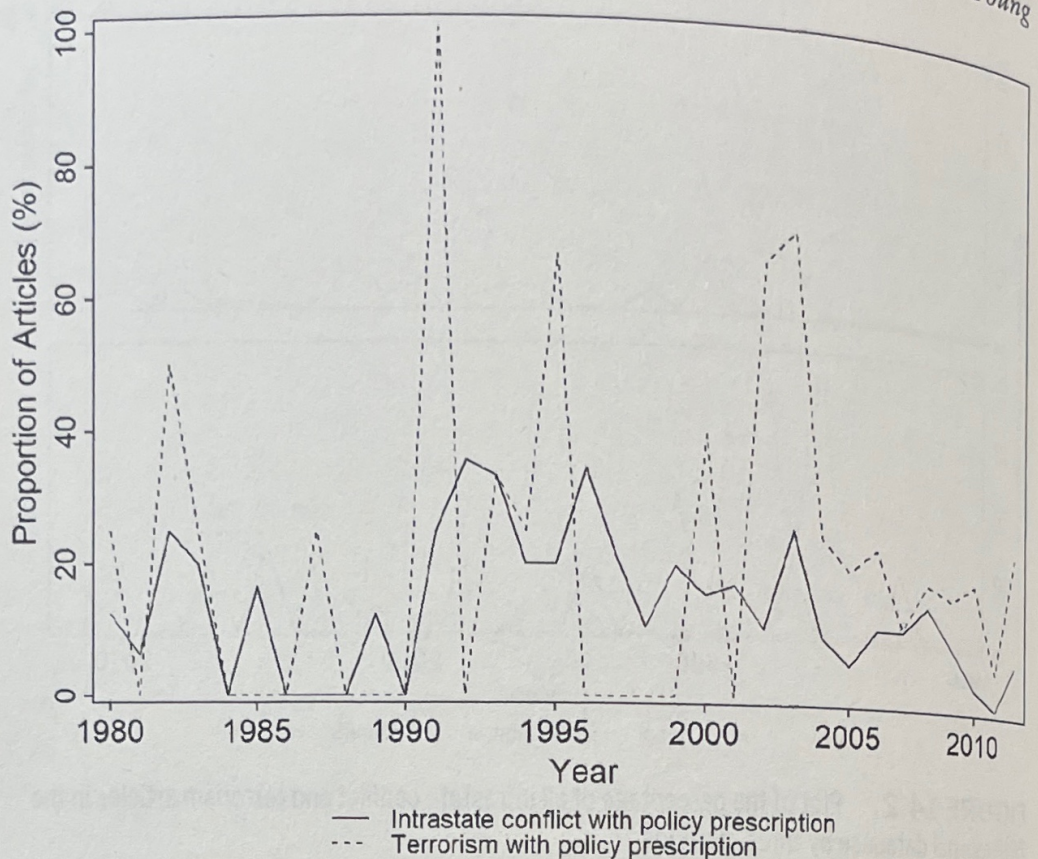


FIGURE 14.3. Plot of the percentage of all terrorism or intrastate war articles in the TRIP journal database that offer a policy prescription

According to the 2006 survey, 9/11 dramatically changed the issue areas covered in class. Less so, it also changed the geographical focus and theoretical approaches. Eighty-five percent of the respondents said that 9/11 significantly or somewhat changed the issue areas that respondents covered in at least one of their courses. Fifty-seven percent report significantly or somewhat changing geographic focus, and 40 percent report significantly or somewhat changing theoretical focus.

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 the most pressing foreign policy issues ranged from the Global Debt Crisis to Global Climate Change, but Arab Spring ranked second overall. Other traditionally intrastate issues, such as ethnic conflict, ranked much lower.

We also considered the descriptive data from the 2008 TRIP survey question asking respondents to “list four scholars whose work has most influenced your own research.” Among the top forty scholars listed, only four had done significant work on intrastate conflict: James Fearon (no. 7),

David Lake (no. 25), Charles Tilly (no. 29), and J. David Singer (no. 37). Although at least three of these scholars 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 twenty years. The list is similarly short on intrastate war scholars, with Michael Doyle moving up the list and the others mentioned previously moving down. It is 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 twenty years. It is probably some combination of these and other factors.

