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The Consequences of Contention: Understanding the Aftereffects of Political Conflict and Violence

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Abstract

What are the political and economic consequences of contention (i.e., genocide, civil war, state repression/human rights violation, terrorism, and protest)? Despite a significant amount of interest as well as quantitative research, the literature on this subject remains underdeveloped and imbalanced across topic areas. To date, investigations have been focused on particular forms of contention and specific consequences. While this research has led to some important insights, substantial limitations—as well as opportunities for future development—remain. In particular, there is a need for simultaneously investigating a wider range of consequences (beyond democracy and economic development), a wider range of contentious activity (beyond civil war, protest, and terrorism), a wider range of units of analysis (beyond the nation year), and a wider range of empirical approaches in order to handle particular difficulties confronting this type of inquiry (beyond ordinary least-squares regression). Only then will we have a better

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and more comprehensive understanding of what contention does and does not do politically and economically. This review takes stock of existing research and lays out an approach for looking at the problem using a more comprehensive perspective.

INTRODUCTION

How does collective and violent behavior within nation-states impact politics and economics? Drawing on some of the most prominent theorists in the social sciences, three very different arguments have been put forward to answer this question, influencing subsequent research within political science as well as related disciplines.

One argument, largely associated with Thomas Hobbes, maintains that particular forms of large-scale contention (i.e., civil war) have devastatingly negative effects on the lives of those who exist within its wake. As he states, in a condition of war, “there is no place for Industry; because the fruit thereof is uncertain; and consequently no Culture of the Earth; no Navigation, nor use of the commodities that may be imported by Sea; no commodious Building; no Instruments of moving, and removing such things as require much force; no Knowledge of the face of the Earth; no account of Time; no Arts; no Letters; no Society; and which is worst of all, continuall feare, and danger of violent death; And the life of man, solitary, poore, nasty, brutish, and short” [Hobbes 1968 (1651), ch. XIII]. Most recently, this conception has found its way into discussions of not only immediate negative aftereffects (Gates et al. 2012) but also persisting “conflict traps” that continue over time (Collier et al. 2003, Hegre et al. 2017).

Another argument, largely associated with Karl Marx and Frederick Engels [Marx & Engels 2010 (1848), Marx 1867], maintains that particular forms of large-scale contention (i.e., class struggle and revolution) have positive effects for some and negative effects for others. Here, we find that although contention leads to significant losses for the ruling class in both political and economic terms (such as the loss of property, position, security, and life itself), it is also the case that contention benefits the working class across the same domains, bringing economic equality and political empowerment. Indeed, in this and related work, it is the duality of fortunes that best characterizes the influence of contention. The resonance of this argument is extensive—albeit with somewhat greater focus being given to either “winners” or “losers” viewed in isolation. For example, Piven & Cloward (1979) put forward a version of this argument in their evaluation of poor people’s movements, and an extensive literature on social movement effectiveness follows this work (Soule & Olzak 2004, Chenoweth & Stephan 2011). Such a conception has also found its way into discussions of how nations after civil war “rise like a phoenix from the ashes” (Organski & Kugler 1980) and, returning to the explicit interest of Marx and Engels, the claim that revolution is the only way for specific political-economic problems like inequality to be resolved (Scheidel 2017).

Still another argument, largely associated with Georg Simmel (e.g., Simmel 1964a,b) maintains that the consequences of contention (less tied to specific forms than in the first two arguments) vary depending upon diverse characteristics, such as the degree of regulation, direct confrontation, and violence contained within the relevant behavior. Within this work, knowledge of what is going on out in the streets and in the mountains is essential for understanding what contention influences as well as how it exerts that influence. Unlike in the other areas of research, scholars in this tradition have not explicitly referenced Simmel, but research draws extensively on his work (Huang 2016).

Which of the arguments above have been empirically supported by quantitative research over the last four decades? Despite a growing amount of work on the consequences of contention, we maintain that existing research has only just begun to provide insights. Most research supports Hobbes' view of contention, but there is also work supporting Marx and Engels as well as Simmel. Part of the difficulty in answering the question is due to significant knowledge gaps in the literature. Consequently, numerous opportunities for advancement are readily identifiable—with a few notable works already leading the way. This review discusses four limitations/areas of opportunity.

First, most research has been focused on specific forms of contention undertaken by challengers (i.e., civil war, terrorism, or protest)—viewed one at a time. While useful, this work ignores the full diversity of forms that contentious behavior could take. For example, this approach generally neglects the activities of political authorities—a topic that has admittedly only come into special prominence over the last 10–20 years (Poe & Tate 1994, Davenport 1995, Conrad & Moore 2010, Nordås & Davenport 2013, Hill & Jones 2014, Sullivan 2014, Ritter & Conrad 2016, Rozenas et al. 2017). An encompassing approach would allow a more comparative assessment, gauging not only how distinct forms of contention impact diverse consequences (evaluated on their own), but also how they impact diverse outcomes when other forms of contention are present. A more encompassing approach would include large-scale and violent activities such as civil war, genocide, revolution, and human rights violation/state repression; large-scale and nonviolent activities such as civil resistance and political surveillance; medium-/small-scale and violent activities such as riots, aggressive protest, protest policing, and political strikes; and smaller-scale and nonviolent activities such as everyday resistance and banditry. Some newer research explores several forms of contention at once (e.g., Buvinic et al. 2013). Unfortunately, this work considers only a limited number of activities and does not consider consequences.

