ANALYTICAL ESSAY

Integrating the Quantitative Research on the Onset and Incidence of Violent Intrastate Conflicts

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Quantitative research into the causes of violent intrastate conflicts has recently shifted away from classical country-year-level regression analyses. When taking steps in new directions, researchers should be mindful of the extent and quality, and indeed of the limitations, of the knowledge accumulated by the scholarly endeavors in the booming period between 2000 and 2015. This article traces trends and patterns regarding the use of explanatory variables and datasets in ninety-four individual studies. It synthesizes findings with regard to 107 explanatory concepts. Drawing on the sign test, the analysis identifies a set of consensus variables likely to determine the onset and incidence of violent intrastate conflict. These factors capture robust covariations and lend themselves as elements of a "standard model specification." Turning to causal mechanisms, the article discusses why variables that turn out to be significant in statistical analyses should have any effect. This is completed by a substantial discussion of the remaining theoretical problems and of methodological prospects that promise paths for future research.

Keywords: conflict onset and incidence, intrastate violence, research synthesis

Introduction

Country-year level regression analyses are being increasingly replaced by spatiotemporally more disaggregated forms of inquiry (Gleditsch, Metternich, and Ruggeri 2014). While this trend was initiated by the opportunity of more precise data, it was also motivated by a certain disenchantment with findings concerning the causes of violent intrastate conflicts (Schrodt 2014). Even after several decades of research and despite the increasing sophistication of methods, we have to acknowledge that we still do not know very much. This impression was reinforced with every systematic literature review published over the years (Sambanis 2004b; Hegre and

Sambanis 2006; Dixon 2009). While the number of variables considered runs in the hundreds, researchers usually turn to a limited set of core variables. In quantitative research, the standard approach has been multiple regression analysis, where the assessment of the explanatory power of a variable is based on partial regression coefficients. Typically, most predictors function as controls—to the extent that Dixon (2009, 731) speaks of a "laundry list of control variables" and Achen (2005, 327) of "garbage can regressions."[1] The fact that the "usual suspects" of independent variables constantly reoccur is a sign of path dependencies in conflict research.

Testing the robustness of findings across multiple studies, this article supplies researchers with a critical inspection of the accumulated knowledge of quantitative conflict research. It seeks to help researchers set priorities: First, as our synthesis shows which of the well-trodden paths led to robust findings regarding the onset and incidence of violent intrastate conflict and which turned out to be a dead end, we are able to recommend a "standard model specification" which can be adopted when setting out for the new shores of more disaggregated studies. Second, beyond these "consensus determinants," we are able to identify variables that have been analyzed frequently but with inconclusive results, making them particularly worthwhile for redoubled research efforts. We certainly do not contend that variables that are not "consensus determinants" are irrelevant. Conflict science has simply not reached a consensus on them. Future research might produce consistent findings for variables that have been underanalyzed so far and/or for those that have featured prominently without strong results.

To our knowledge, Dixon (2009) presented the latest large-scale aggregation of the findings of quantitative conflict research. His ten-year-old review surveyed the country-year paradigm during its heyday rather than at its demise. The present article presents an updated and extended synthesis of quantitative research on conflict onset and incidence between 2000 and 2015. In comparison to Dixon (2009), we review twice as many articles (ninety-four versus forty-seven). Furthermore, while Dixon focused on the onset and recurrence of violent intrastate conflicts, we include their onset and incidence.[2]

We also take a different methodological approach. Instead of counting statistically significant findings in the original studies, we synthesize results based on the signs of coefficients (Borenstein et al. 2009). Since our approach leaves aside statistical significance as an evaluation criterion and includes all variables rather than only those of theoretical interest in the original studies, it mitigates the effects of p-value "hacking" and selective reporting. Under the condition that a variable has been tested often enough, we are able to identify which variables are robustly related to conflict across different research designs.

The remainder of this article is organized as follows. We first study the general trends and patterns regarding dependent and independent variables as well as datasets over the course of sixteen years. Turning to the method of the sign test, we devise a simple procedure of assessing the cumulative level of knowledge regarding the possible causes of the onset and incidence of violent intrastate conflict and present sets of potential consensus determinants. This involves an overview of the theoretical arguments brought forward and the justifications for including these variables, respectively. The article then summarizes the state of quantitative conflict research after a decade and a half of predominantly country-year level analysis. It concludes with a substantial discussion of remaining theoretical problems and methodological prospects that represent promising paths for future research.

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[1] As Clarke (2005) argues, the inclusion of control variables to address omitted variable bias might even increase the bias of estimated coefficients.

[2] Including other outcomes such as conflict recurrence or duration was not feasible due to the low number of studies analyzing these phenomena.

