*Data Set Feature*

Journal of Conflict Resolution
2019, Vol. 63(6) 1542-1578

DOI: 10.1177/0022002719842657
journals.sagepub.com/home/jcr
SAGE

# Sovereignty Rupture as a Central Concept in Quantitative Measures of Civil War

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## Abstract

Empirical studies of the causes or consequences of civil war often use measures that do not correspond to theory and results are sensitive to small changes in the coding of civil wars. Civil war is an instance of "sovereignty rupture" and is inherently a polity-level phenomenon, but that understanding of civil war is not reflected in data in which civil war is coded as a dyadic conflict—the state fighting a domestic challenger. We demonstrate the consequences of conceptual ambiguity about which conflicts to code as civil war and when to code the start and end of a civil war. Using a new data set of civil wars from 1945 to 2016 that is consistent with the concept of sovereignty rupture, we replicate several studies and find that their results are often overturned or weakened when we use our data. We advocate for greater deliberateness in data selection in civil war studies, focusing on the fit between the question of interest and the concept of civil war that is underlying a given data set.

## Keywords

civil wars, internal armed conflict, conflict, data

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Civil wars are large-scale armed conflicts between the government of a sovereign state and domestic challengers. Empirical analyses of the causes and consequences of civil war are premised on our ability to clearly define and measure the concept we are trying to explain, yet scholars classify instances of violent conflict into categories such as civil wars, coups, riots, genocides, or terrorism based on criteria that are fairly arbitrary. The term "civil war" is now used interchangeably to refer to conflicts as large as Syria's multiyear violence that caused more than 400,000 deaths and displaced about half of the country's population as well as any conflict between the state and a domestic armed group causing twenty-five or more battle deaths in a year (see, e.g., Asal et al. 2016; Gleditsch et al. 2002).

We highlight two fundamental conceptual questions about how to code civil wars. First, should civil wars be considered as events that happen to a society *in the aggregate* or should they be conceptualized as dyadic conflicts between the state and an armed group? The current trend in the literature favors the dyadic approach, which we argue is useful in some contexts, but can also create problems such as artificially inflating the number of civil wars in a given country while ignoring interdependencies between these dyadic conflicts. Second, how should acts of armed conflict in a given country be counted? Should they be combined into a single case of civil war based on temporal continuity of violence even when there are large gaps in the fighting? Or should different categories of events be coded when different forms of violence succeed each other in a process of unfolding conflict with transitions into and out of civil war coded to reflect changes in the organization of violence? These questions frame an exploration of the implications of different coding rules for civil war. We demonstrate that coding differences matter for inferences drawn about the causes and consequences of civil war.

Our main argument is that at the core of the concept of civil war is the rupture of state sovereignty, and therefore, civil war inherently occurs at the level of the polity. However, that concept is not reflected in studies that adopt an understanding of civil war as a purely dyadic phenomenon or as a phenomenon defined entirely by technical criteria such as violence thresholds or periods of inactivity. We explain how to use the concept of sovereignty rupture to code conflict data that are appropriate for the analysis of macro-level questions about the onset, duration, termination, or recurrence of civil war. We use our new data, which cover the years 1945 to 2016, to show differences in trends of civil war, making comparisons to the most commonly used database (the UCDP/PRIO Armed Conflict Dataset, or ACD). We then replicate several studies that use ACD data and show that several important results on the causes or consequences of civil war depend heavily on how civil war is coded.

## Conceptual Questions Implicit in the Selection of Civil War Data

Different lists of civil war reflect different assumptions about the unit of analysis and the definition of continuing conflict. We highlight two concepts—aggregation and

continuity—that are crucial for the definition and measurement of civil war. Regarding aggregation, we ask whether civil wars take place between specific actors, or if they are phenomena that acquire meaning only at the level of the political community? Regarding continuity, we ask what defines ongoing conflict as opposed to the end of one civil war and the beginning of a new one?

## *The Aggregation Question: Actor Dyads or the Polity as the Locus of Civil War?*

First-generation quantitative studies of civil war over the past two decades were based on cross-country comparisons using aggregate-level data on violence (Fearon and Laitin 2003; Collier and Hoeffler 2004; Elbadawi and Sambanis 2002; Hegre et al. 2001). In a sharp departure from that approach, second-wave studies use disaggregated data by studying conflicts between the state and armed groups, or conflicts in subnational regions.[1] That shift can help address a number of important questions, but increasingly it has also led to a view of conflict as a dyadic phenomenon and civil war is now discussed as an event that occurs between the government and a rebel group (e.g., Cunningham, Gleditsch, and Salehyan 2009). While disaggregating conflict data has clear advantages, there are also costs, which are not well-understood. Implicit in the new dyadic frameworks is a conceptual shift of war as a phenomenon that does not affect a country as a whole and is rather circumscribed by the intensity and type of violence that occurs between the state and individual challengers. What do we lose by thinking of civil war in that way?

Disaggregation of civil wars into dyadic armed conflicts is appropriate if we want to explore questions about the organization and behavior of armed actors. By contrast, analysis of civil war onset or recurrence is less amenable to such disaggregation as in most civil wars several actors challenge the state and the emergence of conflict dyads is *endogenous* to societal-level political outcomes.[2] Disaggregation can help illuminate a different set of questions, such as why some groups use violent as opposed to nonviolent tactics to pursue their goals (Sambanis and Zinn 2006; Cunningham, Dahl, and Fruge 2017). However, the question of why conflict escalates to civil war introduces complex interdependencies between social groups and conflict actors that cannot be properly accounted for in a purely dyadic framework unless the model changes accordingly. These interdependencies are usually not modeled in studies of macrolevel conflict outcomes that are based on dyadic data, and moreover, the dyadic approach is often applied inconsistently as evidenced by the fact that the government is always included as a unitary actor in all dyads, without regard for the fact that governments can be as fragmented as many of the rebel groups that are coded as distinct actors in dyadic data sets of civil war.[3]

Macrolevel questions about civil war put the problems of dyadic data into sharp relief. Projects that investigate the consequence of armed group fragmentation are a telling example. When we ask a macrolevel question such as what explains war duration, the dyadic approach can be misleading. The key problem is that *the dyads*

*themselves are often endogenous to the outcome under study*. Assume that we want to test a theory that factionalism is more likely to occur when groups are not strong enough to win a decisive victory, due to disagreements about strategy between moderates and extremists. If wars that do not end in decisive victories last longer, then there should be more factionalism and more dyads in longer wars. The unit of analysis is thus endogenous to the dependent variable. There is no easy fix for this problem and a more theoretically consistent approach would be to analyze war duration using country-level data in which all dyads that correspond to a single instance of sovereignty rupture are aggregated up.

The study of the effects of factionalism highlights a second problem related to aggregation: in some cases, factionalism results in the start of a new conflict that should be coded as a separate civil war, whereas in other cases, factionalism takes place within the same instance of sovereignty rupture. In the Philippines, for example, a war between the government and Moro guerillas started in 1971 with the rebels initially represented by the MNLF. Splintering on the rebel side in 1984 led to the formation of the MILF, which continued to fight after the MNLF signed a settlement with the government in 1996. We code a single, ongoing war in that case, as our research suggests that the MNLF and MILF represent a single instance of sovereignty rupture. By contrast, the chronologically overlapping war between the government of the Philippines and the NPA represents a separate instance of sovereignty rupture, which we distinguish as such by coding it as new war starting in 1972 and ending in 1992 (see Supplemental Material for more details).

An advantage of conceptualizing war as a dyadic phenomenon is that it allows us to study the effect of policies targeting specific groups. However, the complex interdependencies that arise from this conceptualization are rarely taken into account in empirical contexts: if rebellious group A is offered concessions, is it because another group B is also challenging the state and do the government's strategies toward one group affect the other group's actions, as well as those of a third group C, that might decide to rebel in the future? These complex interdependencies strain the assumptions underlying empirical models that are commonly used in studies of civil war. Fully exploring the implications of these interdependencies is beyond the scope of this article; to our knowledge, this issue has not been adequately addressed in the previous literature.

## *The Continuity Question: Lasting Conflicts or Transitions In and Out of War?*

The sovereignty rupture idea suggests a theoretically grounded approach to deal with the question of what constitutes continuing conflict. Most coding differences across data sets are due to disagreements over how we distinguish new conflict onsets from long-lived wars in which violence ebbs and flows, marked by periods of inactivity. The prevailing practice has been to use a high death threshold to distinguish civil wars from other forms of conflict, though many studies use the term "civil war" liberally to refer to minor conflicts with more than twenty-five deaths.[4] Uppsala/

PRIO's ACD (Allansson, Melander, and Themnér 2017; Gleditsch et al. 2002) codes episodes of conflict that are usually interpreted as civil wars with a new onset coded each time a conflict causes twenty-five battle deaths as long as there was no conflict of similar or higher intensity in the previous period.[5]

Annual fatality thresholds help identify low-level violence and large-scale conflict separately. The Correlates of War (COW) Project's threshold of 1,000-battle deaths per year was the first instance of this. Many authors use the minimum threshold of twenty-five battle deaths to classify cases as civil wars. The ACD provides data on higher-intensity conflicts and most authors using this data set identify civil wars as those conflicts with "at least 1,000 battle-related deaths in a given year."

Data on low-level violence are a useful resource that can help researchers study escalation processes and the ACD is the best available data set for low-level armed conflict. However, there are problems in separating several periods of low-level conflict from a civil war that encompasses all those periods and strict applications of death counts can be misleading. A given conflict can phase in and out of civil war depending on the yearly death count. In principle, using a measure of conflict intensity to identify periods of civil war seems reasonable. Yet, far from capturing a core concept of civil war, this definition can create fairly arbitrary episodes of conflict.[6]

In some cases, the political disruption that citizens and researchers alike identify as civil war may be ongoing during years of low-level conflict when no civil war would be coded. In other cases, low-level violence that most observers would characterize as residual conflict that takes a different form (e.g., terrorism) can occur shortly after the end of a civil war. Thus, the application of a strictly numerical threshold might result in a purely mechanical coding of civil war that is unrelated to variation in institutional variables of theoretical interest. For example, the most recent version of the ACD codes some form of conflict in Colombia from 1964 through 2013. But during this period, only 1985, 1992, 1994, 1996, 1999 to 2002, and 2004 to 2005 are coded as meeting the 1,000-fatality threshold. How should this case be coded—is it a single civil war, with periods of high- and low-intensity violence? Or several distinct civil wars with several new onsets?

The sovereignty rupture concept helps us address the continuity question in a less arbitrary manner than the annual death thresholds. Specifically, consider how transitions in and out of conflict should be coded in an ideal case where the rebels win, taking control of the capital. Victory is evidenced by the regime transition and might or might not be followed by a cessation of hostilities. The new government almost invariably implements new policies that, in theory, have the potential to pacify the country. If violence resumes shortly after this transition and the new regime is opposed by members of the old regime who are now in the opposition, is this a new war or a continuation of the previous one? Our approach in such cases is to code a new civil war representing a new instance of sovereignty rupture, as we explain below.

## Civil War as a Rupture of Sovereignty

We understand sovereignty rupture to mean a challenge to an incumbent government's role as the ultimate arbiter of behavior within a polity.[7] Understood in these terms, the rupture of sovereignty that results from the violent contest between the governing authority and its opponents constitutes the core feature of civil war.[8] The concept provides a logically consistent framework within which we can delimit the macrolevel process of civil war, that is, onset, duration, and termination, and resolve the aggregation and continuity questions.