Second, most research has focused on specific types of outcomes (e.g., the level of democracy for those interested in political factors, or economic growth for those interested in economic factors). Although crucial for addressing some prominent questions in political science, as well as particular policy concerns in government and civil society, this focus does not adequately capture the variety of ways that contention influences politics as well as economics and thus it does not allow those interested in the topic to generally understand the relationships of interest. Indeed, a more accurate and useful accounting of consequences requires a broader evaluation of how contention influences distinct political-economic characteristics. On the politics side, this would include not only the level of democracy but also topics such as public opinion, political trust, and the development of law. On the economics side, this would include not only studies of economic development and growth but also other areas such as education, employment, and inequality. Again, some research is moving in this direction (Gates et al. 2012), but this work is only recently emerging and it is only focused on specific forms of contention.

Third, most research has paid little attention to the various actors that could be differentially influenced by contention, and it has been aggregated to the level of the nation-state. Although this has been steadily changing over the last 10–15 years [along with the micro-foundational/disaggregated/subnational turn in conflict studies (Blattman & Miguel 2010)], this orientation has not allowed those interested in the topic to understand precisely who benefits as well as who loses when contention takes place. Relatedly, most work has been aggregated to the temporal unit of the year, and inconsistent attention has been given to alternative time horizons. This has precluded evaluations of longer-term consequences of contention similar to the pathbreaking and highly provocative work of Acemoglu et al. (2001) and Nunn & Wantchekon (2011). For a discussion of how our focus differs, see the sidebar titled *Evaluating Behavior Not Institutions*.

EVALUATING BEHAVIOR NOT INSTITUTIONS

We differentiate political conflict and violence from institutions that are, in part, defined by their use of political conflict and violence. For example, the controversial and resonant Acemoglu et al. (2001) have an interest in colonialization, colonialization strategy/policy, and extractive institutions, but these are conceived of as relatively static relationships between political-economic actors and ordinary citizens reflected in law as well as economic relations. What is ignored is the pathway to such relationships and/or the behavior that is used to sustain them. Thus, the extraction of King Leopold from Congo is mentioned but not the genocide, atrocities, imprisonment, forced labor, torture, kidnapping, and mass rape that accompanied it. A similar argument can be made for research like that of Nunn & Wantchekon (2011) and Acharya et al. (2016) with regard to the impact of slavery on public opinion.

Fourth, researchers have tended to rely on a particular set of empirical tools that, while generally suitable for the questions they seek to answer, do not provide the ability to address several problems that plague the type of research being undertaken (i.e., lagged effects and endogeneity). Newer and more sophisticated work has begun to employ better and more various tools for investigation (e.g., Hegre et al. 2017, Rozenas et al. 2017) but, again, these applications are relatively recent and have not yet diffused across the research community being discussed here.

With the goal of improving our understanding of the consequences of contention, the current review begins by summarizing the disparate literature that exists, mainly in political science but with some consideration of economics, public policy, sociology, and psychology. We argue that in order to understand the consequences of contention, we must evaluate all of the different forms that contention could take and all of the different consequences that could be experienced. In addition to this, the scope of such an evaluation should stretch across a decent period of time and cover a significant number of cases as well as actors within the same zones of conflict—compared against reasonable samples, groups, or nations. Unless the literature adopts this approach, we are likely to know very little about what civil conflict and violence actually do politically and economically. In the conclusion, we suggest numerous paths this exciting research should follow in the future.

THE STUDY OF INTRASTATE POLITICAL CONFLICT AND VIOLENCE

Research on collective and violent behavior first emerged during the late 1930s, underwent a major wave of growth in the 1960s and 1970s, and waxed and waned into the late 1990s, after which it has grown steadily. This work is now in the mainstream of political science. It is routinely supported by diverse funding institutions, published in the top venues of the discipline, and undertaken by thousands of scholars worldwide.

At the beginning of this line of inquiry, researchers started with specific topics, such as revolution, before expanding their interests to subjects like political instability, riots and protest, insurgency/counterinsurgency (civil war), repression/human rights violation, protest policing, genocide and atrocities, and terrorism/counterterrorism—these being named roughly in chronological order of appearance in the literature. These investigations largely followed the conceptualization of the conflict cycle, asking why contention starts, what type of tactics are employed during periods of overt contestation, at what frequency/intensity and scope contention occurs, what targets are selected and how (e.g., indiscriminately, selectively, or collectively), how long conflagrations last, how they end (e.g., victory, settlement, or stalemate), whether they restart, and what impact (i.e., consequences) they have on diverse phenomena. These topic areas have been examined across all types of contention (**Figure 1**).