Trends and Patterns in Conflict Research

This review focuses on quantitative studies exploring the causes of violent intrastate conflicts. It reviews studies that investigate at least fifty cases having the onset or incidence of violent intrastate conflict since 1945 or later as their dependent variables. Intrastate conflicts include disputes between governments and rebel groups as well as "subnational" or "nonstate" conflicts among nonstate actors (Gleditsch et al. 2002; Sundberg, Eck, and Kreutz 2012; Trinn and Wencker 2018). Conflict incidence is the existence of a conflict in a given temporal and/or spatial unit (usually year and country). Incidence is typically measured based on battle-related deaths or with reference to the occurrence of a conflict event. Onset is simply the first incidence in such a sequence. These operational approaches do not take into account the deeper question of the conceptual difference between conflict as a progressive stream of actions, that is, a process that continuously (re-)creates the conflict through the sequence of events, on the one hand, and conflict as a persistent set of constellations in between and possibly even without concrete interactions, on the other. Between each action, the conflict can be said to continue to exist in a structural sense (Trinn and Wencker 2018).

Unlike Cederman and Vogt (2017), who focus their literature review on civil wars, we also include other forms of political violence, such as riots, protests, revolutions, one-sided violence, and terrorism. These forms often reflect specific strategies of intrastate conflict and are frequently found in full-fledged civil wars. Moreover, "war" is, strictly speaking, a specific result of an escalation dynamic and not substantially distinct from other forms of intrastate violence. Further analysis has shown that conflict intensities are often "scale-invariant," which means that large events are simply larger variants of smaller events (Clauset, Young, and Gleditsch 2007). There is thus no *fundamental* difference between, say, riots and civil wars. Accordingly, we argue that the various conflict types and levels share enough common characteristics to understand them as manifestations of a single concept (Trinn and Wencker 2018). Nevertheless, we differentiate between different severity levels—for instance, between conflicts with more than a thousand and less than twenty-five fatalities.[3] Interaction terms and their components (but not the other variables in the model containing the interaction) were excluded from the main analysis as their conditional logic warrants a specific interpretation, unlike the other variables. We will return later to the interaction terms contained in the articles under review.

The reviewed studies have been selected from academic journals on conflict studies that scored at least a rank of forty-five on the list of high-impact political science journals provided by Giles and Garand (2007).[4] Ninety-four articles published between 2000 and 2015, which focus either on conflict onset or incidence or both, were drawn from these journals. The selected articles are listed in the supplementary file.

Our overview presents a systematic research synthesis but not a meta-analysis in the strict sense. This is because the combined studies largely pertain to similar populations (the set of intrastate violent conflicts in contemporary history), using the same or very similar datasets. For this reason, the individual regression models in the studies under review are mostly not independent from each other. To mitigate this problem, we aggregate similar regression models and count them as individual observations. Accordingly, the basic unit of our synthesis is the "analytical unit" (AU) specified by three criteria: (a) the reviewed article, (b) the disaggregated outcome (onset or incidence, differentiated by severity), and (c) the conflict dataset

[3] The complete coding scheme is included in the supplementary file.

[4] The following journals were selected (in ranked order): International Organization, American Political Science Review, American Journal of Political Science, International Security, European Journal of International Relations, Journal of Conflict Resolution, Journal of Peace Research, International Interactions, Conflict Management and Peace Science, Security Studies. American Sociological Review was added to the sample.
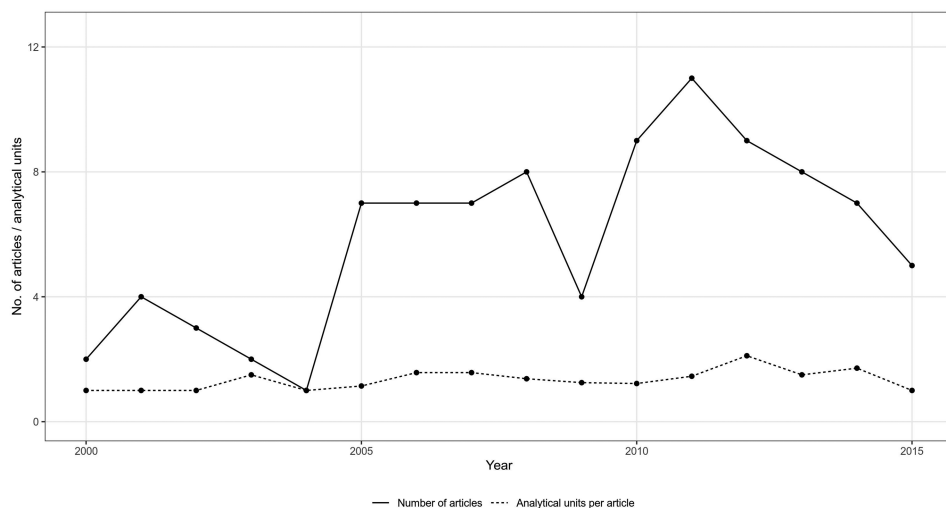
Figure 1. Total number of articles in the sample and share of analytical units per article[12]

used. If, for instance, multiple regression models in a single article vary solely with regard to the independent variables, they are subsumed under one AU.

The number of articles published per year varied in the period under review. It rose from two in 2000 to eleven in 2011 but dropped to five in 2015. The relatively low number of recent articles in our sample is partly explained by the advent of studies on conflict intensities, an outcome that we did not include in this overview. The selected studies contain a total of 134 AU. On average, there were 1.4 AU per article, a fact that did not change much over time (see figure 1). Our analysis includes 657 different regression models (an average of about five per AU).