### *Sovereignty Rupture and the Aggregation Question*

A single rupture of sovereignty often encompasses multiple dyadic conflicts. Breaking down a conflict into dyadic relationships necessarily changes the types of questions we can ask. Specifically, questions such as "what causes ethnic war" when asked with reference to a country can reasonably be understood as a single instance of sovereignty rupture. Within the context of a broader conflict, individual dyadic relationships can rise to the level of conflict intensity that are classified as civil war but that relationship necessarily depends on the dynamics of the broader conflict. The form of the incumbent-challenger contest may shift over time—new armed actors emerge, existing ones unite and fragment, and form alliances and break them. War is the sum total of these interactions, the continuing struggle between the incumbent and challengers. While we may observe the balance of power between the parties to the conflict change over time, the changing fortunes of the combatants do not define the event of civil war. We are interested in who has won or lost a particular war rather than who is winning or losing within it. Emphasizing the rupture of sovereignty therefore allows us to distinguish between events that occur simply in the process of ongoing war and a period in which war ends with regime change, but the new government faces armed resistance or insurgency.

The movement to disaggregate conflict data is appealing for many reasons, but if taken to its logical conclusion can result in treating government-armed organization dyads as the unit of analysis. A case example illustrates the pitfalls of treating new dyad formation as equivalent to new war onsets. We code the Second Palestinian Intifada as a war beginning in September 2000. The practice of coding dyads would generate separate observations for multiple Palestinian armed political organizations. The UCDP/PRIO dyadic data set, for example, lists five conflict dyads in 2001, involving Israel on one side, and individually on the other side the Palestinian National Authority (PNA), Fatah, the Popular Front for the Liberation of Palestine, Hamas, and Palestinian Islamic Jihad (UCDP/PRIO dyadic data v17.2). Yet all of these organizations were active within a single instance of sovereignty rupture. Indeed, it is methodologically problematic to treat these five dyadic observations as independent in a statistical analysis. If we were to include them all in a study of onset, for example, we would in effect be overweighting the impact of any covariates

that are not disaggregated down to the organization level, and, crucially, any that capture the behavior of Israel, or characteristics of its government or the country at the time.

Moreover, the use of dyadic data can produce misleading inferences when the number of dyads changes over time. In the previous year of the Second Intifada, 2000, the same data set codes two dyads, Israel and the PNA, and Israel and Fatah. The increase in dyads between 2000 and 2001 might lead a researcher using the dyadic data to conclude that government- or country-level covariates observed in 2000 are associated with new organizations' entry into an ongoing conflict. Changes in the number of dyads, however, may very well result from the process of onset simply playing out over time as organizations may proceed with military operations at a deliberate pace.

Dyadic conflicts that collectively represent a single rupture of sovereignty should be aggregated to a single macrolevel contest as civil war, provided it meets other coding criteria on which the literature has settled, such as a threshold of violence, effective resistance, and so on. For many applications, coding civil war around the concept of sovereignty rupture captures the on-the-ground reality; disaggregation can be a lens that obscures rather than clarifies.

The question of the correlates of counterinsurgency success provides an example of the potential pitfalls of disaggregation. During the height of Iraq's civil war (2006–2007), at least fifteen major militias were active;[9] and observers have counted scores of distinct armed groups through the war's various phases.[10] Should a dyadic conflict be coded for each one of these factions?[11]

For the macrolevel outcome of counterinsurgency success, the dyadic approach may fall into a trap of overstating the importance of fragmentation. It treats the formation of a rebel splinter group as equivalent to an entirely new rebel group entering into the conflict. Research using dyadic data might therefore highlight certain time-varying factors as linked to counterinsurgency failure based on observations that include numerous examples of splinter groups as the basis for coding the onset of conflict. But, a splinter group could be a sign that the government has begun to turn the tide in the war, with military pressure leading rebel groups to fragment (e.g., Staniland 2014, 39). The formation of a splinter group need not indicate a fundamental failure of government attempts to quash a rebellion. Using dyadic data could therefore lead scholars to mistakenly conclude that certain factors are associated with failed government attempts to quash rebellions when in fact those same factors likely point to future counterinsurgency success.

## *Sovereignty Rupture and the Continuity Question*

Viewing civil war as a rupture of sovereignty forces us to recognize shifts in sovereignty as a critical component of defining war onset or termination. From the opposition's perspective, deposing the sitting government and seizing power eliminate the rupture of sovereignty by placing sovereignty in the hands of the

opposition. A new rupture of sovereignty may occur in short order—in this case, from the deposed government mounting an ongoing challenge—and continued fighting between the newly ensconced rulers and the former government should be understood as a new civil war. Thinking about whether a sovereignty rupture continues or is resolved changes the way episodes of civil war are coded. Table 1 lists the types of questions that emerge with respect to continuity, the guidance provided by the sovereignty rupture framework in answering these questions, and country examples.

Rebel victories illustrate the need to use sovereignty rupture in coding war termination and new war starts. If rebels win and government changes hands, but violence continues with no interruption or with only a short interlude with no fighting, most data sets would code a single episode of civil war. Afghanistan is such a case, where many data sets code a single ongoing civil war since 1978. This ignores several regime transitions that occur as a result of rebel victory and collapse into new violence that represents new instances of sovereignty rupture. From that perspective, one could code four different civil wars in Afghanistan with new onsets in 1992, 1996, and 2001 (see Supplemental Material for a detailed discussion). For studies that consider the relative stability of military victories versus negotiated settlements, the implications of these coding differences are clear since by combining all these episodes into a single civil war one would effectively expunge three cases of rebel victory that fails to establish peace.

A counterargument might be that the decision to code a single war event is driven mainly by the observation of continuing high levels of violence. But focusing simply on violence levels misses the point, as illustrated by the example of the Chinese civil war from 1946 to 1949 and the residual conflict that followed it, from 1949 to 1953. Although the bulk of the Chinese Nationalist Party's (KMT) military forces were evacuated from the Chinese Mainland by 1949, some KMT units remained behind, as did local elites who chose not to flee to Taiwan. Prior to its final evacuation from the Chinese mainland, the KMT distributed arms to local elites and militias and a number of its armies in Southern China crossed into Burma and Thailand. From late 1949 to 1953, the Chinese Communist Party (CCP) engaged in what it called "the suppression of bandits and local despots" throughout Southern China.[12] This conflict is distinct from the land reforms and political purges that took place in other areas under the CCP's control, which would be classified as politicides.[13] According to our sources (see details in the Supplemental Material), thousands were killed and the hostilities ended around 1953. This episode was clearly related to the broader Chinese civil war; we code it, however, as a separate period of state consolidation in which the new government purges pockets of armed opposition in what amounts to a new civil war.

A different question related to continuity concerns is when to code an end to war due to inactivity (lack of fighting). Sharp differences in coded onset and termination of wars arise if annual death thresholds are used strictly to code war termination versus a more encompassing approach in which cumulative death thresholds are

**Table 1.** Continuity Coding Questions and the Sovereignty Rupture Framework.[a]

| Question | Country Example | Sovereignty Rupture Guidance | War Dates in Line with Sovereignty Rupture |
|---|---|---|---|
| Does a rebel victory define start of new war if violence continues? | Afghanistan, 1978–present | Acknowledge shift in sovereignty that rebel victory entails; with these victories, continuing violence represents new war, fought between new government and its challengers | 1978/4–1992/2<br>1992/2–1996/9<br>1996/9–2001/10<br>2001/10-ongoing |
| | Cambodia, 1970–1991 | | 1970/3–1975/1<br>1975/5–1991/10 |
| | Somalia, 1991–present | | 1991/5–2006/6<br>2006/6–2006/12<br>2007/1–ongoing |
| Does residual violence after rebel victory constitute a new war? | China, 1946–1953 | If violence meets threshold for war onset, constitutes rupture of the sovereignty now being exercised by the rebel group-turned-government | 1946/3–1949/1<br>1949–1953 |
| How much inactivity is necessary to code an end to war? | Laos, 1960–1979 | Peace agreements and military victories represent mending of sovereignty rupture. Shorter inactivity period required to identify war end following agreement or victory versus absence of agreement or victory and inactivity | 1960/10–1961/6<br>1963/4–1973/2<br>1976/5–1979 |
| | Chad, 1965–2010 | | 1965/10–1979/6<br>1980/3–1982<br>1982–1987/6<br>1990–1994<br>1998–2003<br>2005/12–2010/9 |

*(continued)*

**Table 1.** (continued)

| Question | Country Example | Sovereignty Rupture Guidance | War Dates in Line with Sovereignty Rupture |
|---|---|---|---|
| Should a strict fatality and effective resistance thresholds be applied for every year of the war? | Turkey, 1984–present | Sovereignty rupture does not end because conflict fatalities dip slightly below a strict threshold. Use lenient fatality threshold criteria to code war end, but ones that distinguish between ongoing war and termination due to activity followed by a new war start | 1984–2003<br>2005–ongoing |
| | Colombia, 1964–2015; ACD v17.2 codes some form of conflict during this period, but conflict at level of 1,000 deaths only in 1985, 1992, 1994, 1996, 1999–2002, and 2004–2005 | | 1948/4–1966<br>1978/11–2015/9 |

[a]The Supplemental Material provides descriptions of several cases and coding decisions listed here; all others are contained in the coding notes. Ongoing indicates that criteria for war end had not been met as of December 31, 2016.

used, effectively combining periods of violence into a single conflict even if they are interrupted by periods of inactivity. In the Supplemental Material, we illustrate this using data from the ACD, contrasting two different ways Mozambique's civil war could be coded. Selecting a high threshold of 1,000 deaths would lead us to code the war as starting in 1972 ending in 1973, starting again in 1981 and ending in 1991. Using the cumulative death criterion, the war would be coded as starting six years earlier (1966), ending in 1974, restarting in 1981, and ending in 1992. The war would now be coded as restarting in 2013, ending in 2016 because the same party, RENAMO, was engaged in lower-level armed conflict.

Coding war termination on the basis of violence thresholds is satisfactory from the standpoint of war-as-sovereignty-rupture only in cases where the government

prevails since suppressing armed opposition to the point of inactivity eliminates the rupture of sovereignty. However, rebel victories or settlements represent shifts in sovereignty. The establishment of a new political order implies that the sovereignty rupture represented by war has ended. Thus, such events should constitute a basis for coding war termination rather than strictly adhering to violence thresholds, according to which war termination might not occur until years later. The shift in sovereignty might not correspond to a reduction in violence (although it usually does).

Chad provides an example of how these coding practices result in substantial differences between data sets. Coding war termination only with reference to levels of violence would lead to recording three fewer instances of war termination than our approach of also noting victories that result in regime transition or with peace agreements that stop the fighting for a period of six months; our data acknowledge the rebel victory in 1979, Habre's capture of the government in 1982, and the government victory in 1987.

When coding termination due to inactivity, the coding rule underlying the data influences what questions the data can help us address. If we want to know whether or not regime transition affects the risk of war recurrence, then coding war termination on the basis of a death threshold alone would be insufficient. Recognizing the resolution of the sovereignty rupture reflected in the regime transition implies that war termination would be coded at the time of rebel victory (or settlement) and such a coding rule would be better suited for the question at hand. Thus, how we code termination has obvious implications for studies of war duration or recurrence.[14] Table 1 summarizes the main "continuity" issues and presents examples that illustrate how differently these cases are treated in the leading data set as compared to our own coding based on the concept of sovereignty rupture.

An intuitive and influential argument contends that military victory is more likely than other forms of civil war settlement to lead to a stable, lasting peace.[15] But to study postconflict peacebuilding requires civil war coding rules that properly address the continuity question.

Consider the difference between applying a yearly death threshold versus our coding criteria for conflict termination to the case of Somalia between 2006 and 2007. According to the ACD, there were at least twenty-five battled-related deaths in 2006 and at least 1,000 in 2007. A twenty-five yearly deaths threshold would therefore result in coding ongoing war for this entire period.