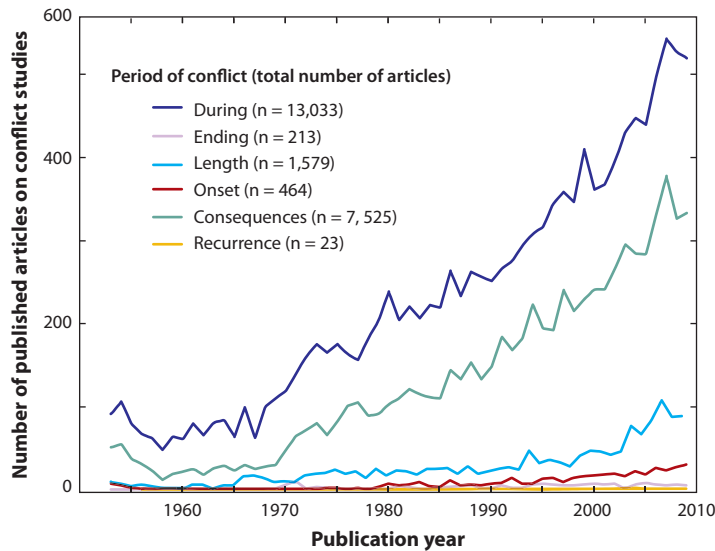


Figure 1

Number of published articles on conflict studies, 1953–2009. The data come from political science articles from all English-language journals included in JSTOR’s archives from 1953 through 2009. This corpus includes 204,684 articles, but the figure represents the subset of articles reasonably related to conflict studies broadly defined, totaling 28,078 articles. Making some reasonable assumptions about how often words related to any given aspect of conflict studies should appear in articles for them to be classified as focusing on that aspect, we further divided the corpus into articles that focus on the period during conflict, the ending, the length, the onset, the outcomes, and the recurrence of conflict.

We might expect that researchers would start with investigations of onset, and that once some clarity around this phenomenon emerged, they would move on to tactics, frequency/intensity, severity, and so on. But this has not been the case. The study of contention has emerged in a largely uncoordinated fashion across topic areas, and very little has been done to assess how far the field has come. Reviews do exist, but they are generally focused on one type of contention, such as repression (Davenport 2007), civil war (Blattman & Miguel 2010), or riots (Wilkinson 2004). As a result, specific aspects of contention such as their consequences have been hard to address, because to do so would require studying different types of contention in tandem. One can find a review of select forms of contention and one consequence, but here we advocate something broader.

TYPES OF CONTENTION AND TYPES OF CONSEQUENCES

Acknowledging the diversity in the types of intrastate conflict as well as the necessity for considering multiple forms at the same time, the concept of contention we adopt in this article is quite encompassing. We agree with McAdam et al. (2001, p. 6) that “the study of political contention has grown too narrow, spawning a host of distinct topical literatures—revolutions, social movements, industrial conflict, war, interest group politics, nationalism, democratization—dealing with similar phenomen[a] by means of different vocabularies, techniques and models.”

How should we think about this broader concept? To map out different types/forms of contentious behavior, it is useful to differentiate by who is the initiator and who is the target. In

cases where the government is both initiator and target, we have coups. Where the initiator is the government and the target is some political challenger, we identify a larger number of activities, including protest policing, counterinsurgency, counterrevolution, counterterrorism, human rights violations/state repression, political discrimination, ethnic conflict, legal execution, extrajudicial execution, lynching, pogroms, forced relocation, and political exclusion. Civil war would be placed here, but it would be joint with another category because both governments and challengers have to be present for the behavior to be identified. Governments could also target those not explicitly challenging them politically (i.e., civilians and social groups). This, too, is referred to by many names: civilian targeting, atrocity, genocide, political discrimination, domestic surveillance, human rights violation, state repression, one-sided violence, asymmetrical violence, ethnic conflict, legal execution, extrajudicial execution, famine, forced relocation, and political exclusion.

When political challengers are initiators and they target governments, forms of contention include protest, terrorism, revolution, insurgency, and civil war. When political challengers target each other, this is classified as nonstate conflict or countermovement behavior. Civilian targeting and terrorism result when political challengers target civilians.

Finally, we include what is referred to as everyday resistance and protest. Both concern instances where civilians and social groups are initiators and political authorities are targeted. Everyday resistance includes slowdowns, sabotage, rumors, and the like, whereas protest includes more traditional forms of contention such as marches, sit-ins, and petitions. Civilians may defect from the side of the challengers and provide information to political authorities about what challengers are doing, where they are doing it, and when; or they may defect from the side of authorities and provide information to challengers, serving as informants. We do not consider situations where civilians or social groups challenge one another. Such dynamics would be classified as criminal and/or private and fall outside of our scope.

While we include a broad conception of contention, we focus on only two types of consequences in this review: political and economic. Regarding the political consequences, we consider objective evaluations of structure (e.g., the level of democracy) as well as subjective evaluations of government performance (e.g., perceptions of efficacy and trust in government). We also consider different levels of analysis: nations, regions within nations, local municipalities, groups, and individuals. In the area of economic consequences, we consider diverse activities such as trade, employment, duration of time working, spending, and saving. We consider the structure of relations including inequality (i.e., vertical as well as horizontal) and the type of economy (e.g., land-specific and primary-commodity). We also consider characteristics best thought of as processes or outcomes, such as the level of development, infant mortality, and access to potable water.

For both consequences, we address temporality. For example, researchers have focused on contemporaneous, short-term (1–4 years), medium-term (5–10 years), and longer-term effects (25 years and more). This variation is useful to consider as we attempt to understand how short-lived or enduring different consequences might be.