DISCUSSION OF THE TRIP DATA

There are some notable challenges in using TRIP datasets to describe the subfield of intrastate conflict or to analyze the relationship between research and policy practitioners. Some weaknesses may be partially addressed through keyword searching and/or automated text analysis. These empirical approaches certainly merit greater attention moving forward. A key limitation with the current TRIP data is that almost no attention is given to the peace(-making, -keeping, -building) literature. If one were to think about policy decisions the United States or other countries consider on a regular basis, many (most?) of them involve peace operations. Libya, Syria, Ukraine, and Iraq, to name a few, all had peace operations at stake. Indeed, for most developed countries, interstate wars are highly uncommon; yet many of these countries are active in peace operations worldwide.

Perhaps the greatest challenge is to connect the various types of data in ways that allow credible identification of the impact of scholarly work on policymaking or implementation. We venture that at this stage it may be impossible to identify precise causal effects in any completely satisfactory way. Instead, we may be limited to identifying patterns of covariation in academic ideas and policy impact through both TRIP data and case evidence. However, 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 (see Jaschik 2013).

Accordingly, 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 comparison as well as specifically designed interventions and outcomes, which could facilitate the identification of causal effects. 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. Such experiments are promising and one of us is engaged

in a set of experiments that offer a model for moving forward in this regard (Brigham et al. 2013; Findley et al. 2014).

STATE OF ACADEMIC AND POLICY ENGAGEMENT

Over the past fifty years most intrastate violence has occurred in areas of Africa and Asia (Uppsala Conflict Data Program 2018), where the United States and many other countries have had little strategic interest. Thus, even if academics and policymakers were speaking openly and consistently, there has been relatively little policy interest in the regions experiencing intrastate conflict. Brief forays into 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 learning valuable, although painful, lessons about intervention in these areas.

At the same time that policy interest in intrastate conflict has increased, academic interest in the topic also increased. Yet, unfortunately, the two progressed along different paths. Scholars turned toward increasingly sophisticated quantitative 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 be having a healthy dialogue that produces real-world impact. Yet there appear to be at least some influential initiatives in which both policy influence and engagement have occurred, which we now discuss.

From Academy to 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 scholars such as Davies (1962), Moore (1966), Gurr (1970), Tilly (1978), and Skocpol (1979). A related area, although 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, although the latter featured more prominently, if indirectly, in US 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.

moving forward in this regard

POLICY ENGAGEMENT

ence has occurred in areas of (Ham 2018), where the United States has a strategic interest. Thus, even in regions and consistently, there are regions experiencing intrastate conflict demonstrated how few are involved. Even UN peacekeeping operations in the post-Cold War years were limited to intervention in these areas. Intra-state conflict has increased, and unfortunately, the two problems are moving toward increasingly sophisticated methods (for average effects) of analysis. Scholars expressed interest in policy engagement with policymakers or practitioners. There has been a little attempt to understand these approaches.

Academics and policymakers are interested in the real-world impact. Yet there are a number of alternatives in which both policy engagement and we now discuss.

Intra-state conflict was carried out during the Vietnam War. The work in these areas includes scholars such as Davies (1962), and McPol (1979). A related area, counterterrorism, is the study of counterterrorism with practitioner-scholars. An example is an officer prior to publishing at the Rand Corporation. Counterterrorism literature was abundant, although the latter feature was prominent in Vietnam. Gurr's work with the National Commission on the Assassination of Martin Luther King Jr. and

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 was housed within the CIA, has routinely engaged academics for help in providing its estimates of instability. Academics such as Jay Ulfelder, Erica Chenoweth, Monty Marshall, and Jeremy Weinstein have been involved to 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 policymakers in the intelligence community or security industry.

The Integrated Crisis Early Warning System (ICEWS) is another example of scholarly input into policy arenas, specifically the military, and it provides suggestive evidence for the idea that when policymakers need cutting edge research methods (see argument 1b in chapter 1), this may provide opportunities for scholars to develop new knowledge and provide it to policy practitioners (in this case, policy implementers rather than policymakers). Academics have been involved with the program for years. In 2007, the Defense Advanced Research Projects Agency started ICEWS as a tournament to see which models could provide 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 Schrodt formerly at Penn State, Ian Lustick at Penn, and Steve Shellman at William & Mary are all academics who are involved in the project. Again, many of the activities in the ICEWS project are not public, thus making it difficult to assess any impact. We do know that the US government is paying tens of millions of dollars to specific IR scholars and US universities to generate these event data and use them to forecast violent political events.

The Good Judgment Project, which works with the US Intelligence Advanced Research Projects Activity, is another initiative that has turned to crowdsourcing information to forecast world events, including intrastate conflict. The Good Judgment Project claims 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. This project is the result of a multidisciplinary collaboration that combines insights from political science and psychology to generate predictions but also to train forecasters how to make better predictions.