These two years, though, were ones of anything but continuity in the Somali polity. Between February and June 2006, the emerging Islamic Courts Union successfully consolidated territorial control against the alliance of groups that had controlled the capital of Mogadishu. Next, from June through December, the ICU government faced strong resistance from the forces of the Transitional Federal Government (TFG), which was backed by Ethiopian troops. The TFG-Ethiopian forces defeated the ICU in December, but by the end of January 2007 were battling an insurgency against their newly established rule (Menkhaus 2007, 374, 380-82, 385-86).

Our coding rules clarify that these transitions in government represent end points for what had been ongoing wars. We code three distinct wars during the period in question, one of which continued after 2007, while the use of a strict yearly death threshold criterion would lead to coding ongoing war in Somalia from before 2006 through 2007 and beyond. Our approach recognizes two victories during 2006, neither of which produces postconflict peace, while a strict yearly death threshold approach is more likely by design to generate data that associate victory with postconflict peace because it only classifies events that are not followed by additional fighting as victories.

## *Coding Multiple, Temporally Overlapping Wars in One Country*

Identifying distinct civil wars in a single country during a given time period is appropriate if these constitute separable instances of sovereignty rupture. A challenge to a government's sovereignty has two dimensions—political stakes and strategic coordination. In center-seeking rebellions, this rupture is total; the challengers dispute the sitting government's claim to rule, over the complete territorial extent of the polity. Thus, if several groups arise in a revolutionary civil war, each aiming to capture the state, we would code a single war by virtue of the single, overarching rupture of sovereignty in the country. In contrast, the rupture of sovereignty in a secessionist war is partial: while a secessionist group seeks to entirely displace the sitting government in a region of the country, its claims do not necessarily extend to the government's sovereignty over the rest of the polity, and often, the violence can be geographically confined. In such cases, more than a single war can be coded in the country if the state faces two unrelated separatist movements.

The extent to which challengers coordinate political and military strategies should also be reflected in coding decisions. The closer the direct or indirect coordination between armed groups, the stronger the evidence that the war represents a single sovereignty rupture. We illustrate this with an example from Myanmar's political conflicts. Table 2 summarizes the use of sovereignty rupture to distinguish multiple, chronologically overlapping wars for the Myanmar example that follows, and the additional example of Ethiopia between the mid-1970s and 1990.

Myanmar (Burma) is infamous for having experienced "the world's longest-running civil war" (Economist 2013). Violent conflict erupted immediately following independence in 1948, and a communist insurgency was quickly followed by a series of rebellions by ethnic minority groups (Smith 2002). More than thirty separate armed groups fought against the state at different times (Smith 1999, 2002; Human Rights Watch 2002). The government managed to end the communist insurgency in 1988, but could not quell the ethnic rebellions until 1998, through a series of cease-fires with the major groups. Low-level violence has continued through the present.

Our concept of sovereignty rupture leads us to code the first round of ethnic rebellions (1948–1951) as a single war distinct from the communist insurgency

**Table 2.** Sovereignty Rupture as the Basis for Coding Multiple Civil Wars in a Single Country During a Given Time Period—Examples.

| Country Example | Sovereignty Rupture Guidance | Coding Result |
|---|---|---|
| Myanmar, 1948–1995 | Code separate war for the ethnic rebellions and the communist insurgency due to distinctive political stakes<br>Code a single war encompassing multiple ethnic rebellions due to common political stakes | 1948–1951 (Ethnic rebellions)<br>1948–1988 (Communist insurgency)<br>1960–1995 (Ethnic rebellions) |
| Ethiopia, 1974–1991 | Code separate Eritrean secessionist and Ogden irredentist wars due to distinctive political stakes. Distinctive stakes mean that coordination between Eritrean and Tigrean groups is an insufficient basis upon which to aggregate Eritrean secessionist war and center-seeking rebellion<br>Combine center-seeking rebellions into one war based on common political stakes | 1974–1991 (Eritrean secession)<br>1978–1991 (Center-seeking rebellion)<br>1976–1988 (Irredentist war, Ogaden) |

*Note:* See Supplemental Material for descriptions of the Ethiopia cases and coding decisions.

(1948–1988), followed by a subsequent round of ethnic rebellions as a third war. The communist insurgency and ethnic rebellions had fundamental differences in the political stakes (the Burma Communist Party [BCP] wanted to replace the existing central government, while the ethnic rebellions pushed for secession, autonomy, or a new federal structure). As Silverstein (1990, 120) summarizes, "Unlike the minorities, the BCP wanted a united and centrally controlled Burma, not an ethnically divided federal union." Therefore, even a period of tactical coordination between the BCP and ethnic armed groups in the late 1980s did not represent a shift in the nature of the war. The links between the different ethnic rebellions are less straightforward than is the difference between them and the communist insurgency.

During the second war, a series of alliances brought together multiple minority groups. One of the more successful efforts at a formal alliance between the minorities came in 1976 when The National Democratic Front was formed by armed groups representing eight of the ethnic minorities—the Arakanese, Kachin, Karen, Karenni, Lahu, Palaung, Shan, and Pa-O. It grew and later expanded to also include the Wa, Mon, and Chin (Silverstein 1990). The NDF existed to unify the efforts of all anti-government forces and included a mutual defense provision. This codified the complementarities between the rebellions by the many armed groups, and when viewed from the perspective of the Burmese government, the ethnic rebel groups

represented a single threat to the territorial integrity of the state. This is reflected in the government's strategy for ending the conflict: it offered cease-fires and autonomy deals to all ethnic groups and by getting some to enter into these agreements, it reduced the armed threat posed by the remaining groups. Thus, our sovereignty rupture concept leads us to combine these conflicts into a single one as opposed to coding up to thirty separate conflict dyads.

## New Data on Civil Wars

We now apply our insights about the coding of civil war to extend and slightly revise the data set created by Sambanis (2004). The revisions are designed to improve the consistency of the coding with the concept of sovereignty rupture. Our data set covers the 1945 to 2016 period. All coding decisions are explained in detail in Coding Notes available online, and the Supplemental Material provides a list of cases that have been recoded.[16]

Mapping trends in the onset of civil war is a useful starting point. We explore these for our data in Figure 1, in which we also plot wars as coded by Uppsala/PRIO's ACD. Although the incidence of civil wars was declining since the end of the Cold War, we observe a sharp rise in the past decade. The uptick in the number of wars in the last decade is due to a resurgence of ethno-sectarian conflicts that challenge Gurr's (2000) early predictions of the decline of ethnic war. There is overall a declining linear trend of war incidence in our data since 1990 (Figure 1, triangles/red line), while the trend is flat in the ACD (using the cumulative death criterion; left panel). The differences are smaller if we fit a quadratic trend line (Figure 2), though the lines intersect if we use the annual death threshold. There is also a noticeable and growing gap after year 2006, which we believe is a consequence of the coding approach taken by the ACD and, specifically, how they code conflicts involving the Islamic State.[17] Using the annual conflict intensity ACD coding (right panels in Figures 1 and 2), the trend lines appear more similar, though there is a large gap in the number of wars coded in the ACD and our data with significantly fewer country-years of war coded in the ACD.

Comparing war onsets between our data and ACD, we see broadly similar negative trends since 1990 in the second panel of Figure 1 (using the annual 1,000 death threshold for ACD conflicts). Figure 2 shows that the divergences in the trend lines in our data compared to ACD grow depending on whether we use the ACD 25-battle deaths criterion or the 1,000-battle deaths criterion, which is to be expected given the preceding discussion. The uptick in war onsets since the early 2000s might constitute a distinct set of cases of civil war as several of these conflicts are the outgrowth of the US invasion of the Iraq invasion in 2003 and of regime instability associated with the Arab Spring uprisings beginning in 2011. Figures S3.1 to S3.4 in the supplement present this information in more detail, comparing trends in our data and ACD data using the 1,000-fatality

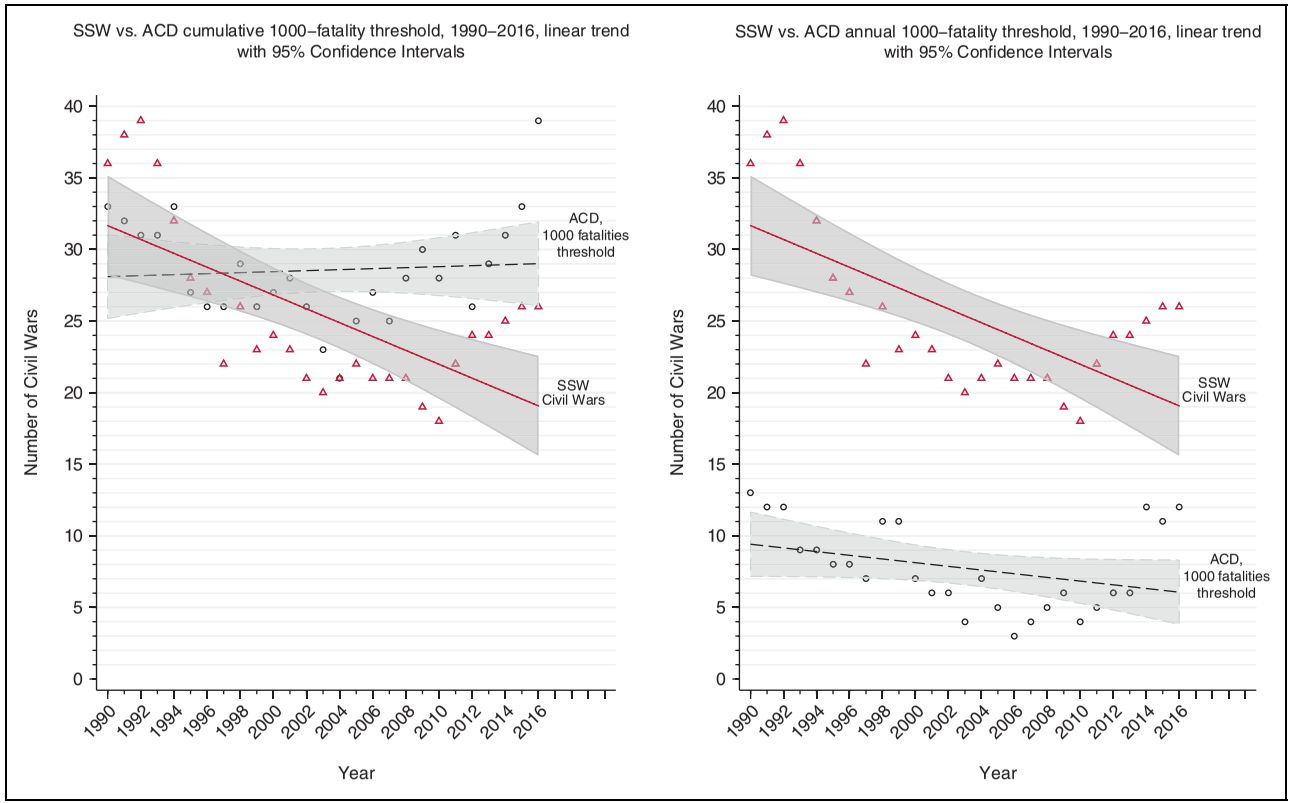


**Figure 1.** SSW versus ACD civil wars, 1990 to 2016. Red triangles represent the number of civil wars per year as coded by SSW, and black circles represent the number of civil wars per year that reached the 1,000+ battle deaths threshold in a given year as coded by ACD.