Absent from our evaluation are (among various topics) physical and mental health-related consequences. Work on these outcomes is extensive, not generally addressed by political science, and beyond the scope of the current review.

A GENERAL OVERVIEW OF THE LITERATURE

What do we know about political and economic consequences of contention? Hobbes's arguments about the negative aftereffects of contention seem to find the greatest support, but the work of Marx and Engels as well as Simmel also receives some support. The latter two are more often involved in the literature on political consequences because it is generally more interested in diverse

forms of contention than is the literature on economics. Clearly, however, prior research has some limitations that hinder our ability to assess who provided the more accurate characterization of what happens. Looking at the research, several patterns are clearly visible. These are reflective of an important and fertile line of inquiry, which has only begun to scratch the surface. The discussion here simply provides a general summary. Across the next two sections, we delve more deeply into specific literatures discussing political and economic consequences—noting some of the topics, data, methodology, findings, and limitations.

One limitation is that most scholarship on the topic has focused on three forms of contention: civil war (the clear majority), terrorism, and protest. The attention to civil war is understandable. Armed challenges to the state fundamentally center on claims to resources and voice, which through coercive and forceful means transform both the patterns of mobilization and the distribution of power that constitutes the polity. In contrast, there has been very little research dedicated to other forms of contention, such as riots, coups, or revolutions. Given the discussion of these forms of contention by early theorists such as Marx and Engels as well as Simmel, it is somewhat puzzling that they have received so little attention.

Especially noteworthy, no research exists on the consequences of state repression and human rights violation. This is not to say that nothing at all has been written on the subject. For example, some research has investigated how human rights violations influence allocations of foreign aid. Although initial examination found support for a direct and negative impact (repression decreased aid), more sophisticated theorization and modeling has revealed that this relationship is more complex. For instance, Lebovic & Voeten (2009) find that repression has no direct effect on aid; rather, repressive behavior increases criticism, which in turn decreases aid. While directly related to the consequences of contention, however, this work is limited to only a few scholars, and important topics remain under-researched. For instance, all of the investigations could benefit from exploring alternative operationalizations and specification. Conceptually, both foreign direct investment (FDI) and aid need to be unpacked, as they combine very different actors and actions. Repression also needs to be disaggregated, as some government tactics might be more likely to influence economic factors than others.

With regard to the specific consequences that researchers have investigated, we further see an imbalanced focus across consequences as well as methodologies employed. For political consequences, most research has focused on particular institutional characteristics such as democracy and has used a standard and conventional set of estimation techniques, but other studies (e.g., on political trust and a few on gendered representation) have investigated a more varied set of characteristics and used a broader set of estimation techniques. In contrast, we find that economic consequences have been studied in a more diverse, comprehensive, and sophisticated manner than political ones, but the economic consequences investigated are considerably narrower in terms of topics covered, with most attention being given to income.

METHODOLOGICAL ISSUES AND CHALLENGES

In addition to discussing what has been examined, it is important to consider how the topic has been and should be studied. From this point of view, it is clear that fundamental methodological issues arise within the current literature because the strategy has generally been to examine a type of contention and a specific consequence within a particular territorial unit, across some unit of time. With such a configuration, research design requires a great deal of care. Broadly speaking, three methodological issues emerge: whether and how to include unit effects, whether and how to include temporal effects (e.g., dynamics, detrending), and whether and how to

incorporate geographic/spatial information. Of course, not all of these issues apply equally, or at all, to all parts of the literature considered.

First, in the classical ordinary least-squares (OLS) framework, the decision to include unit effects (e.g., country or dyad effects) has come down to a question of whether the author wants to assess the “within” relationship (i.e., the average within-unit effect of the variables) or the “between” relationship—the effect that unit averages of x have on unit averages of y . Unit fixed effects produce a within estimate. OLS without unit effects produce some average of within- and between-unit effects. Random effects models with contextual covariates can estimate between-unit effects and also within-unit effects if properly specified, according to Bell & Jones (2015). The use of fixed and random effects gets more complicated in nonlinear models as the problem of incidental parameters has been shown to cause bias and inconsistency in commonly applied generalized linear models, like logits and probits (Wooldridge 2002).

At present, there is an interesting disconnect between the studies that use linear models, most of which use fixed effects to model within-unit relationships, and the studies that use nonlinear models, which often do not do so. Many of the country-year or dyad-year studies within the consequences literature use a fixed-effects approach, generally involving linear models on a continuous dependent variable (e.g., FDI, GDP, growth). Far fewer of the studies in the field use random-effects methods to evaluate hypotheses, and those that do often estimate the fixed-effects model, too. Studies that employ nonlinear models are less likely to use fixed effects.

Second, temporal effects are crucial to address. For example, if a shock such as the outbreak of terrorism, repression, or armed conflict unwinds over time, then dynamic processes are required to model it. Generally, if the dynamic processes themselves are of interest, a lagged dependent variable is used in the linear regression case. If the dynamic process is not of interest in its own right, an $AR(p)$ model might be used to account for the dynamic processes in the errors. Researchers tend to use a lagged dependent variable to account for temporal dynamics. While there are many examples, very few take the advice of Williams & Whitten (2012) and identify the dynamic effect of variables. In most research on consequences of contention, the dynamic process is seen as a nuisance. This needs to change, however, as there is important information contained here.