The most frequent dependent variable has been conflict onset, studied in almost three quarters (ninety-seven) of the AU. Nearly all such studies focused on the number of fatalities in determining the threshold of onset, turning a blind eye to other indicators of conflict intensity used by, for instance, the Political Instability Task Force (PITF) (Marshall, Gurr, and Harff 2017), the Center for Systemic Peace (CSP) (Marshall 2017), and the Disaggregated Conflict Dataset (DISCON) (Trinn and HIIK 2016). Only four AU followed a qualitative approach incorporating a wider assessment of violence-induced consequences. In thirty-seven cases, conflict incidence was the variable to be explained; eleven of those followed a qualitative approach.

Some 93 percent of the studies focused on only one dependent variable (onset or incidence). Six articles considered two *explanantia*. As we learn from figure 2, onset analyses clearly dominated until 2012.

The reviewed articles used seventeen extant conflict databases.[5] In addition, the authors of eleven articles compiled their own datasets. The dataset by the Uppsala Conflict Data Program (UCDP) (Gleditsch et al. 2002; UCDP 2013) and the Peace Research Institute Oslo (PRIO) was used in fifty-eight different AU and is thus a long way ahead of the rest (figure 3). Next come the datasets by Fearon and Laitin (2003), the Minorities at Risk (MAR) Project (MAR 2009), and the Correlates of War (COW) Project (Sarkees and Wayman 2010) (nineteen, seven, and six, respectively).

While the dataset by Fearon and Laitin is important from the year of its release until about 2007, UCDP/PRIO's dominance was consolidated thereafter (see
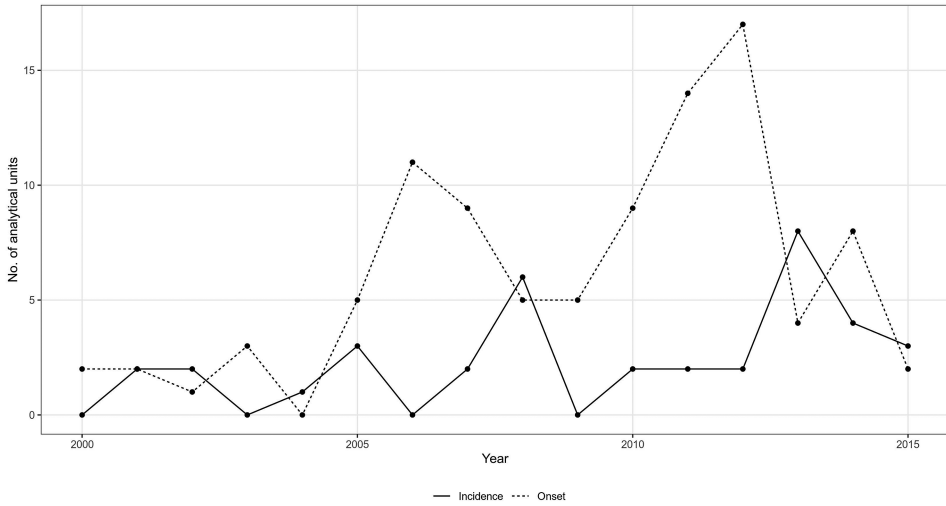
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[5] We counted different dataset versions as one.

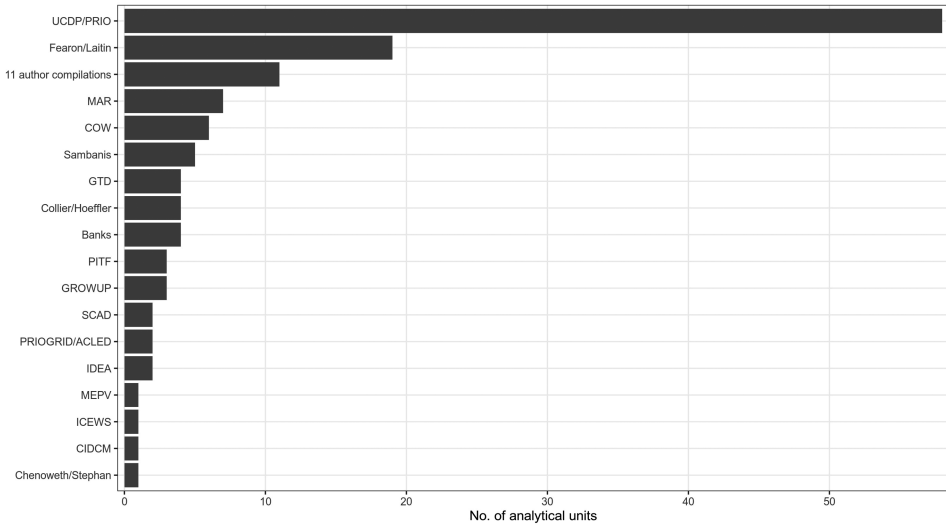**Figure 2.** Number of analytical units per dependent variable

Figure 3. Number of analytical units per dataset[13]

figure 4). The importance of other, established datasets, such as the Global Terrorism Database (GTD) (START 2018) or the Social Conflict Analysis Database (SCAD) (Salehyan et al. 2012), has been rising since 2010.