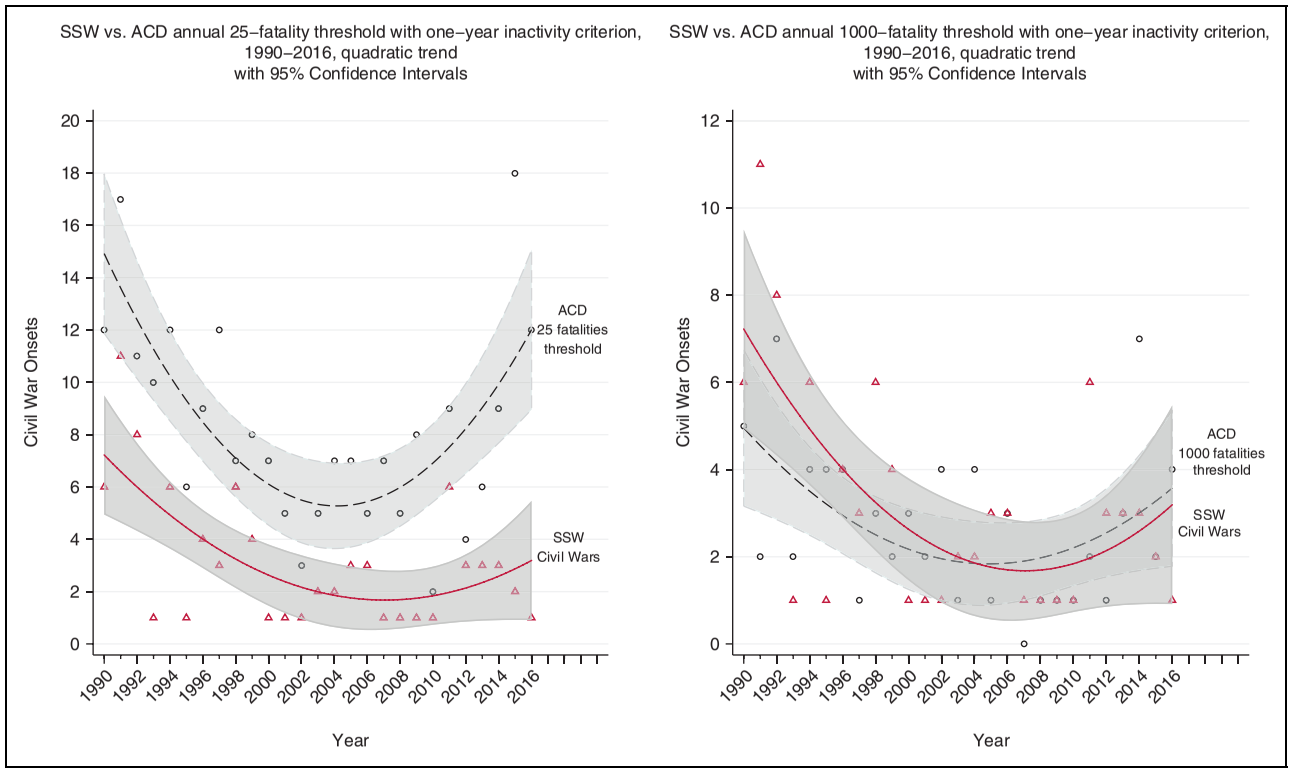


**Figure 2.** SSW versus ACD civil wars, 1990 to 2016. Red triangles represent civil war onsets as coded by SSW, and black circles represent civil war onsets as coded by ACD. Here, ACD onsets capture years where a conflict causes at least the specified number of battle-related deaths, provided that the conflict did not cause at least that number of battle-related deaths in the prior year.

threshold (Figures S3.1 and S3.2) and the annual fatality threshold for 25 annual deaths (Figure S3.3) and 1,000 deaths (Figure S3.4).

## *Replications of Studies Using Civil War as an Explanatory Variable—Focusing on Continuity*

Next, we use our data to revisit important substantive questions for which the incidence of civil war is a potentially crucial explanatory variable. Specifically, we replicate three studies and examine how the effect of using our data on their results: Graham, Miller, and Strøm's (2017) article on the effects of powersharing on democratic survival; Lai and Thyne's (2007) article on the effect of civil war on education; and Colgan's (2015) article on oil dependence, domestic-armed conflict, and democratization. For each study, our expectation is that the conceptual questions related to continuity that we addressed earlier are likely to have important ramifications for these analyses. To conserve space, we summarize the main research question and empirical results, followed by a brief discussion of each analysis.

Graham, Miller, and Strøm (2017) analyze the effects of three types of powersharing agreements—constraining, dispersive, and inclusive—on democratic survival. They find that only "constraining" powersharing has consistently positive effects and that, in the subset of countries that have experienced civil war, inclusive powersharing after war also promotes democratic survival, whereas dispersive powersharing does not. They define postconflict countries as those that have experienced a conflict that has caused at least 1,000 cumulative deaths according to the UCDP/PRIO data set. We replicate the analysis from Table 4, in their article focusing on the interaction of the postconflict indicator with each type of powersharing institution. We then replace their postconflict indicator with one coded in an identical manner but using our civil war data. Results are shown in Table 3. There is a marked change in the estimated effect of dispersive powersharing using our data: it goes from negative to positive and significant using the three model specifications in their main analysis. Using our data, the magnitude of the effect of inclusive powersharing changes substantially, and the effect of constraining powersharing attenuates and is not statistically significant in model 3. Thus, the results of this study are not robust to an alternative coding of civil war.

Lai and Thyne (2007) argue that civil wars should reduce education expenditures and school enrollments by destroying infrastructure and shifting expenditures from human capital development to military capacity. Using data from COW and the ACD, they show that civil war has a significant negative impact on school expenditures and enrollment but find no evidence that it leads to the reallocation of educational funds toward the military. They also find that civil war continues to exert a negative impact on educational outcomes into the postconflict period. Their civil war variables are binary indicators for country-years in which either COW or the ACD codes an ongoing intrastate conflict. COW codes civil war for all country-years in which a conflict caused 1,000 battle deaths. Lai and Thyne use a comparable

**Table 3.** Replication of Graham, Miller, and Strøm (2017) with ACD (1,000+ cumulative deaths; original) and SSW Data.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Table 4 Model 1 | | Table 4 Model 2 | | Table 4 Model 3 | |
| | ACD | SSW | ACD | SSW | ACD | SSW |
| Inclusive powersharing | −0.0648 | −0.0303 | −0.0534 | −0.00031 | −0.145 | −0.0520 |
| | (0.0771) | (0.0865) | (0.0933) | (0.111) | (0.102) | (0.124) |
| Dispersive powersharing | −0.0128 | −0.0424 | −0.0669 | −0.0809 | −0.0841 | −0.0973 |
| | (0.0941) | (0.0956) | (0.0966) | (0.0981) | (0.102) | (0.104) |
| Constraining powersharing | 0.397*** | 0.367*** | 0.323*** | 0.296*** | 0.353*** | 0.341*** |
| | (0.0839) | (0.0819) | (0.0925) | (0.0896) | (0.0973) | (0.0954) |
| Post–Civil War (ten years) | 0.593*** | 4.897*** | 0.736*** | 4.865*** | 0.797*** | 5.605*** |
| | (0.175) | (0.663) | (0.196) | (0.664) | (0.198) | (0.712) |
| Inclusive × Post–Civil War | 2.113*** | 18.77*** | 2.249*** | 19.02*** | 2.403*** | 22.32*** |
| | (0.450) | (1.989) | (0.599) | (2.092) | (0.628) | (2.314) |
| Dispersive × Post–Civil War | −0.440*** | 1.557** | −0.452*** | 1.432** | −0.489*** | 1.501** |
| | (0.162) | (0.723) | (0.166) | (0.683) | (0.166) | (0.725) |
| Constraining × Post–Civil War | 0.250 | 0.540** | 0.309* | 0.467** | 0.383** | 0.382 |
| | (0.162) | (0.233) | (0.172) | (0.237) | (0.184) | (0.241) |
| Ethnolinguistic fractionalization | −0.489* | −0.521* | −0.538* | −0.586** | −0.375 | −0.424 |
| | (0.264) | (0.275) | (0.280) | (0.28964) | (0.285) | (0.293) |
| Regional polity | 0.0990*** | 0.104*** | 0.0981*** | 0.101*** | 0.0913*** | 0.0944*** |
| | (0.0173) | (0.0183) | (0.0182) | (0.0190) | (0.0189) | (0.0197) |
| GDP/capita (logged) | 0.597*** | 0.548*** | 0.516*** | 0.470*** | 0.486*** | 0.440*** |
| | (0.0789) | (0.0808) | (0.0958) | (0.0930) | (0.0938) | (0.0913) |
| GDP growth | 0.00859 | 0.00856 | 0.00789 | 0.00809 | 0.00618 | 0.00607 |
| | (0.0105) | (0.0102) | (0.00966) | (0.00948) | (0.0112) | (0.0112) |
| Fuel dependence | −0.0110* | −0.0210*** | −0.0113** | −0.0202*** | −0.0130** | −0.0218*** |
| | (0.00586) | (0.00555) | (0.00557) | (0.00525) | (0.00569) | (0.00532) |
| Population (logged) | −0.0640 | −0.0383 | 0.0114 | 0.0280 | 0.00895 | 0.0234 |
| | (0.0420) | (0.0425) | (0.0455) | (0.0453) | (0.0456) | (0.0448) |
| Past democratic breakdown | −0.211*** | −0.224*** | −0.197*** | −0.203*** | −0.182*** | −0.183*** |
| | (0.0598) | (0.0617) | (0.0628) | (0.0646) | (0.0670) | (0.0685) |

*(continued)*

**Table 3.** (continued)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Table 4 Model 1 | | Table 4 Model 2 | | Table 4 Model 3 | |
| | ACD | SSW | ACD | SSW | ACD | SSW |
| Democracy age | −0.00468** | −0.00181 | −0.00783*** | −0.005 | −0.00843*** | −0.00576** |
| | (0.00234) | (0.00294) | (0.00273) | (0.00317) | (0.00259) | (0.00274) |
| Freedom house | | | 2.017*** | 1.712*** | 1.861*** | 1.452*** |
| | | | (0.411) | (0.395) | (0.424) | (0.412) |
| Horizontal constraints | | | −0.177 | −0.0418 | −0.157 | −0.0673 |
| | | | (0.398) | (0.393) | (0.422) | (0.415) |
| Recent regular turnover | | | | | 0.342*** | 0.281** |
| | | | | | (0.126) | (0.128) |
| Recent irregular turnover | | | | | −0.576*** | −0.524*** |
| | | | | | (0.183) | (0.178) |
| Polity | | | | | 0.0360 | 0.0388 |
| | | | | | (0.0273) | (0.0277) |
| Disruption | | | | | 1.021 | 0.634 |
| | | | | | (0.756) | (0.808) |
| Constant | −2.578*** | −2.395*** | −4.401*** | −3.919*** | −4.434*** | −3.838*** |
| | (0.844) | (0.869) | (1.019) | (1.025) | (1.040) | (1.030) |
| Years of Powersharing Cubic Polynomials | Y | Y | Y | Y | Y | Y |
| Observations | 2,181 | 2,181 | 2,137 | 2,137 | 2,124 | 2,124 |

*Note:* Robust standard errors are given in parentheses.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