Third, spatial dependence remains a more general issue. Although there is a growing trend to consider the geographic or spatial effects of, for instance, conflict onset, duration, and termination, this work is in its nascency. For example, Bara (2017) uses a dependent variable measuring whether or not conflict is ongoing in a country’s neighborhood to study the diffusion of conflict. Neumayer & Plumper (2016) use spillover effects of nearby terrorism on tourism. Interestingly, neither of these employs a conventional spatial lag model where the neighborhood effect of conflict is included as a predictor, or something similar.

Fourth, the issue of endogeneity remains woefully neglected and is especially problematic in the study of contention because both conflict and its aftereffects can be mutually reinforcing. Usually endogeneity is addressed through a two-stage least-squares approach, where the effect of one variable is instrumented by another that is correlated with the independent variable of interest, but not with the errors and not with the dependent variable conditional on the independent variable of interest. These restrictions are often quite difficult to satisfy in specific cases. For instance, Dincecco & Prado (2012) use casualty counts in premodern wars to instrument for current fiscal institutions to estimate their impact on GDP. In general, instrumental-variable approaches are used with continuous dependent variables in an OLS regression framework. There are options for estimating these models in other contexts, but we do not see them used in this literature. In addition, some recent work has drawn on other sources of variation to estimate causal effect. To study the political consequences of terrorism, Balcells & Torrats-Espinosa

(2018) leverage a natural experiment created when Euskadi Ta Askatasuna (ETA) perpetrated terrorist attacks in Spain at the same time as people's political attitudes were being surveyed. Besley & Mueller (2012) use variation in patterns of violence across Northern Ireland to study the economic consequences of armed conflict. Approaches such as these seem particularly useful.

Political Consequences

Usually relying on standard OLS and logit models (the former often with fixed effects), the overall findings of this work suggest that contention generally hurts society—viewed in the aggregate. Some isolated studies find that conflict and violence enhance specific state attributes or that specific individuals or groups within the nation where contention takes place could end up with some positive outcomes (Blattman 2009). This said, there is no work making the case that the changes brought about by conflict are worth the experience—at least not explicitly.

As noted above, most of this work examines civil war. In addition, there is a predominant focus on how this form of contention affects particular topics: democracy and democratic rule. This is perhaps not surprising. A main feature of democracy is the interest in establishing and maintaining order through nonviolent means. For example, several scholars describe the establishment of democracy as an institutionalized concession by dictators seeking to co-opt revolutionary threats to their rule from mobilized masses (Acemoglu 2006). Democracy thus represents an important political response to arguably one of the most significant threats to its existence. There are also practical reasons for this focus. Roughly half of the countries that have emerged from armed conflict since 1946 saw a transition to electoral democracy during the first decade after the end of hostilities.

In an effort to better understand the conflict–democracy linkage, the literature has gone beyond simply looking at whether a country is democratic, examining more fine-grained issues. For example, research discovered that rebel groups who use diplomacy during contentious episodes—as well as institution- (and organization-) building—offer more inclusive and socially embedded rebel governance, which ultimately aids in the establishment of a viable postconflict democracy (Huang 2016). This is not an easy path by any means. Even though some warring actors have enjoyed popular legitimacy among parts of the population, it is noted that there are still challenges when incorporating armed combatants into political systems where voting in free and fair elections supposedly substitutes for violence (Dunning 2011, Matanock 2017). For instance, the decimation of the state's infrastructural and administrative power during contentious episodes leads to weak capacity to conduct free and fair elections in the immediate postconflict period (Flores & Nooruddin 2012). The lingering impact of hostilities on the social fabric of intergroup relations has also been widely recognized in the literature on institutional engineering, e.g., on power-sharing (Hartzell 2015). The interactive and conditional considerations emerging out of this work point to an important direction that could be explored by further research.

While offering significant insights into political consequences of civil war, the cross-national literature has been limited in important ways. First, the consequences being evaluated are often assumed, not measured (unless indirectly through studying the impact of variation in the level and scope of violence itself, as suggested earlier by Simmel). Second, when only countries exposed to conflict are included in relevant samples, the consequences of conflict are hidden in the absence of counterfactuals. This problem is aggravated by the fact that democracy and peace grow out of and are sustained by many of the same factors. Hence, the political predicaments of postconflict societies may be significant determinants of selection into conflict, rather than merely consequences of the same.

One way to address this issue has been to drill down further into causal processes at the micro level, as can be seen in the more recent literature. Based on individual-level data from surveys, field experiments, and social games, this research analyzes how fear of or exposure to particular forms of large-scale violence—but also lower-level threats and coercion—shapes outcomes such as voting, political participation, political and social trust, willingness to protest, parochial altruism, and attitudes to compromise. To date, the results are notably mixed, reflecting the duality in outcomes noted by Marx and Engels.

On the one hand, a number of studies report evidence of the detrimental legacies that contention has for people's support regarding participation in social and political processes. This work reveals many things relevant to the topic: Exposure to violence undermines support for democracy (Burchard 2015), causes traumatized individuals to withdraw and hide (Hutchison & Johnson 2011), reduces support for political compromise (Canetti et al. 2017), leads to polarized as well as exclusionary attitudes at the level of the individual (Balcells 2012), and reduces voter turnout (Zhukov & Talibova 2018).