The continued use of self-compiled data may point to shortcomings of the main databases. Apparently, they do not always fulfill the needs of researchers. Even though most of the self-compiled datasets are mainly based on the data of UCDP, COW, MAR, or PITF, this extension of the number of datasets allows for more robust findings but, at the same time, restricts the comparability of the results (see Eberwein and Chojnacki 2001).

The analyses under review make use of an impressive variety of independent variables.[6] We identify 107 individual variables, analyzed a total of 6,364 times. This

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[6] Our synthesis does not distinguish between explanatory and control variables. Statistically, multiple regression does not differentiate between predictors. However, theoretical interest motivates the choice of the overall set of predictor
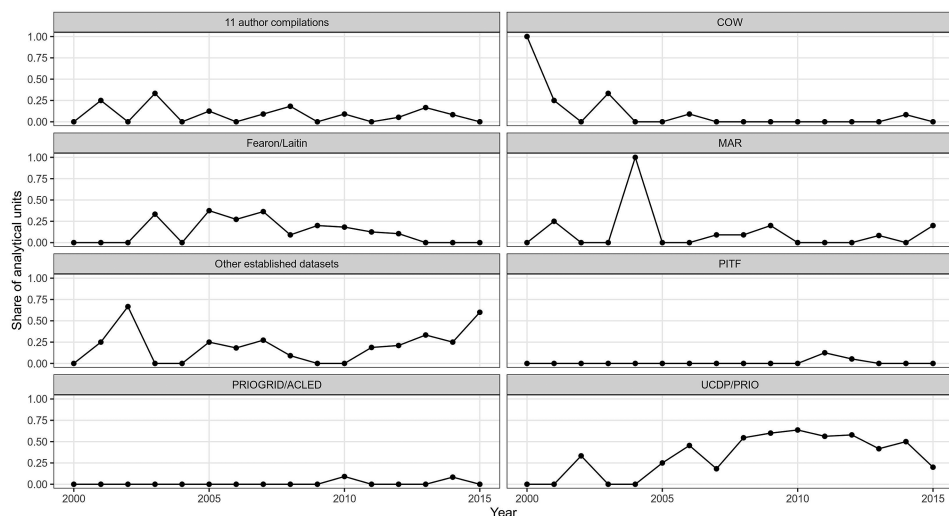
Figure 4. Relative share of datasets used

finding is based on a conceptual understanding of "variable": On the one hand, we sought to preserve the distinctions of the concepts embodied in the underlying definitions, e.g., between oil exports, production, and reserves. On the other hand, different operationalizations pointing to an identical or sufficiently similar concept were subsumed under a single variable. For instance, "conflict duration" is often measured in years, months, or days. The goal was to avoid the peril of "amorphous" conceptual identities (Dixon 2009) by reaching a compromise between differentiation and comparability. In order to fully grasp the complexity present in the articles under review, we also provide a more differentiated typology, which disaggregates the variables into 418 subentries (e.g., oil production on a metric scale, as a dummy and per capita). For the complete typology of independent variables, see the supplementary file.

The sheer number of independent variables shows that, on the whole, researchers seek to be precise and often to be innovative, putting to use nearly all data available for an increasingly sophisticated catalog of variables. Most variables, however, are rarely used. Only fifty-one (about 48 percent) were applied in more than five distinct analytical units. Seventeen variables were explored twenty times or more. The top five variables were living standards (primarily GDP per capita [log]), included in 119 AU; population size (101); ethnic diversity (70); previous conflict (51); and linear regime level (49).

We aggregate the multitude of independent variables into seven categories: (1) conflict variables cover, for instance, information on conflict actors, peace duration, or previous conflicts; (2) demographic variables (e.g., population size or ethnic fragmentation); (3) geographic variables (e.g., mountainous terrain or noncontiguous territory); (4) data on specific nonstate actors such as ethnic or rebel groups; (5) resource variables (such as diamonds or oil); (6) socioeconomic variables (e.g., GDP per capita, education, or inequality); and (7) political variables, including information on regime types and levels, internal power relations, or international relations. For a detailed overview of the typology, see the supplementary file.

Figure 5 shows the development of each category. On average, the political variables are most prominent (25 percent). Demographic and conflict variables

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variables. Consequently, we do not exclusively base our theoretical discussion below on our synthesized results but also take into account the arguments in the original articles. Moreover, we present results for coefficients of explanatory variables in figures 7 and 11.
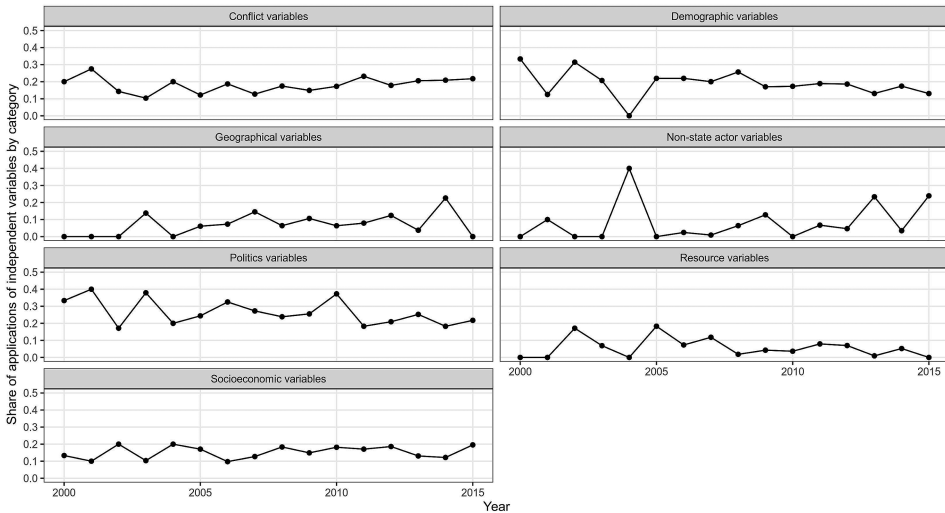
Figure 5. Relative share of independent variable applications broken down by category