**Table 4.** Replication of Lai and Thyne (2007) with ACD (1000+ Deaths/Year; Original) and SSW Data.

| | DV: % Change Education Expenditures | | | | DV: % Change Primary Enrollment | | DV: % Change Tertiary Enrollment | |
|---|---|---|---|---|---|---|---|---|
| | Table 1 Model 4 | | Table 1 Model 5 | | Table 2 Model 10[a] | | Table 2 Model 12[a] | |
| | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW |
| | (0.0646) | (0.0643) | (0.0650) | (0.0659) | (0.00946) | (0.00927) | (0.0287) | (0.0284) |
| Civil War | −0.0330* | 0.00180 | −0.0312* | 0.00292 | −0.0154** | −0.00521 | −0.0327** | 0.0141 |
| | (0.0176) | (0.0124) | (0.0179) | (0.0122) | (0.00659) | (0.00341) | (0.0135) | (0.0137) |
| Post–Civil War | −0.00322 | 0.0220* | −0.00313 | 0.0221* | 0.0115* | 0.0118** | −0.0110 | 0.0582*** |
| | (0.0149) | (0.0128) | (0.0151) | (0.0128) | (0.00602) | (0.00467) | (0.0179) | (0.0205) |
| Civil War × % Military Expenditures | | | −0.0383 | −0.0161 | | | | |
| | | | (0.0531) | (0.0346) | | | | |
| Polity | 0.000712 | 0.000645 | 0.000705 | 0.000642 | −0.000224 | −0.000201 | −0.00253** | −0.00237** |
| | (0.000597) | (0.000612) | (0.000596) | (0.000611) | (0.000173) | (0.000195) | (0.00114) | (0.00110) |
| GDP/capita (logged) | −0.00654 | −0.00289 | −0.00647 | −0.00284 | −0.0130*** | −0.0127*** | 0.00880 | 0.0143 |
| | (0.00465) | (0.00480) | (0.00468) | (0.00478) | (0.00159) | (0.00192) | (0.00994) | (0.0108) |
| % Nonmilitary government expenditures | 0.200*** | 0.197*** | 0.200*** | 0.198*** | 0.00560 | 0.00501 | −0.0336 | −0.0387 |
| | (0.0683) | (0.0680) | (0.0684) | (0.0680) | (0.00947) | (0.00931) | (0.0300) | (0.0295) |
| % Population | | | | | 0.538*** | 0.526*** | 0.946** | 0.863** |
| | | | | | (0.147) | (0.141) | (0.384) | (0.344) |
| Constant | 0.0969** | 0.0591 | 0.0961** | 0.0584 | 0.121*** | 0.118*** | −0.00952 | −0.0709 |
| | (0.0396) | (0.0411) | (0.0398) | (0.0410) | (0.0157) | (0.0189) | (0.0874) | (0.0971) |
| Observations | 1,362 | 1,362 | 1,362 | 1,362 | 1,681 | 1,681 | 1,207 | 1,207 |

*Note:* Standard errors are given in parentheses.

[a]We substitute Lai and Thyne's original postwar variable (one for all postwar years) for the dynamic one used in their Table II.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

civil war variable from the ACD, one that codes civil war for all country-years in which a conflict caused 1,000 battle deaths, but with one key difference from COW—they count only wars classified by UCDP/PRIO as "internal wars." This choice excludes a number of civil wars that UCDP/PRIO classifies as "internationalized," yet there is no reason to do so given the conceptualization of civil war in their study. They also use two variables that code all country-years that follow a civil war, one corresponding to the COW civil war variable, the other to the UCDP/PRIO civil war variable.

Replicating their analysis after replacing these indicators with identically coded ones based on our data makes us more sanguine about the negative effects of civil wars on education. Results are shown in Table 4. Where Lai and Thyne find significant negative effects of civil war, our reanalysis always yields insignificant results. In addition, the effect of postconflict periods is either negative or null in the original article; using our data, we find that postcivil war periods actually see statistically significant increases in educational spending and enrollment.[18]

The studies replicated above used the ACD war list based on a cumulative death threshold or a 1,000 yearly death threshold but with the exclusion of internationalized conflicts. As we showed earlier with respect to trends in onset, the differences between our data and the ACD are smaller when the annual death threshold is used and internationalized conflicts are included, so we chose to replicate a study that meets these criteria: Colgan (2015). Colgan asks an interesting question: why are petro-states more prone to civil war while also being autocratic if civil wars create opportunities for regime transitions? He finds that civil war has a significant positive impact on the likelihood of autocratic regime transitions, but that oil income has no additional effect on the likelihood of regime transitions during civil conflict periods. Results using our data are very similar to Colgan's. As shown in Table 5, there are some differences in magnitude, but the sign and significance of estimated coefficients for the key explanatory variables are substantively the same. This is partly due to the fact that the key variable—the designation of a country as a petro-state—does not vary significantly over time, so results are less sensitive to differences in the coded timing of civil war onset. These results are also consistent with our earlier observation that the cumulative death criterion maximizes differences with other data sets. We conclude that the ACD's annual death threshold is preferable to its cumulative intensity coding, with the important qualification that researchers should not limit themselves to ACD's list of internal wars but instead also include internationalized civil wars.

## *Replications of Studies Using Civil War as a Dependent Variable—Continuity and Aggregation*

The differences observed in Figures 1 and 2 comparing coded war onsets in our data versus in the ACD could affect the substantive conclusions of studies of war onset

depending on which data set is used. We explore this further by replicating three studies where civil war onset is the dependent variable.

Buhaug, Cederman, and Gleditsch (2014) test hypotheses about the effect of ethnic economic and political inequality on the risk of civil war at the country level. They use new empirical measures of inequality that significantly improve on previous studies. They correlate these measures with a binary civil war variable based on the ACD, coded 1 the first year a conflict between the state and a domestic rival causes twenty-five deaths or more, and zero otherwise. A new onset is coded every time deaths rise to this level if in the preceding two years violence levels were lower. They also use Fearon and Laitin's (2003) coding of civil war as an alternative dependent variable to check the robustness of their results.

In Table 6, we replicate results shown in Tables 1 and 4 of their article and we also show results for the same models using our data to code civil war onset. We find that their results for horizontal economic inequality, the size of the largest group that is discriminated against, and groups' loss of political status (all positively associated with civil war onset) are robust to changes in the coding of civil war. This is probably because they aggregate dyadic conflict data to the country level (consistent with our recommendation) and also because their inequality measures are fairly constant over time, hence less sensitive to changes in the start and end dates of war. We do nonetheless find some intriguing differences with respect to the effect of power-sharing on conflict onset. While Buhaug, Cederman, and Gleditsch find no effect using either the ACD or Fearon and Laitin data, using our data we find that ethnic executive powersharing significantly *increases* the likelihood of war onset. The effect of ELF, which is positive using ACD data, also disappears using our data (or the Fearon and Laitin data).[19]

Koubi and Böhmelt (2014) revisit the debate on "greed versus grievance" in civil war (Collier and Hoeffler 2004) and argue that there is an interactive effect of low per capita GDP and ethnic political exclusion, such that war onset is more likely in richer countries with excluded ethnic groups. Civil war onsets from 1951 to 2005 are analyzed with the country-year as the unit of observation. In Table 7, we present results from a number of their models using the original specification, followed by estimates using our data on civil war onset. We find that the key explanatory variable—the interaction between GDP and exclusion—is no longer statistically significant using our data; and there is also no significant effect for several other key explanatory variables (share of excluded groups, ethnolinguistic fractionalization, and oil).

Basedau, Pfeiffer, and Vüllers (2016) test the effect of religious discrimination and religious calls for violence (using originally coded data on these variables) on civil war onset for 1990 to 2010, relying on the ACD as their source of data on wars. The unit of observation is the country-year, but onset appears to be coded based on ACD conflict ids, with new onset coded each year in which there are twenty-five battle-related deaths for a given ACD conflict id provided that conflict id experienced no conflict in the previous year.[20]

**Table 5.** Replication of Colgan (2015) with ACD (original) and SSW Data.

| | Interaction | | | Conflict Years | | | No Conflict Years | | |
|---|---|---|---|---|---|---|---|---|---|
| DV: Regime Transition | ACD (25+ Annual Deaths) Table II Model B | ACD (1000+ Annual Deaths) Appendix Model B | SSW Table II Model B | ACD (25+ Annual Deaths) Table II Model C | ACD (1000+ Annual Deaths) Appendix Model C | SSW Table II Model C | ACD (25+ Annual Deaths) Table II Model D | ACD (1000+ Annual Deaths) Appendix Model D | SSW Table II Model D |
| Petrostate | −0.794** | −0.911*** | −0.852** | −1.919*** | −2.958* | −1.799** | −0.783** | −0.954*** | −0.803** |
| | (0.365) | (0.328) | (0.332) | (0.666) | (1.625) | (0.780) | (0.367) | (0.343) | (0.344) |
| Domestic conflict | 1.118*** | 0.935*** | 0.709*** | | | | | | |
| | (0.207) | (0.252) | (0.204) | | | | | | |
| Conflict × Petrostate | −0.865 | −0.891 | −0.650 | | | | | | |
| | (0.604) | (0.822) | (0.688) | | | | | | |
| GDP per capita (log) | 0.326*** | 0.278** | 0.263** | 0.480** | 0.198 | 0.397 | 0.263* | 0.308** | 0.207* |
| | (0.111) | (0.108) | (0.103) | (0.232) | (0.492) | (0.277) | (0.135) | (0.125) | (0.125) |
| Economic growth | −0.0375*** | −0.0390*** | −0.0388*** | −0.0433*** | −0.0317 | −0.0456*** | −0.0313** | −0.0396*** | −0.0369*** |
| | (0.00963) | (0.00963) | (0.00947) | (0.0149) | (0.0250) | (0.0172) | (0.0141) | (0.0116) | (0.0127) |
| Previous transitions | 0.128** | 0.171*** | 0.183*** | −0.0929 | −0.355 | −0.138 | 0.313*** | 0.222*** | 0.304*** |
| | (0.0611) | (0.0581) | (0.0545) | (0.119) | (0.377) | (0.165) | (0.0648) | (0.0652) | (0.0642) |
| Muslim, %population | −0.208 | −0.116 | −0.0895 | −0.392 | 0.170 | −0.180 | −0.0541 | −0.0773 | −0.0857 |
| | (0.270) | (0.252) | (0.242) | (0.522) | (1.166) | (0.672) | (0.312) | (0.295) | (0.280) |
| Monarchy | −0.861** | −0.862** | −0.872** | −0.0453 | 2.122 | 0.152 | −1.172** | −1.063** | −0.908** |
| | (0.399) | (0.388) | (0.382) | (0.692) | (1.839) | (1.063) | (0.509) | (0.443) | (0.428) |
| Constant | −4.467*** | −4.103*** | −3.963*** | −2.133 | −0.488 | −1.357 | −4.659*** | −4.973*** | −4.235*** |
| | (0.882) | (0.864) | (0.826) | (1.801) | (3.756) | (2.080) | (1.107) | (1.056) | (1.051) |
| Time dummies | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Observations | 4,030 | 4,030 | 4,030 | 819 | 349 | 616 | 3,211 | 3,676 | 3,414 |

*Note:* Standard errors are given in parentheses.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

**Table 6.** Replication of Buhaug, Cederman, and Gleditsch (2014) with ACD (25+ Deaths/Year; Original), Fearon and Laitin, and SSW Data.