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, and Michael Doyle—began or advanced while these scholars were embedded in policy organizations such as the UN

or World Bank. In the area of intrastate conflict, one of the most visible impacts of academic work was the 2011 World Development Report devoted to the connection between armed conflict and international development (World Bank 2011). A number of noted development and conflict scholars contributed background papers and otherwise helped author the report, which is frequently cited by policymakers within the World Bank and member governments when they explain aid allocation decisions and design interventions.

In a similar vein, one of the fastest growing trends in recent years is policy-relevant experimental work. The Jameel Poverty Action Lab, Innovations for Poverty Action, Experiments in Governance and Politics, and other projects have 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 in which implementing partners explicitly commit to following academic design mandates. Christina Schneider makes a similar observation in chapter 6 and links such collaborations to the need for practitioners to draw on academic researchers when cutting-edge methods are required (argument 1b).

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 UN 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 also participate in this business. In a variety of direct and indirect ways, scholars have contributed to the implementation of development strategies in these contexts.

Academic, policy-relevant work is occurring, but it is unclear whether we have a solid understanding of just how much, in what form, or how consequential it is. And we may never know; as in the following discussion, it may be wiser to try to understand the precise conditions under which practitioners (policymakers, policy staff, and policy advocates) pay attention to academic research, even if it occurs in narrow settings but ones in which we can establish sufficient control to identify such effects.

From Policy to Academy

Observing policy influence on academics (Ribar 2016) is ostensibly simpler than detecting the influence of academics on policy. There is a 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 useful tools to examine,

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and policymakers have slowly to conflict through the lens Peacebuilding Architecture date to work through other development and humanitarian resources to implement development transitions. The World Bank, and others also participate in ways, scholars have contrasted strategies in these contexts. However, but it is unclear whether in what form, or how come the following discussion, it conditions under which practitioners (advocates) pay attention to things but ones in which we effects.

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for example, numbers of terrorism articles pre- and post-9/11. Indeed, surveys of this nature exist.

Some published work and other anecdotal evidence suggest 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.

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 conduct similarly motivated research.

Although we like to think of influence as positive, this is not always the case. Indeed, the improvements in impact evaluation 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 who have left their academic posts to work in governments, intergovernmental organizations (IGOs), or nongovernmental organizations (NGOs). Jeremy Weinstein (Stanford), as mentioned previously, served as deputy and chief of staff for Samantha Power, the US Ambassador to the United Nations, before returning to Stanford University. Michael Doyle left his academic posts to serve as assistant secretary-general and special advisor to Kofi Annan. Michael Horowitz returned to the University of Pennsylvania after 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 who begin work in the academy simply stay in the academy. And those who begin in the policy world typically stay there.

Academics and practitioners work together in other ways, however. Experimental field research, much of it on development and conflict, relies on close academic-policy collaborations. NGOs or IGOs seek to carry out large-scale projects but need assistance from scholars who have expertise in designing proper interventions. Thus, numerous partnerships are born, 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.

university bureaucracy, and the publication system in the academy provide numerous challenges that would need to be accounted for to fully address impact.

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 in this book could understate or overstate the impacts, and it is conceivable that our inferences could be completely wrong. We hope that scholars will think creatively about how to prioritize strategies for stronger causal inference in international relations (Findley et al. 2013; Hyde 2015).

ENDNOTES

1. The TRIP journal data has an *issue area* variable, which captures a higher level of aggregation than the *substantive focus* variable. The most relevant issue area for us is International Security, but this variable does not allow us to distinguish intrastate from interstate or other types of conflict.
2. See Moore (2006) for a discussion on the “dark side” of this definition.
3. See Hoffman (2006) and Young and Findley (2011) for a more complete discussion of defining terrorism.
4. This despite the fact that the dataset covers twenty-one years before 9/11 and only thirteen years after that date. Using a simple chi-square test, this difference is statistically significant at the $p < .05$ level.
5. Findley, Piazza, and Young (2012) and Conrad (2011) also connect the study of interstate war with transnational terrorism.
6. The editors’ useful discussion of types of policy actors is important here and draws attention to the policy staff and advocates who are more likely users of policy prescriptions.
7. 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. We thank Tricia Bacon, American University faculty member and former member of the intelligence community, for this insight.
9. Some rigorous qualitative work, such as Skocpol (1979), outlines necessary and sufficient conditions where a single case could undermine the claim. These arguments are rarer among empirical scholars.
10. Many of these additional articles are in the quantitative tradition (Young 2019).

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DANIEL MALINIAK, SUSAN PETERSON, RYAN POWERS,
and MICHAEL J. TIERNEY, *Editors*

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