account for roughly 19 and 18 percent, respectively, of all variable applications, socioeconomic variables for 15 percent. The other categories range from 6 to 9 percent each. While the frequency of demographic and resource variables has slightly declined over the years, the relative importance of geographic data and information on nonstate actors has increased. The latter finding shows that scholars have taken heed of Dixon's (2009) exhortation to include more group-level variables in their analyses.

Interaction terms were used 140 times, in thirty-one articles in twenty-five different regression models. Conditional relations were, therefore, studied in only about 2 percent of all variable applications. All interaction terms are unique for the specific studies, as only one interaction effect was analyzed twice across articles.[7] This makes a systematic synthesis impossible. About a third of the interaction terms turned out to be statistically significant, and 81 percent have the status of an explanatory variable. Most of the variables used in interaction terms cover the realms of economy (including resources), demography, culture, and power relations. By far the most common combination comprises two demographic variables (14 percent of all interaction terms). The effects of cultural properties linked to either power relations or regime variables are also common (9 and 7 percent, respectively). Economic factors are often linked to regime variables, demographic properties, or to other economic variables (7, 6, and 6 percent, respectively). Seventeen other combinations account for the other 50 percent of the applications.

Consensus Determinants of Violent Intrastate Conflict

The primary goal of this article is to arrive at a set of "consensus determinants" of violent intrastate conflict, to use a term by Dixon (2009). They are "determinants" not in the sense that we can be certain that they identify causal relationships, but all of them have been introduced to shed light on conflict causalities. When such a variable reaches a consensual status, this means that it has been analyzed *frequently* and produced *consistent* results. Therefore, while consensus determinants do not necessarily express causality, they represent robust covariation.

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[7] Ethnic fragmentation and democracy dummy.

We synthesize regression results by drawing on the sign test (Borenstein et al. 2009). This method tests whether the number of negative and positive coefficients for a variable significantly differs from a binomial distribution with a probability of success of 0.5.[8] Looking at each of the four outcomes of interest separately, we attribute explanatory power to a variable if a binomial test rejects the null hypothesis of a random distribution with the usual p-value of 0.05.

Concerning each outcome separately, the binomial test for a variable tested in less than six AU cannot fall below the threshold $p < 0.05$. To gain enough statistical power for our test, we require at least six AU per variable and outcome. Researchers focusing on a specific variable can apply this threshold, whereas those interested in ranking all available variables have to consider the problem of multiple hypothesis testing—that is, the risk of falsely rejecting the null hypothesis (type I error). To account for this, we apply the Holm-Bonferroni correction to adjust p-values. Since we are agnostic as to whether the variables under scrutiny have a positive or negative influence on the phenomena under review, we perform a two-tailed test.

For the *sign test*, the statistical significance found in the original studies is irrelevant. Drawing on the distribution of positive and negative coefficients irrespective of their significance, the sign test is immune to false negatives, which can arise in the original studies due to low statistical power.[9] In this case, the sign test offers an advantage over exclusively focusing on statistically significant results, which is typical for narrative reviews. Hence, our approach offers an alternative to "vote counting" as applied by, for example, Dixon (2009). Not taking into account levels of significance and not exclusively focusing on variables in theoretical focus, the sign test also mitigates biases due to selective reporting (or "p-hacking").

Multivariate model specifications in the synthesized studies vary greatly with regard to the included covariates. The sign test requires models in the original studies to be sufficiently well specified. Drawing on the signs rather than the values of coefficients somewhat mitigates the problem of possibly biased estimators. In extreme cases, however, misspecification can lead to a change in coefficient signs. Our synthesis thus rests on the assumption that neither the key explanatory variables nor the covariates are biased in a way that would lead to a reversal of the signs of coefficients.

The main drawbacks of the sign test are that it neither allows to estimate effect sizes nor takes into account sample sizes (Borenstein et al. 2009, 326). Moreover, unlike genuine meta-analyses, it does not enhance the statistical power of the original studies by pooling observations. Although we are aware that superior methods of synthesizing studies exist, we consider the sign test the best available option in view of our study design for three reasons.

First, comparing effect sizes across studies is complicated by varying variable specifications across studies (Becker and Wu 2007). For instance, units of measurement often differ (an example are different inflation adjustments in GDP measurement). Synthesizing effect sizes would thus require extensive recoding.