On the other hand, several studies find that exposure to contention is associated with increased political participation, e.g., propensity to vote, civic engagement, willingness to protest, interest in politics, and community leadership (Bellows & Miguel 2006, 2009; Blattman 2009; Gilligan et al. 2014; Grosjean 2014). Some also find that exposure to violence affects prosocial attitudes, strengthening altruism, heightening an aversion to inequality, and solidifying adherence to social norms and increased social cohesion (Voors et al. 2012, Bauer et al. 2014, Gilligan et al. 2014), as well as increasing the likelihood that people will support peaceful compromise and enhancing their willingness to make political concessions (Tellez 2018, Gould & Klor 2010).

A central mechanism invoked in the latter studies is post-traumatic growth, by which affected individuals bind together and forge collective coping mechanisms to defend themselves from the consequences of violence. This work highlights the distinction drawn between in- and out-group members, where prosocial attitudes and norms are largely parochial in nature and extend primarily to one's in-group (Bauer et al. 2016). The work here is not isolated. These results resonate well with new evolutionary psychological perspectives holding that intergroup competition favored adaptive psychological responses that promote the success of the in-group relative to the out-group (Boyd et al. 2003, Choi & Bowles 2007) while reaffirming older arguments made by Simmel (1964a). These exciting intersections and revisitations call out for further exploration.

Economic Consequences

The economic cost of war has attracted the attention of scholars since at least the end of World War I. Originally this literature developed to study the consequences of interstate conflict (Pigou 1916, Keynes 1919, Taylor 1974, Tilly 1985), and the intrastate counterpart has only rigorously been pursued for about 30 years (see the sidebar titled *The Organized Study of Interstate Versus Internal Conflict*). Like the literature on political consequences, the literature on economic outcomes has focused predominantly on one form of contention: civil wars. However, in contrast to the variety of consequences noted above, this work tends to focus on one type of consequence: economic development. Also, in contrast to the political consequences literature, there are a large number of articles on this topic and many scholars who are engaged in relevant research, using a broader range of empirical tools. As a result, the economic literature tends to be more sophisticated methodologically, utilizing not only OLS and fixed-effects least squares, but also logit, event history, 2SLS, weighted least squares, multivariate analysis of variance, kernel-weighted local polynomial regression, quasi-Poisson, and other models.

THE ORGANIZED STUDY OF INTERSTATE VERSUS INTERNAL CONFLICT

Research on internal conflict has evolved very differently from research on interstate conflict. The difference is largely explained by the small number of scholars from a few programs who set out to study interstate war, as well as the organizational leadership provided by specific research communities—most notably the Correlates of War project, which left an important imprint even after the study of interstate war diffused to a larger number of individuals and institutions. No such single organization, however, has had an enduring impact on the study of intrastate conflict behavior, broadly conceived, and as a result the intrastate conflict community contains a greater variety of theory, data, and methods. This said, it is impossible to consider the systematic study of intrastate behavior without acknowledging the importance of scholarship on interstate conflict for such research.

The study of intrastate conflict eclipsed the study of interstate behavior only after the end of the Cold War. This transition from inter- to intrastate conflict makes sense given that, following this period of global conflict, intrastate conflicts significantly outnumbered interstate ones both in frequency and in number of casualties generated.

This concentrated focus has led to some important insights. For example, there is extensive evidence of the negative and substantial impact of contention on average. Collier (1999) and Gates et al. (2012) estimate that civil wars reduce GDP growth by more than 2% for each year of war duration. This 2% figure has been replicated in numerous studies and is well established. Such a finding stands in strong contrast to the literature on political consequences, where there has been very little replication, cross-validation, and agreement on the impact of contention. Research not only identifies a contemporaneous effect but also reveals that civil wars can lock countries into a “conflict trap” over time, whereby contention causes a deterioration in development, which, in turn, increases the risk of renewed conflict (Collier et al. 2003). Moving beyond the primary concerns of the political consequences work, the economic literature finds that impacts of civil war on growth are not limited to the country in conflict; Murdoch & Sandler (2002, 2004) demonstrate that civil war has adverse growth effects in neighboring countries, making traps regional as well.

Why do we see the impacts that we do? In contrast to the literature on political consequences, which has moved toward evaluating more disaggregated processes at a subnational level, the economic consequences literature moved toward a macrotheoretical approach. For example, Collier (1999), drawing very much on Hobbes, theorized alternative routes through which conflict reduces development into destruction, disruption, diversion, and dissaving. Here, actions by warring parties destroy production and health facilities, reduce the workforce, hinder economic exchange, and increase transportation costs. Disruption occurs through the insecurity created by violence and a general breakdown of the social order, as well as the effect of large populations fleeing their homes and thus their jobs. Civil wars lead to massive diversion of public funds through increased military spending. Finally, war economies suffer from dissaving and massive capital flight.

Armed conflict is further shown to adversely affect the structure of the economy. Since land-specific capital such as agriculture and other primary commodities are less mobile, the flight of capital means that conflict makes economies more primary-commodity dependent, which further increases the risk of future conflict (Collier et al. 2003, p. 84). The erosion of incentives to invest in the conflict-country exists at all levels of the economy. Skilled labor migrates, middle-class citizens with savings move them abroad, foreign companies close down all activities if the consequences of protecting investments become too high, and governments become short-sighted as well as opportunistic. The income losses due to war are typically of the kind that increase the future risk of new conflicts. Supplies of financial and human capital contract relative to land, natural resource extraction, and unskilled labor. The breakdown of government control opens up opportunities for production of illegal drugs.