| | Table 1 | | Table 1 | | Table 3 | |
|---|---|---|---|---|---|---|
| | Model 1 | | Model 2 | | Model 4 | |
| | VI | | HI | | HI | |
| DV: Civil War Onset | ACD | SSW | ACD | SSW | FL | SSW |
| ELF | 1.148*** | 0.779 | 0.974** | 0.253 | 0.183 | 0.309 |
| | (0.424) | (0.525) | (0.428) | (0.483) | (0.562) | (0.482) |
| Gini income inequality | −0.00468 | −0.0179 | −0.00375 | −0.0130 | 0.00743 | −0.0134 |
| | (0.00954) | (0.0113) | (0.00994) | (0.0107) | (0.0126) | (0.0110) |
| Demographic size of largest discriminated group | | | 1.288*** | 2.097*** | 1.501*** | 1.901*** |
| | | | (0.346) | (0.359) | (0.473) | (0.342) |
| Positive horizontal inequality | | | −0.0447 | −0.318 | −0.179 | −0.173 |
| (mean per capita income of richest group/national GDP per capita) | | | (0.175) | (0.247) | (0.247) | (0.230) |
| Negative horizontal inequality | | | 0.321*** | 0.506*** | 0.526*** | 0.469*** |
| (national GDP per capita/mean per capita income of poorest group) | | | (0.119) | (0.130) | (0.148) | (0.131) |
| Power downgrade | | | 0.860*** | 1.132*** | 0.332 | 1.019*** |
| | | | (0.255) | (0.335) | (0.458) | (0.333) |
| Powersharing | | | −0.0287 | 0.501** | 0.317 | 0.430* |
| | | | (0.221) | (0.246) | (0.308) | (0.250) |
| Democracy | 0.176 | 0.264 | 0.350 | 0.488 | 0.423 | 0.652 |
| | (0.319) | (0.379) | (0.345) | (0.418) | (0.492) | (0.412) |
| Population | 0.249*** | 0.153* | 0.234*** | 0.150* | 0.207** | 0.124 |
| | (0.0693) | (0.0794) | (0.0792) | (0.0822) | (0.0841) | (0.0830) |
| GDP/capita | −0.382*** | −0.603*** | −0.432*** | −0.662*** | −0.580*** | −0.706*** |
| | (0.140) | (0.157) | (0.147) | (0.164) | (0.186) | (0.173) |
| Civil War lag | 0.161 | −0.138 | −0.0258 | −0.317 | −0.631* | −0.243 |
| | (0.279) | (0.312) | (0.298) | (0.305) | (0.344) | (0.323) |
| Constant | −5.968*** | −4.480*** | −6.311*** | −5.230*** | −6.808*** | −5.040*** |
| | (0.782) | (1.016) | (0.850) | (0.936) | (1.121) | (0.961) |
| Observations | 5,219 | 5,219 | 5,219 | 5,219 | 4,433 | 4,433 |

*Note:* Robust standard errors are given in parentheses.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

**Table 7.** Replication of Koubi and Böhmelt (2014) with ACD (25+ Deaths/Year; Original) and SSW Data.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Table II Model 1 | | Table II Model 2 | | Table II Model 3 | | Table II Model 4 | | Table II Model 5 | | Table II Model 6 | |
| | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW |
| Share of excluded groups | | | | | | | .00601 | .00378 | −0.177** | −0.180* | −0.200** | −0.142 |
| | | | | | | | (0.00525) | (0.00949) | (0.0778) | (0.102) | (0.0845) | (0.105) |
| GDP per capita (ln) | −0.167*** | −0.190*** | −0.197*** | −0.153*** | −0.197*** | −0.153*** | −0.211*** | −0.144*** | −0.195*** | −0.218*** | −0.237*** | −0.161*** |
| | (0.0437) | (0.0434) | (0.0522) | (0.0468) | (0.0522) | (0.0468) | (0.0584) | (0.0519) | (0.0512) | (0.0508) | (0.0607) | (0.0552) |
| Share of excluded groups × GDP per capita (ln) | | | | | | | | | .0247** | .0253* | .0280** | .0198 |
| | | | | | | | | | (0.0101) | (0.0130) | (0.0111) | (0.0137) |
| Oil | .342*** | .370*** | .250** | .236* | .250** | .236* | .248** | .207* | .311*** | .339*** | .247** | .204 |
| | (0.0988) | (0.113) | (0.115) | (0.120) | (0.115) | (0.120) | (0.123) | (0.124) | (0.107) | (0.115) | (0.123) | (0.125) |
| × Polity | .0195* | .0135 | .0224* | .0194 | .0224* | .0194 | .0264** | .0191 | .0243* | .0119 | .0252* | .0181 |
| | (0.0114) | (0.0120) | (0.0119) | (0.0125) | (0.0119) | (0.0125) | (0.0135) | (0.0138) | (0.0130) | (0.0134) | (0.0136) | (0.0136) |
| × Polity2 | −0.00407 | −0.00447 | −0.00461 | −0.00577** | −0.00461 | −0.00577** | −0.00477 | −0.00661** | −0.00445 | −0.00519 | −0.00503 | −0.00690** |
| | (0.00327) | (0.00290) | (0.00324) | (0.00290) | (0.00324) | (0.00290) | (0.00357) | (0.00321) | (0.00360) | (0.00321) | (0.00361) | (0.00324) |
| Population | .0585** | .0442* | .0680** | .0591* | .0680** | .0591* | .0502 | .0551* | .0527 | .0438 | .0487 | .0540* |
| | (0.0282) | (0.0259) | (0.0276) | (0.0307) | (0.0276) | (0.0307) | (0.0310) | (0.0321) | (0.0326) | (0.0295) | (0.0319) | (0.0328) |
| Ethnic fractionalization | .380*** | .271* | .488*** | .293* | .488*** | .293* | .484*** | .191 | .355** | .173 | .488*** | .191 |
| | (0.131) | (0.146) | (0.139) | (0.173) | (0.139) | (0.173) | (0.147) | (0.180) | (0.140) | (0.164) | (0.151) | (0.185) |
| Mountainous terrain | .0640*** | .0847*** | .0442* | .0839*** | .0442* | .0839*** | .0382 | .0775** | .0603*** | *.0746** | .0376 | .0778** |
| | (0.0215) | (0.0294) | (0.0250) | (0.0324) | (0.0250) | (0.0324) | (0.0279) | (0.0339) | (0.0232) | (0.0309) | (0.0287) | (0.0348) |
| Middle East | | | .310* | .533*** | .310* | .533*** | .226 | .636*** | | | .190 | .611*** |
| | | | (0.182) | (0.187) | (0.182) | (0.187) | (0.200) | (0.184) | | | (0.199) | (0.184) |
| North Africa | | | .207 | .251 | .207 | .251 | .154 | .422** | | | .149 | .417** |
| | | | (0.152) | (0.170) | (0.152) | (0.170) | (0.166) | (0.172) | | | (0.173) | (0.176) |
| Sub-Saharan Africa | | | −0.0540 | .234 | −0.0540 | .234 | −0.124 | .356** | | | −0.167 | .331* |
| | | | (0.145) | (0.192) | (0.145) | (0.192) | (0.144) | (0.180) | | | (0.145) | (0.180) |
| Latin America | | | .185 | .115 | .185 | .115 | .112 | .176 | | | .0931 | .162 |
| | | | (0.132) | (0.164) | (0.132) | (0.164) | (0.145) | (0.172) | | | (0.147) | (0.173) |
| East Asia | | | −0.0874 | .139 | −0.0874 | .139 | −0.0612 | .289* | | | −0.0919 | .269 |
| | | | (0.144) | (0.171) | (0.144) | (0.171) | (0.138) | (0.175) | | | (0.139) | (0.175) |
| West Asia | | | .0931 | .272 | .0931 | .272 | .0854 | .399** | | | .0599 | .382** |
| | | | (0.147) | (0.177) | (0.147) | (0.177) | (0.142) | (0.167) | | | (0.144) | (0.167) |
| Peace years cubic polynomial | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Constant | −1.292*** | −0.928** | −1.182** | −1.527** | −1.182** | −1.527** | −0.834 | −1.560*** | −0.973** | −0.602 | −0.589 | −1.395** |
| | (0.370) | (0.426) | (0.557) | (0.595) | (0.557) | (0.595) | (0.610) | (0.598) | (0.443) | (0.505) | (0.626) | (0.632) |
| Observations | 6,447 | 6,447 | 6,447 | 6,447 | 6,447 | 6,447 | 4,828 | 4,828 | 4,828 | 4,828 | 4,828 | 4,828 |

*Note:* Robust standard errors are given in parentheses.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

**Table 8.** Replication of Basedau, Pfeiffer, and Vüllers (2016) with ACD (25+ Deaths/Year; Original) and SSW Data.

| | Model 1 | | Model 2 | | Model 3 | | Model 4 | | Model 5 | | Model 6 | | Model 7 | | Model 8 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW | ACD | SSW |
| Religious fractionalization | -2.038*** | -1.276 | | | | | | | | | | | | | | |
| | (0.652) | (0.869) | | | | | | | | | | | | | | |
| Religious polarization | | | -1.225** | -0.986 | | | | | | | | | | | | |
| | | | (0.484) | (0.640) | | | | | | | | | | | | |
| Religious dominance | | | | | 0.942*** | 0.270 | | | | | | | | | | |
| | | | | | (0.342) | (0.361) | | | | | | | | | | |
| Ethnic overlap | | | | | | | 0.622** | 0.377 | | | | | | | | |
| | | | | | | | (0.245) | (0.372) | | | | | | | | |
| Economic overlap | | | | | | | | | 0.107 | 0.431 | | | | | | |
| | | | | | | | | | (0.251) | (0.320) | | | | | | |
| Regional overlap | | | | | | | | | | | 0.233 | 0.299 | | | | |
| | | | | | | | | | | | (0.278) | (0.421) | | | | |
| Grievances over discrimination | | | | | | | | | | | | | 0.159 | 0.645** | | |
| | | | | | | | | | | | | | (0.247) | (0.257) | | |
| Religious Calls for Violence | | | | | | | | | | | | | | | 0.289 | -0.0183 |
| | | | | | | | | | | | | | | | (0.262) | (0.491) |
| Population | 0.330*** | 0.203* | 0.314*** | 0.196* | 0.319*** | 0.202* | 0.272*** | 0.193* | 0.298*** | 0.168* | 0.297*** | 0.195* | 0.296*** | 0.125 | 0.289*** | 0.162 |
| | (0.0923) | (0.110) | (0.0903) | (0.108) | (0.0852) | (0.105) | (0.0813) | (0.101) | (0.0901) | (0.0951) | (0.0844) | (0.103) | (0.0939) | (0.0904) | (0.0892) | (0.100) |
| GDP growth | 0.0181 | -0.00962 | 0.0166 | -0.00977 | 0.0182 | -0.0102 | 0.0107 | -0.0119 | 0.0139 | -0.0115 | 0.0129 | -0.0125 | 0.0118 | -0.0132 | 0.0131 | -0.00980 |
| | (0.0152) | (0.0267) | (0.0147) | (0.0267) | (0.0150) | (0.0256) | (0.0129) | (0.0246) | (0.0136) | (0.0260) | (0.0139) | (0.0260) | (0.0140) | (0.0235) | (0.0136) | (0.0267) |
| GDP/capita | -0.641*** | -0.529*** | -0.629*** | -0.541*** | -0.575*** | -0.472*** | -0.514*** | -0.434** | -0.500*** | -0.469** | -0.484*** | -0.419** | -0.495*** | -0.432** | -0.484*** | -0.349** |
| | (0.146) | (0.181) | (0.146) | (0.187) | (0.135) | (0.170) | (0.128) | (0.176) | (0.133) | (0.190) | (0.134) | (0.183) | (0.130) | (0.171) | (0.126) | (0.173) |
| Durability | 0.000465 | -0.00448 | -0.00136 | -0.00502 | -0.00494 | -0.00758 | -0.00819 | -0.00934 | -0.00680 | -0.0101 | -0.00648 | -0.00884 | -0.00667 | -0.00968 | -0.00639 | -0.00737 |
| | (0.00884) | (0.0144) | (0.00904) | (0.0144) | (0.00820) | (0.0146) | (0.00904) | (0.0152) | (0.00899) | (0.0145) | (0.00919) | (0.0150) | (0.00934) | (0.0141) | (0.00902) | (0.0148) |
| Polity | 0.00944 | -0.0149 | 0.0111 | -0.0150 | 0.00297 | -0.0162 | 0.0210 | -0.0104 | 0.0115 | -0.0190 | 0.0126 | -0.0155 | 0.0116 | -0.0146 | 0.0118 | -0.0145 |
| | (0.0218) | (0.0228) | (0.0229) | (0.0229) | (0.0198) | (0.0226) | (0.0226) | (0.0241) | (0.0219) | (0.0223) | (0.0226) | (0.0223) | (0.0233) | (0.0228) | (0.0221) | (0.0238) |
| Terrain | -0.00187 | -0.0568 | 0.00478 | -0.0565 | 0.0304 | -0.0302 | 0.0735 | -0.00863 | 0.0492 | -0.0102 | 0.0604 | -0.00366 | 0.0350 | -0.0290 | 0.0428 | -0.0483 |
| | (0.0887) | (0.123) | (0.0891) | (0.124) | (0.0903) | (0.120) | (0.0878) | (0.114) | (0.0870) | (0.121) | (0.0888) | (0.109) | (0.0876) | (0.111) | (0.0838) | (0.117) |
| Oil exports | 0.00110** | 0.000421 | 0.00111** | 0.000380 | 0.00107** | 0.000383 | 0.00126** | 0.000385 | 0.00113** | 0.000529 | 0.00113** | 0.000394 | 0.00112** | 0.000433 | 0.00103** | -0.000138 |
| | (0.000540) | (0.000931) | (0.000529) | (0.000925) | (0.000517) | (0.000972) | (0.000541) | (0.00107) | (0.000512) | (0.00100) | (0.000516) | (0.00101) | (0.000518) | (0.000997) | (0.000502) | (0.00114) |
| Years since onset | -0.106*** | -0.0774** | -0.109*** | -0.0761** | -0.107*** | -0.0790** | -0.112*** | -0.0790** | -0.118*** | -0.0769** | -0.117*** | -0.0793** | -0.118*** | -0.0862** | -0.117*** | -0.175*** |
| | (0.0267) | (0.0347) | (0.0272) | (0.0347) | (0.0275) | (0.0348) | (0.0269) | (0.0345) | (0.0291) | (0.0361) | (0.0278) | (0.0346) | (0.0284) | (0.0345) | (0.0286) | (0.0468) |
| Constant | -3.136** | -2.719 | -2.920* | -2.446 | -4.705*** | -3.679* | -3.946*** | -3.800** | -4.056** | -3.070 | -4.314*** | -3.942** | -4.060** | -2.651 | -4.028** | -3.205 |
| | (1.579) | (2.228) | (1.592) | (2.263) | (1.649) | (2.162) | (1.515) | (1.904) | (1.680) | (1.975) | (1.561) | (1.999) | (1.690) | (1.678) | (1.608) | (2.038) |
| Observations | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,103 | 2,094 | 2,094 | 2,103 | 2,103 |