Second, as argued above, studies in conflict research repeatedly analyze the same population. Consequently, pooling these studies would not significantly increase the sample size, which is a key motivation of meta-analysis. We partly account for a lack of independence between studies by aggregating the results of regressions

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[8]If several models subsumed under a single AU are contradictory with regard to the direction of influence for a variable, we coded this as "indifferent" and excluded it from the synthesis.

[9]For instance, the probability that in ten independent studies at least nine coefficients turn out positive or at least nine coefficients turn out negative for a variable unrelated to the outcome is 2.1 percent. If not accounting for multiple hypothesis testing, we would reject the null hypothesis of no association with a p-value of 0.021 even if none of the coefficients turned out significant in the original studies.
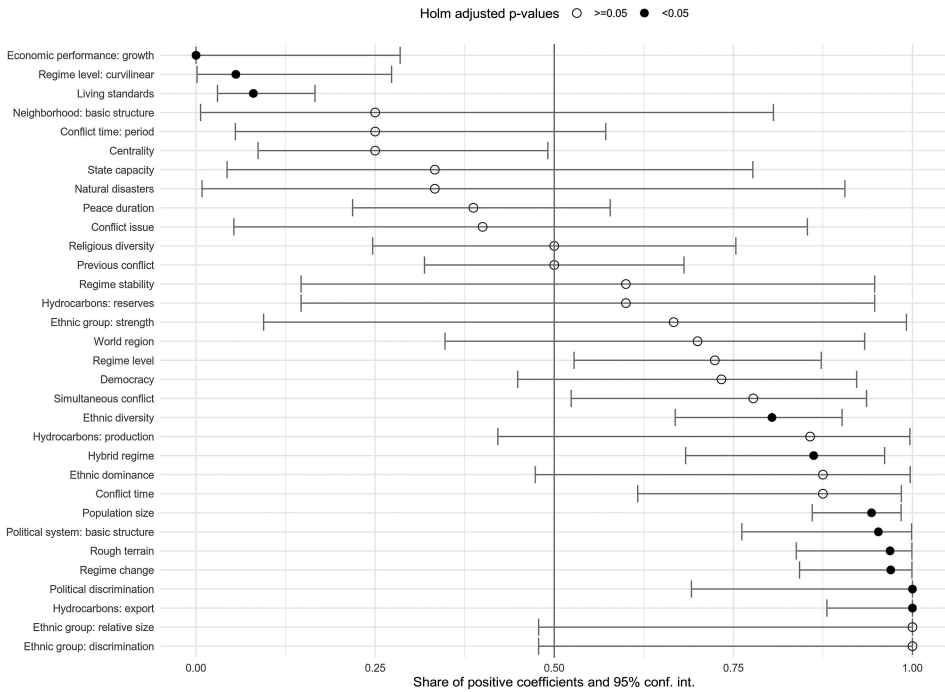
Figure 6. Variables related to conflict onset

in "analytical units," as explained above. Although this does not solve the issue of overlapping or even identical samples *across* studies, our procedure allows for uncovering the robustness of effects given different study designs. Consequently, we do not consider our approach a genuine "meta-analysis."

Third, other methods provide no viable alternative. Vote counting (i.e., synthesizing studies by counting significant positive and negative as well as nonsignificant coefficients) is generally discouraged (see, e.g., Borenstein et al. 2009, 251–55). Hedges and Olkin (1980) show that the power of vote counting might even decrease with a growing number of studies under review. Combining p-values in order to test for the null hypothesis that no study under review shows a statistically significant effect (see Borenstein et al. 2009, 326–30) does not fit our research question as it puts too much value on sensitivity—that is, avoiding false negatives.

Figure 6 presents the results of our synthesis for the outcome "conflict onset." The points indicate the share of positive coefficients of the respective independent variable. Filled points show whether the p-values adjusted using the Holm-Bonferroni method lie below $p = 0.05$. Those comparing the relative explanatory power of all variables in the list should focus on the more conservative, adjusted p-values (indicated by filled points). The confidence intervals provide insight into the certainty of this estimate. The more often a variable has been tested, the smaller is the confidence interval.

Our results show that the onset of conflict seems to be associated with hydrocarbon export (primarily oil), political discrimination and exclusion, regime change, rough (especially mountainous) terrain, the basic structure of political systems (primarily recent independence), population size, anocracy (hybrid regimes and "inverted-U" effects), ethnic diversity (mostly fragmentation), low living standards (mostly GDP per capita), and low economic growth (see figure 6). To give an idea

of why the identified variables might be causally relevant, the following discusses the theoretical arguments brought forward in the literature.[10]

The size of the population is almost always used as a control variable. In light of the clear pattern emerging from the data, it is rather surprising that none of the reviewed articles detail the causal mechanisms linking population size and conflict onset. The general assumption is that larger populations are more diverse and more likely to contain at least one (ethnic) group that is willing to rebel (Gleditsch 2007; Buhaug, Cederman, and Rød 2008). Wimmer and Min (2006, 886) illustrate this by means of a thought experiment: "Imagine a one-person country in which the likelihood of warfare is necessarily 0 and contrast this with a world-state in which all wars would be counted as civil wars." Other authors provide more indirect arguments: population size indicates the state capacity and/or opportunity costs (Thyne 2006); larger populations tend to live in bigger countries with larger domestic markets that make them less outward-looking (Schneider and Wiesehomeier 2008); and large populations are more likely to experience natural disasters (Slettebak 2012).