The research again has demonstrated that contention is substantively impactful. Indeed, evidence suggests that in terms of their influence on growth, the economic consequences of civil war are greater than those of interstate war (Collier 1999). Cerra & Saxena (2008, p. 442) examine a variety of crises (e.g., civil war, currency and political crises, banking crises, and changes to executive power) and find that for civil wars, on average, “output declines by 6% initially,” making them the most devastating type of crisis they studied. Replicating this study, Mueller (2012) finds that the decline is actually closer to 18%. Cerra & Saxena (2008), moreover, find that half the loss is recouped after four years, but three percentage points of cumulative loss remain even after a decade.

In line with Simmel (1964a), the effect of conflict on economic growth appears to be contingent on the conflict’s intensity and duration—scope conditions not investigated sufficiently in the political consequences literature. For example, while short wars “cause continued postwar [GDP] decline...sufficiently long wars give rise to a phase of rapid growth” (Collier 1999, pp. 175–76). This so-called phoenix effect is much celebrated in interstate research (Organski & Kugler 1980), but it does not appear to be a particularly robust finding with regard to intrastate conflict. In one of the only investigations of the topic, Hegre et al. (2017) find little evidence for such an effect following civil conflict and conclude instead that the conflict trap is more severe than earlier studies have indicated.

Following the dominant trends in the literature, we have focused our discussion on the research that considers the impact of contention on GDP or GDP growth. Other effects have been examined, however. For example, contention is found to impact other aspects of economic development (Gates et al. 2012), in particular poverty and—arguably a better proxy for the overall level of economic development in a country—infant mortality rates (Iqbal 2010). Beyond poverty and infant mortality findings, the work of Gates et al. (2012) is especially useful because it examines the consequences of contention for an array of different aspects of economic development associated with the United Nations’ Millennium Development Goals. Perhaps not surprisingly, this work finds that contention has detrimental effects on most of them, revealing that this form of political contention significantly impacts human life. For instance, they estimate that a medium-sized conflict with 2,500 battle deaths increases undernourishment by 3.3%, reduces life expectancy by about one year, increases infant mortality by 10%, and deprives 1.8% of the population of access to potable water. In this, they echo earlier findings by Ghobarah et al. (2003).

Despite these significant advancements, however, there is clearly more work to be done. As some believe that in situations of conflict, “The ability to enforce contracts is reduced as the institutions of civil society are weakened, trust declines, time horizons shorten due to uncertainty, and opportunism becomes more profitable” (Collier 1999, p. 178), there has yet to be a micro-foundational wave comparable to what is taking place for political consequences to examine this rigorously.

A NEW RESEARCH AGENDA AND NEXT STEPS

As we hope our review has revealed, the literature on the consequences of contention is large and multifaceted in terms of the topics investigated, methods employed, and findings identified. This work has covered substantial ground, but like most areas concerning collective and violent behavior, it has done so in a largely uncoordinated and non-integrated manner with little awareness of the research area to which it belongs, evidenced through citations across different types of contention and consequences. A key task going forward will be to bring together the different strands of the literature, facilitating a comprehensive evaluation and a richer understanding of the topic. To improve, we need to go beyond the siloed state of the present literature. To approach this issue, we suggest two directions that researchers could take.

First, we have identified the need to reconfigure how the subject is examined. For example, investigators need to consider different types of consequences resulting from different types of contention, particularly those that have often been overlooked or underdeveloped. We need to disaggregate the consequences of contention across ethnic groups, classes, ideological orientations, and genders. Such variation is rarely examined within existing work. Finally, we need to consider consequences in the short, medium, and long term. This is important because numerous studies have identified the enduring impact of civil war on vote choice (Costalli & Ruggeri 2014) and the rejection of out-group identities (Balcells 2012). But moving beyond this form of contention, it has also been found that repression influences political turnout (Zhukov & Talibova 2018) as well as political and social attitudes (Lupu & Peisakhin 2017, Rozenas et al. 2017). Such efforts are important because, while studies that restrict themselves to a shorter time frame may be well suited to estimate the destructive effects of conflicts on infrastructure, they may miss important parts of the puzzle when it comes to the more long-term impacts of contention on human perception and capital accumulation or, critically, innovation.

Second, we suggest that the literature needs to take sequencing more seriously. Although it is not frequently discussed in existing statistical research, it is clear from viewing historical research that different types of contention often follow each other temporally. Given this, it is possible that different actors within the same territorial unit may go through decades of diverse forms of contention, so that by the end of the relevant period virtually all of the different types noted in this article have been experienced. There is some evidence for this already. For example, Hegre & Nygård (2015) find not only that repression increases during civil war but also that rates of repressive behavior remain high long after the conflict has ended. This leads to an important realization: If you only study civil war, you might come to one conclusion about consequences; but, if instead you study repression, you might come to another conclusion; and, if you were studying both forms of contention, you might come to yet another conclusion. To really understand the political and economic consequences of contention, we would need to include many, if not all, forms of contention.