*Note:* Robust standard errors are given in parentheses.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

Using their preferred list of civil war onsets, the authors find that religious fractionalization, polarization, dominance, and whether religious identities coincide "at least partially" with other identity categories (ethnic, regional, or economic) are all statistically significant COW onset. All of these results disappear if we use our civil war onset data to replace their conflict onset measure (see Table 8). The study also reports no significant effect of religious grievances over discrimination if all types of conflict are considered (model 7) but does find religious discrimination to be a significant correlate of interreligious or theological conflict. The authors point to these results as motivation for distinguishing between conflict types. However, using our data, we find that there is, in fact, a significant positive effect of religious grievances for armed conflict generally (model 7). Religious grievances are more significant than the authors initially thought, but the data may not support the notion that the effect of religious grievances varies according to the type of conflict that the authors identify. Finally, the country's dependence on oil exports, which is significant and positive in their data, is no longer significant using our data.

To sum up, we have replicated several influential studies of civil war and found that their results frequently depend on how civil war is coded. Many of these studies are exemplary in presenting new data on important correlates of violent conflict. But they do not justify their choice of data on civil war and they fail to consider whether this matters. This is hardly a lacuna; the authors' substantive conclusions are in some cases annulled, in others reversed entirely, when we code civil war consistent with its widely accepted core trait—sovereignty rupture. And, as summarized in Table 8, these studies' descriptions of civil war and its potential effects correspond to the sovereignty rupture concept that our coding criteria capture.

## Conclusion: The Intertwined Nature of Theory, Data Selection, and Coding Rules

The inferences we make about the causes and consequences of civil war are based on the analysis of quantitative data that often differ greatly across data sets. Key concepts—such as civil war, victory, or powersharing—are often understood and measured differently across cases. This pluralism is not inherently bad, but it makes it difficult to produce cumulative knowledge: analyses produce wildly different conclusions depending on which data are used.

The first sensitivity analysis of empirical results on internal armed conflict (Hegre and Sambanis 2006) showed that there are important differences in the correlates of civil war as compared to minor armed conflicts. Since then, rather than see researchers drawing more careful distinctions between wars and lower-level conflict, we have seen the opposite. Using the UCDP/PRIO Armed Conflict Database, which codes annual levels of battle deaths, researchers increasingly refer to any conflict that causes twenty-five battle deaths per year as a civil war.[21] And, studies increasingly disaggregate state–group interactions over time and use data

from lower-level armed conflicts to test theories about the causes and consequences of civil war.

This article argues that civil war is characterized by the concept of sovereignty rupture, and as such, it affects an entire polity. We have provided new data that are coded consistent with this concept of civil war and compared our data to the UCDP/PRIO list of civil wars, which is the most widely used source in the literature. Reviewing several studies and replicating their analyses reveals that researchers often use off-the-shelf measures that they assume capture civil war without carefully considering whether the coding of the data corresponds to this theoretical concept given the question being studied.

To improve this situation, we suggest that all studies justify their selection of war data in a clear, transparent manner. If a twenty-five death threshold is used to code war onset, why is this is a reasonable decision given the question being addressed? If a cumulative threshold is preferred over an annual death threshold, why does this better capture the phenomenon of civil war as understood in the underlying theory? If a war is broken down into several dyadic conflicts, why is this appropriate given the nature of these conflicts and the interdependencies among them?

Our data should serve as an alternative source to check the robustness of empirical findings in studies of civil war. Key substantive conclusions of published articles should not depend on seemingly small differences in the technical criteria used to code conflict episodes. Selecting a number of studies at random to replicate, we found stark differences in the conclusions that these studies can support when we use our data instead of the authors' preferred operationalization of civil war (summarized in Table 9). These studies' empirical results on the effects of powersharing, education, religious fragmentation, oil dependence, or other variables that we have explored in our replication analysis could have been the basis for developing policy prescriptions for conflict management. Such results should therefore not depend on small differences in the coding of civil war; or if they do, researchers should be transparent about these differences and justify their selection of data.

Our data set, coding conflicts from 1945 to 2016, has the advantage of being based on a detailed coding rule and it is supported by more than 500 pages of coding notes. The data set improves on competing sources by being less reliant on an overly strict application of the fatalities threshold in identifying distinct episodes of conflict; and it explains which dyadic conflicts should be aggregated up into a single instance of sovereignty rupture and which should be separated into new wars. The key here is that disaggregation should reflect sovereignty rupture, not the endogenous creation of actors involved in conflict.

Careful matching of data to the research question at hand is particularly important for studies that focus on war duration and recurrence as well as any study of the effects of civil war. Errors in the coding of war onset or termination will introduce significant bias in those studies, as evidenced by the replication results we have presented.

**Table 9.** Summary of Replications.[a]

| | Civil War Variable (CWV) | | | | | Using SSW Data | |
|---|---|---|---|---|---|---|---|
| Study | Conceptual Description | Operationalization | Type | Main Finding(s) | Operationalized CWV versus Sovereignty Rupture | Main Findings Hold? | Other Substantive Differences? |
| Graham, Miller, and Strøm (2017) | "Civil war"; large-scale, organized political violenceb | 1,000+ deaths, cumulativec | Explanatory | Constraining powersharing (PS) affects democratic survival; in post–Civil War countries, inclusive PS promotes democratic survival, dispersive PS does not | Includes low-level armed conflict and misses sovereignty shifts | No | No |
| Lai and Thyne (2007) | "Civil war"; large-scale, organized political violenced | 1,000+ deaths/year from COW; 1,000+ deaths/year from ACD, but "internationalized" conflicts excluded | Explanatory | Civil war negatively affects school expenditures and enrollment | Countries may cycle in and out of conflict due to inflexible deaths threshold; misses sovereignty shifts; "internationalized" conflicts not included | No | Yes[e] |
| Colgan (2015) | "Civil war"; large-scale, organized political violence; "violent domestic conflict," "violent rebellion"f | 25+ deaths/year; 1,000+ deaths/year checked in appendix | Explanatory | Civil war increases the likelihood of autocratic regime transitions, but that oil income does not affect the likelihood of regime transitions during periods of civil conflict | Includes low-level armed conflict; misses sovereignty shifts; countries may cycle in and out of low-level or large-scale conflict due to artificial deaths threshold | Yes | No |
| Buhaug, Cederman, and Gleditsch (2014) (BCG) | "Civil war"; large-scale, organized political violence | 25+ deaths/year or 1000+ cumulative deathsg | Dependent | The size of the largest group that is discriminated against, economic inequality, and loss of political status increase the likelihood of civil war onset, while powersharing does not affect it | Includes low-level armed conflict; misses sovereignty shifts; countries may cycle in and out of low-level armed conflict | Yes | Yes[h] |
| Koubi and Böhmelt (2014) | "Civil conflict"; "domestic violence [*sic*]," "revolt"; "civil war," large-scale conflicti | 25+ deaths/year; new onset only if no war in previous nine yearsj | Dependent | The interaction between GDP per capita and ethnic political exclusion increases the likelihood of civil war onset | Includes low-level armed conflict; misses sovereignty shifts; countries less likely to cycle in/out of conflict due to artificial deaths threshold than would otherwise be the case but could miss new onsets | No | Yes[k] |

*(continued)*

**Table 9.** (continued)

| Study | Civil War Variable (CWV) | | | Main Finding(s) | Operationalized CWV versus Sovereignty Rupture | Using SSW Data | |
|---|---|---|---|---|---|---|---|
| | Conceptual Description | Operationalization | Type | | | Main Findings Hold? | Other Substantive Differences? |
| Basedau, Pfeiffer, and Vullers (2016) | "Armed conflict"; "organized violence"; civil war; large-scale, organized political violencel | 25+ deaths/year; new onsets possible in a country with ongoing war because codes onset based on conflict id | Dependent | Religious fractionalization, polarization, dominance, and overlap between religious identities and other identity categories (ethnic, regional, or economic) increase the likelihood of civil war onset | Includes low-level armed conflict; misses sovereignty shifts; countries may cycle in and out of low-level conflict; may code multiple onsets within a single instance of sovereignty rupture | No | Yes[m] |