Theoretical arguments regarding ethnic fractionalization broadly follow either a rationalist or "grievance"-oriented reasoning (see Collier and Hoeffler 2004). Rationalist approaches hold that diversity reduces the conflict actors' ability to organize efficiently (see Wegenast and Basedau 2014). Along this line, Reynal-Querol (2002) argues that ethnic groups in highly fractionalized populations are less likely to overcome the collective action problem. Additionally, ethnic diversity limits the recruitment pool available for each group (Collier and Hoeffler 2004). Gibler and Miller (2014) extend this argument to the government side, arguing that fragmentation decreases the state's recruiting abilities, as well. The "grievance" argument is aptly summarized by Buhaug (2006, 698): "The more fragmented a state, the higher the number of potentially marginalized or excluded ethnic groups." This line of reasoning can be tied to the ability of a regime to co-opt ethnic groups into government positions, an ability that decreases with increasing fractionalization (Schneider and Wiesehomeier 2008). De Soysa and Fjelde (2010) observe that highly fractionalized societies are faced with coordination problems, which might negatively affect the quality of governance, leading to grievances. Blimes (2006) cuts across both camps by arguing that ethnic heterogeneity does not directly increase the probability of violent intrastate conflict. Rather, it might make societies more susceptible to factors that have a direct effect on conflict onset.

Two lines of reasoning link a country's status as an oil exporter to a higher probability of conflict onset. Both arguments were originally introduced by Fearon and Laitin (2003). On the one hand, oil-dependent countries are likely to develop rentier states where governments are less dependent on raising revenues by levying taxes. The reduced incentive for the state to penetrate society might lead to less administrative control and less structured and capable bureaucratic systems (Fearon 2005; Buhaug 2006; Thyne 2006; Gubler and Selway 2012). Rentier economies might also leave governments with fewer incentives to develop a broader economy and to provide "public goods in order to raise productivity" (de Soysa and Neumayer 2007, 202). On the other hand, "oil revenues raise the value of the 'prize' of controlling state power" (Fearon and Laitin 2003, 81; see Koubi et al. 2012). This "prize" can lead to two different scenarios of "state capture": by offering an incentive for rebel groups to seize the government (Le Billon 2001) or by inducing rent-seeking among political elites (Fjelde 2009).

Although GDP per capita consistently turns out to be a frequently employed explanatory variable, it is also difficult to interpret because of competing and highly contested theoretical approaches (Collier and Hoeffler 2002; Buhaug, Cederman,

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[10]We primarily screened those articles for theoretical arguments that featured signs of coefficients identical to the results of our synthesis as we have intended to substantiate the reasoning behind our findings. We do not present an exhaustive theoretical review of the literature.

and Rød 2008; Blattman and Miguel 2010; Gleditsch and Ruggeri 2010; Cederman, Weidmann, and Gleditsch 2011). Three different explanations appear in the literature. First, GDP per capita is used as a measure for state capacity in general and military strength in particular (Collier and Hoeffler 2002; Fearon and Laitin 2003; Thyne 2006; Nieman 2011; Wegenast and Basedau 2014). Weak states often have low counterinsurgency capabilities, allowing insurgents to establish bases in or control over peripheral areas (Cederman, Buhaug, and Rød 2009; de la Calle and Sánchez-Cuenca 2012). This reasoning has been disputed, however, since not all poor states are weak, as exemplified by the case of North Korea (Gleditsch and Ruggeri 2010). Second, it is argued that low per capita income lowers the opportunity costs for rebels as their alternative, forgone income decreases. This in turn lowers the recruitment costs, making rebellion financially more viable (Collier and Hoeffler 2002, 2004; Lujala, Gleditsch, and Gilmore 2005; Thyne 2006). Cederman, Buhaug, and Rød (2009), however, shed doubt on this argument as they find that low state-reach explains civil war risk better than the depth of poverty. A third argument concerns the infeasibility of distributive measures for poor states. They lack the resources that enable richer states to provide public goods, redistribute wealth, invest in social insurance services, and generally reduce class conflict (Hegre et al. 2001; de Soysa 2002; Wimmer and Min 2006; Fjelde and de Soysa 2009). When the economic "cake" is bigger, such distributive measures might pacify, rather than crush, opposition, thereby legitimizing the government (cf. Cederman, Weidmann, and Gleditsch 2011).

Collier and Hoeffler (2002) argue that economic growth reduces the risk of conflict—either by making rebel recruitment more difficult because job opportunities increase or by alleviating grievance levels. This view has been adopted by de Soysa (2002), Sørli, Gleditsch, and Strand (2005), de Soysa and Fjelde (2010) as well as Hendrix and Salehyan (2012). Bohlken and Sergenti (2010) find a strong negative link between growth rates and riots. Innovatively, Koubi et al. (2012) argue that a lack of economic growth might mediate the effect of climatic conditions on violent conflict. Despite the seemingly clear findings, Hegre and Sambanis (2006) warn that the results for economic growth are still ambiguous.