There are numerous ways in which the research community could pursue a more comprehensive and dynamic accounting of contention. For example, the authors of this review are presently working on a latent measure of the relevant concept, which will account for both challenger and government behavior, including genocide, civil war, state repression/human rights violation, terrorism, and protest and civil resistance across as well as within nation-states. This measure represents one way to study the full cycle of contentious activity without truncating it and focusing only on specific subsets of relevant behavior.

While noting where research could go, we also highlight the greatest challenge facing the consequences of contention literature: endogeneity. Above, we identified how some researchers have already started to address this problem, but it is clear that all research on this topic needs to address it in some manner. Based on this broader discussion, we now outline more specific puzzles and prospects for the literatures on political and economic consequences of contention, respectively.

Specific Puzzles and Prospects for the Study of Political Consequences

Despite significant progress in the examination of political consequences, the literature is missing a comprehensive analysis of political institutions and behavior in the long term, especially beyond the present focus on civil war in the developing world or the impact of terrorism in the West. A fruitful area of research in the last ten years has been the micro-level literature directed toward understanding how contention influences individual attitudes and behavior in many situations,

both real and experimental. There are some limitations to this work, however, and thus some opportunities for further development.

First, much of the empirical evidence is contextually limited, drawn from field research in specific countries such as Burundi, Sierra Leone, Uganda, Israel, and Nepal. This raises questions about external validity and the possibility of generalizing across settings. More use of similar instruments and interventions across contexts—accompanied by an increased willingness of the scholarly community to publish such replication efforts—could help us better understand whether previous findings travel across contexts. Resources like the new xSub project could help with these efforts (Zhukov et al. 2019).

Second, the underlying mechanisms for the micro-level findings are not always clear, and more attention to mechanisms may help us better adjudicate between different and seemingly contradictory findings. For example, exposure to political conflict and violence may on the one hand reduce prosocial attitudes because of threat-induced fear and anxiety, but on the other hand it may make individuals more likely to compromise if concessions mitigate risk and exposure to future threat (Beber et al. 2014, Gilligan et al. 2014).

Third, and related to the discussion above, more explicit modeling of conditioning variables may result in a more coherent picture. Whereas prosocial effects have been found across different settings and types of violence—including crime exposure (Bateson 2012)—many of the effects are likely to be conditional on the form of contention itself, as well as the embeddedness of the individual in a specific political context. These conditioning variables are rarely modeled explicitly.

Finally, we need a better understanding of how the micro-level patterns aggregate to macro-level outcomes, and more linkages between the different subfields focusing on political consequences of contention. For example, how do institutional characteristics, regime behavior, and political developments more broadly reflect individual-level behavior, attitudes, and emotions in societies affected by violence? Focusing more on the role of collective agents—such as political parties and civil society—may assist in bridging the gap between macro- and micro-level perspectives.

Specific Puzzles and Prospects for the Study of Economic Consequences

The literature relevant to the topic has developed to such an extent that revisitation would not be especially valuable for those undertaking it, unless one was going to explore micro-foundations, which remain neglected, or attempt a more thorough investigation of a different form of contention (e.g., repression). We suggest both but highlight the second because such a study is important for many reasons.

First, we do not expect that repressive behavior will have the same kind of dramatic physical consequences in terms of destruction as organized armed conflict—that is, unless it reaches the level of genocide, where it might actually exceed the effects of civil war. Generally, however, for many of the mechanisms that Collier (1999) identifies besides destruction (i.e., disruption, diversion, and dissaving), repression may very well have similar (i.e., quite damaging) economic consequences as armed conflict. A particularly interesting question here is whether a repression trap exists, akin to the conflict trap (Collier et al. 2003) discussed above. Some preliminary evidence suggests that this is the case. Poe & Tate (1994), Davenport (1995), and Sullivan et al. (2012) show that repressive behavior breeds repressive behavior, which constitutes one important part of creating a “trap.” However, substantially more research is needed to uncover whether and how repression hurts economies and how, if it does, this harm is related to future incidence of repressive activity. As noted, some relevant work on aid and trade exists, but there is essentially nothing on other consequences.

Second, one particular type of economic consequence seems to call out for attention: inequality, and in particular its horizontal form (i.e., between ethnically, linguistically, or religiously defined groups). The reasons for such a focus are compelling. For example, the problem of inequality is one of the largest problems remaining for humankind as well as social science research. The toll that inequality takes seems devastating, but attention to the subject is relatively limited. The impact of contention also appears to be relevant. Taking a long-term view, Scheidel (2017) argues that mass, collective violence is one of the only forces strong enough to fundamentally change patterns of inequality in society. If this is indeed the case, then the economic impact of contention in terms of inequality appears to be substantial. Our current understanding of how this works, however—especially across diverse forms of contention over time—is limited.

Going forward, it is clear that we need to focus on integrating findings and insights across different literatures. Furthermore, there is a pressing need to broaden the lens through which researchers study this topic; we must stop treating specific types of contention as isolated, discrete events and specific types of consequences as the only characteristics of interest. This broader conception and more consistent attention to the linkages between other consequences will allow us to see what research is being done, where reasonable conclusions are being reached, and where additional investigations would be useful. Only in this manner will the research discussed above be improved.

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