[a]Here, deaths refers to battle-related deaths.
[b]For example, Graham, Miller, and Strøm (2017) note that "In societies that have recently undergone violent civil conflict, mutual security is of pre-eminent concern" (p. 687). They also note, "Societies that have recently undergone civil conflict often have special and more severe institutional needs. Intense hostility and insecurity, coupled with high uncertainty over the future, encourage the resumption of violence, and political leaders often have easy access to arms and experienced combatants" (p. 694).
[c]As a robustness check, the authors also use a definition of civil war that lowers the fatality threshold for conflict to twenty-five battle-related deaths per year.
[d]For example, Lai and Thyne (2007) explain a difference in their results when using a civil war measure based on 1000+ battle-related deaths per year versus 25+ battle-related deaths per year as follows: "Only the Uppsala/PRIO civil war measure was significant. The lack of significance for lower-level civil conflicts is unsurprising as only higher-level conflicts are likely to inflict the amount of damage that would retard a state's educational system" (p. 282).
[e]In the results reported in Lai and Thyne (2007), the effect of postconflict periods is either negative or null. In our Table 4, columns 5 and 7, when we substitute their original postwar dummy variable for the dynamic one used in their Table II, we find a positive effect on enrollment but not spending, when using ACD data. Using the SSW Data, we find that statistically significant increases in educational spending and enrollment are associated with post–civil war.
[f]Colgan uses the subtitle "domestic conflict" in his literature review, but the article text discusses "civil war onset" and uses examples of large-scale, organized political violence. To an extent, he prevaricates regarding whether he intends to study civil war as sovereignty rupture. For most of the article, Colgan use the term "civil war" (e.g., p.10), cites studies of civil wars and uses civil wars as examples, but when he turns to the data analysis, he switches to implying that any level of political violence might be of interest.
[g]The later in a robustness check that uses Fearon and Laitin's (2003) definition of civil war.
[h]Buhaug, Cederman, and Gleditsch find no effect of powersharing on conflict onset, but using our data, we find that ethnic executive powersharing has a statistically significant positive effect on the likelihood of onset.
[i]Koubi and Böhmelt's article is in dialogue with the literature on civil wars. Its references to examples of "armed conflict" include "Nepal, northeast India, Nigeria, Sierra Leone, [and] Indonesia" (p. 23).
[j]Koubi and Böhmelt appear not to have coded the data in accordance with their nine-year criterion, however. For example, in the Western Hemisphere, they code onsets in Argentina in 1955 and 1963, Bolivia in 1946 and 1952, Cuba in 1953 and 1961, Guatemala in 1949 and 1954, El Salvador in 1972 and 1979, Mexico in 1994 and 1996, and Paraguay in 1947 and 1954.
[k]We find no effect of share of excluded groups, ethnolinguistic fractionalization, and oil on onset when using our data.
[l]Basedau, Pfeiffer, and Vullers (2016) mention as examples of the phenomenon that their article analyses the following cases of civil war: Sri Lanka, Northern Ireland, Syria, Sudan, Croatia, Côte d'Ivoire, Philippines, Algeria, and Azerbaijan. They also describe it as "violence conflict occurring between state security forces and nonstate actors. For the latter, it is a significant challenge to successfully organize rebellion against the state. Potential rebels need to overcome the collective action problems that are connected to the necessity of having (a) the opportunity to launch collective military action and (b) enough people sufficiently motivated to join a rebel group and enact violence (Collier and Hoeffler 2004)" (p. 231).
[m]Using our data, we find that religious grievances about discrimination increase the likelihood of onset for all civil wars, while Basedau, Pfeiffer, and Vüllers find such an effect for "theological armed conflict" onset. Using our data, we also find that a country's dependence on oil exports has no effect on the onset of conflict, while Basedau, Pfeiffer, and Vüllers found such an effect.

Moreover, forecasting future civil wars will similarly likely be extremely sensitive to the list of civil war used. If an arbitrarily high (or low) number of war onsets if coded based on a strict application of a fatalities threshold, then the training set that will be used to calibrate a forecasting model will generate very different out-of-sample predictions as compared to a model calibrated on a differently coded set of wars.

Over the past two-and-half decades, social scientific research on civil war has become a vibrant field. Quantitative studies have played an important role in its development. Increasingly, scholars employ microlevel research designs, but macrolevel studies of the causes and consequences of civil war have been important not only in their own right but also in how they inform our theories about human choices during conflict. An important step in the field's maturation has been the recognition of the limitations inherent to all of the research methods and designs available to scholars; thus the importance of pluralism. But progress must not be impeded by facets of research that are within our control—the careful selection and use of data. Careful matching of data to theory will help quantitative, macrolevel research on civil war to achieve its potential—the cumulative production of knowledge.

## Acknowledgments

We thank the editor of the *Journal of Conflict Resolution* and two anonymous referees for helpful guidance and comments. We owe special thanks to Christopher Blair for superb research assistance with the replication studies and for helping to extend the coding of wars to 2016, Marc Opper for painstaking coding of the war list through 2012 and attentive revisiting of the coding of war in China after 1949, and Joan Ricart-Huguet for contributing to the first extension of our data set in 2013.

## Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

## Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

## Supplemental Material

Supplemental material for this article is available online.

## Notes

1. Cederman and Gleditsch (2009) discuss the move toward disaggregation; and Cederman and Vogt (2017) discuss civil war trends drawing on the logic of civil war as a dyadic conflict.

2. Who are the relevant "challengers" or "actors" is another question that is often left unaddressed in the literature. Should units of analysis be ethnic groups, rebel groups, political movements, or parties? This question becomes relevant in disaggregated studies.
3. The Peruvian insurgency in the 1980s and 1990s is a good example. The Peruvian Communist Party, commonly known as the Shining Path (*Sendero Luminoso*, SL), began anti-government activity immediately before the 1980 presidential election. SL fought the government, as did the Túpac Amaru Revolutionary Movement (*Movimiento Revolucionario Túpac Amaru*, MRTA). The dyadic version of the ACD codes two conflicts—the state versus SL; and the state versus the MRTA. But there were also multiple actors fighting the rebels: the Peruvian Army, Marines, regular police forces, special counter-guerrilla police, civil defense groups, and even right-wing vigilante groups. Many of these groups maintained significant autonomy, so we could identify multiple anti-rebel actors. Yet the government is assumed to be a single actor (see the supplement for more discussion of this case). An important contribution that could help address this issue is Carey, Mitchell, and Lowe's (2013) data set on state-backed militias.
4. This is typically done by researchers using the ACD (see, e.g., Cederman et al. 2015; Gleditsch et al. 2002; Themnér and Wallensteen 2014). Published articles are rarely clear about the distinction between any armed conflict and civil war, though Hegre and Sambanis (2006) in the first sensitivity analysis of empirical results in this literature established that there are significant differences between the correlates of civil war, understood as large-scale armed conflict, and minor armed conflict (twenty-five battle deaths criterion).
5. The data set allows researchers to choose the number of years of violence below the twenty-five deaths threshold that are required to code a new onset. These intervals vary widely from one year (Basedau, Pfeiffer, and Vüllers 2016) or two (Cederman et al. 2015) to nine (Koubi and Böhmelt 2014). There is usually no theoretical justification for selecting an interval of specific length, though longer ones are clearly more consistent with the idea of a new onset.
6. For this reason, authors like Fearon and Laitin (2003) and Sambanis (2004) set a casualty threshold for the course of the entire war and a less strict one for a yearlong (Fearon and Laitin) or multiyear period (Sambanis).
7. Here, we follow Krasner's (1988) definition of sovereignty of "the assertion of final authority within a given territory" (p. 86). This also corresponds to the concept of "empirical statehood" (Jackson and Rosberg 1982; Jackson 1990, 21). See also Krasner's (1999, 4) definition of "domestic sovereignty": "the formal organization of political authority within the state and the ability of public authorities to exercise effective control within the borders of their own polity."
8. Indeed, sovereignty rupture anchors nearly all social scientific definitions of civil war. See, for example, Licklider (1993) and the discussion in Eckstein (1965) and Rosenau (1964), although the latter two authors prefer the term "internal war." Across a wide swathe of history, diverse civilizations have understood civil war similarly (e.g., the concept of *stasis* in ancient Greece, the Roman concept of *bellum civile*, and *harb ahliyya* in Arabic; see also Armitage 2017).

9. The Mapping Militant Organizations project at Stanford University (Crenshaw 2015) lists the following groups as active from 2006 to 2007: Hamas Iraq, 1920s Revolutionary Brigades, Mujahideen Army, Islamic Army in Iraq, Ansar al-Sunna, Ansar al-Islam, al-Qaeda in Iraq, Jaysh Rijal al-Tariqa al-Nashqabandi, Fatah al-Islam, Kataib Hezbollah, Asa'ib Ahl al-Haqq, Promised Day Brigades, the Mahdi Army, the militia of the Supreme Council for Islamic Revolution in Iraq, and the Badr Brigade (militia of the Islamic Supreme Council of Iraq).
10. Ridolfo (2004), for example, lists twenty-seven armed groups. International Crisis Group (2006) listed thirteen insurgent groups (not taking into account Shia militias), and "As of mid-December 2005," had catalogued "some 50 different brigades claiming military deeds under the banner of one major group or the other" (ICG 2006, 1). See also descriptions in Human Rights Watch (2005).
11. The ACD codes four factions on the anti-government side in the Iraqi civil war: Al-Mahdi Army, Ansar al-Islam, ISI, and RJF. RJF is actually a coalition of three groups: the Islamic Army in Iraq, the Mujahideen Army, and Ansar al-Sunna.
12. See Du Runsheng (1996, 309-13) and Luo Pinghan (2005, 304-18).
13. Dikötter (2013) details these campaigns. He estimates deaths resulting from them at five million (see p. xiii).
14. For example, in Laos, we code three wars, but if we did not use victories or settlements to mark the end of a war and the start of a new one, we would instead code a single war from 1960 to 1979. In Chad, this coding rule leads us to code six episodes of war since the country's independence in 1965. By contrast, Fearon and Laitin code 2 wars through 1999 versus five in our data, and the ACD codes 1 through 2016 (v17.2; ACD codes a second conflict in 2015 involving the Islamic State, but it does not reach the 1,000 death threshold and is coded as ending in that year).
15. See, for example, Luttwak (1999) and Toft (2010).
16. We revise eighteen observations from Sambanis (2004). This includes cases of wars that are split into multiple wars, one case that is eliminated, and several cases with slightly recoded dates (month or year of onset). We also add six new wars: the potentially ambiguous Oromo and Ogaden wars in Ethiopia starting in the late 1990s; and Chad 1998 to 2003, China 1949 to 1953, Laos 1976 to 1979, and Romania 1989.
17. As mentioned in Table 1, ACD codes new conflicts over new incompatibilities in all countries where IS is active, leading them to code several new war onsets from 2014 to 2016. However, there is a clear "lineage" between IS and groups that were previously active in the same countries (e.g., AQIM and IS in Algeria) and the same basis for sovereignty rupture—a contest over the state—so by our rules, these conflicts should be considered a single incidence of sovereignty rupture. See Supplemental Material for a more detailed discussion of coding conflicts that involve IS.
18. In our Table 4, columns 5 and 7, we report a replication of their results using ACD in which we substitute their original postwar dummy variable for the dynamic one used in Table II of their article. This yields a statistically significant increase in enrollment, but not spending. Using the SSW Data, we find consistent results—a statistically significant increase across enrollment and spending.

19. In sensitivity tests reported in their article, Buhaug, Cederman, and Gleditsch (2014) note this change in the effect of ELF when data from Fearon and Laitin are used (p. 428).
20. Angola is an instructive example to illustrate how these coding rules operate. Basedau, Pfeiffer, and Vüllers code new conflict onsets in 1991, 1994, 1996, 1998, 2002, 2004, 2007, and 2009. The ACD (v17.2) lists an ongoing conflict between the government and UNITA from before 1990 through 1995. The new onsets up until 1996 are the result of the Cabinda conflict rising above and falling below the twenty-five battle-death threshold. The new onset in 1998 is due to renewed conflict between UNITA and the government starting in that year, the new onset in 2002 is due to a resumption of the Cabinda conflict despite ongoing conflict between the government and UNITA, and the new onsets in 2004, 2007, and 2009 are due to the Cabinda conflict against rising above and falling below the twenty-five battle-death threshold.
21. For example, of the fourteen studies of civil war and armed conflict published by the *Journal of Conflict Resolution* between January 1 and August 2, 2018 that used cross-country data analysis (includes publication via "online first," excludes articles the primary function of which was to present a new data set), ten used a twenty-five yearly battle-related death threshold (or lower) and four used the UCDP/PRIO ACD or a data set built on it but did not explain their criteria for what constituted a civil war. The former are Gleditsch et al. (2018), Fisk (2018), Bohnet, Cottier, and Hug (2018), Prorok (2018), Otto (2018), Conrad et al. (2019), Maekawa (2019), Wiegand and Keels (2019), Asal et al. (2018), and Kim and Hong (2019). The latter are Blankenship (2018), Kim (2018), Lee (2018), and Roy (2018).

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