Fearon and Laitin (2003) argued that civil war becomes more likely when a newly independent state loses the coercive backing of the former colonial power, thus favoring the political and military resources of the insurgents. Many authors replicate Fearon and Laitin's model without bringing forward new arguments (e.g., Lujala, Gleditsch, and Gilmore 2005; Blimes 2006; Salehyan and Gleditsch 2006; Cederman and Girardin 2007; de Soysa and Neumayer 2007; Buhaug, Cederman, and Rød 2008; Thies 2010; Nieman 2011; Cunningham and Lemke 2013; Regan and Frank 2014). However, Hegre and Sambanis (2006, 527) warn that the new state variable is "theoretically ambiguous," as newly independent states are more likely to have a hybrid regime, be more unstable, and be ethnically fractionalized. These aspects might be captured by other variables in the same model.

Following Ellingsen (2000), Hegre et al. (2001), and Fearon and Laitin (2003), "anocracies"—that is, semidemocratic or semi-authoritarian, hybrid, institutionally mixed, and inconsistent regimes in the gray zone between democracy and authoritarianism ("inverted-U")—are viewed as inherently instable and particularly prone to conflict (see Salehyan and Gleditsch 2006; Brancati 2007; Forsberg 2008).

> These states combine some level of repression with a limited degree of openness: an unfortunate mix that enables the annoyed populace to protest against the inept regime, which all too often responds by calling upon regular or paramilitary forces. (Buhaug 2006, 696)

However, Hegre and Sambanis (2006) pointed to endogeneity concerns since the Polity index (Marshall, Gurr, and Jaggers 2016), as the most widely used regime dataset, includes the occurrence of extensive political violence as part of its

assessment of nonregulated participation. This effect was substantiated by Vreeland (2005, 2008), who showed that the seemingly reliable finding that anocracies as measured by Polity are more susceptible to civil war is an artifact. Results concerning this variable should therefore be treated with suspicion (see Treier and Jackman 2008; Cederman, Hug, and Krebs 2010). Nevertheless, there have recently been a number of analyses that continue to pursue the anocracy argument with the Polity data (Fjelde and Nilsson 2012; Mousseau 2012; Slettebak 2012; Forsberg 2014).

Regime change—termed "political instability at the center" by Fearon and Laitin (2003)—is linked to disorganization and state weakness (see Thyne 2006). Hegre et al. (2001) argue that the interim period after the authoritarian regime has collapsed is particularly vulnerable to ethnonational agendas. Cederman, Hug, and Krebs (2010) show that regime change is indeed linked to the outbreak of armed conflict but that different processes underlie the emergence of conflict in cases of democratization and autocratization. Partial elites in new democracies need time to mobilize supporters, which is especially feasible after losing elections. By contrast, in autocratization processes, "already politically mobilized citizens oppose the closure of the political space" (Cederman, Hug, and Krebs 2010, 379), potentially leading more quickly to conflict escalation.

Tir and Jasinski (2008, 650) argue that political discrimination (i.e., the exclusion of individuals or groups from political power) is less likely in democracies, "but even some democracies have been found to engage in institutionalized discrimination." The level of discrimination is thus not necessarily identical with the regime level. The studies under review investigated political discrimination primarily at the level of (mostly ethnic) groups. Many used the Minorities at Risk (MAR) or the Ethnic Power Relations (EPR) dataset. Excluded groups and those being the objects of state-led discrimination are more likely to harbor grievances against the government, which might result in violent action (Cederman, Weidmann, and Gleditsch 2011; Cederman et al. 2013; Cederman, Weidmann, and Bormann 2015; Omelicheva 2011). Political discrimination has these potentially explosive effects also because it often translates into economic discrimination, increasing the scope of grievances (Regan and Norton 2005). Wimmer and Min (2006) specifically address the role of political discrimination along ethnic lines in conflicts occurring during nation-state formation processes.

Almost all analyses that find mountainous terrain to be positively related to conflict onset argue that rough terrain provides cover and hideouts for insurgents (Fearon and Laitin 2003; Bussmann, Schneider, and Wiesehomeier 2005; Wimmer and Min 2006; Brancati 2007; Nieman 2011) or weakens government forces due to impeding conventional warfare (Cederman, Buhaug, and Rød 2009; Gibler and Miller 2014). Bussmann, Schneider, and Wiesehomeier (2005, 573) are the only authors in our sample arguing that "[m]ountainous terrain might also be related to lower levels of trade as it inhibits transportation."

As we can see, the literature brings forward numerous theoretical arguments for each consensus variable. In fact, there are usually several alternative explanations. Thus, even if we can derive from the above synthesis a set of potential determinants of conflict onset, we do not know *why* these determinants might influence the emergence of violence. We are in dire need of empirical analyses geared to assess the proposed causal mechanisms themselves. Unfortunately, most of the literature so far does not fulfill this need.

Forty other variables were tested in at least six different AU but did not reach statistical significance in the sign test. This is because of low statistical power in the sign test and possibly false negatives, because the synthesized results are undecided as to the direction of association, or a combination of both. Eight of these variables have been analyzed quite often (more than twenty times) but with inconclusive results: previous and simultaneous conflict, peace duration, regime level in general and democracy in particular, religious diversity, geographic centrality, and the historical
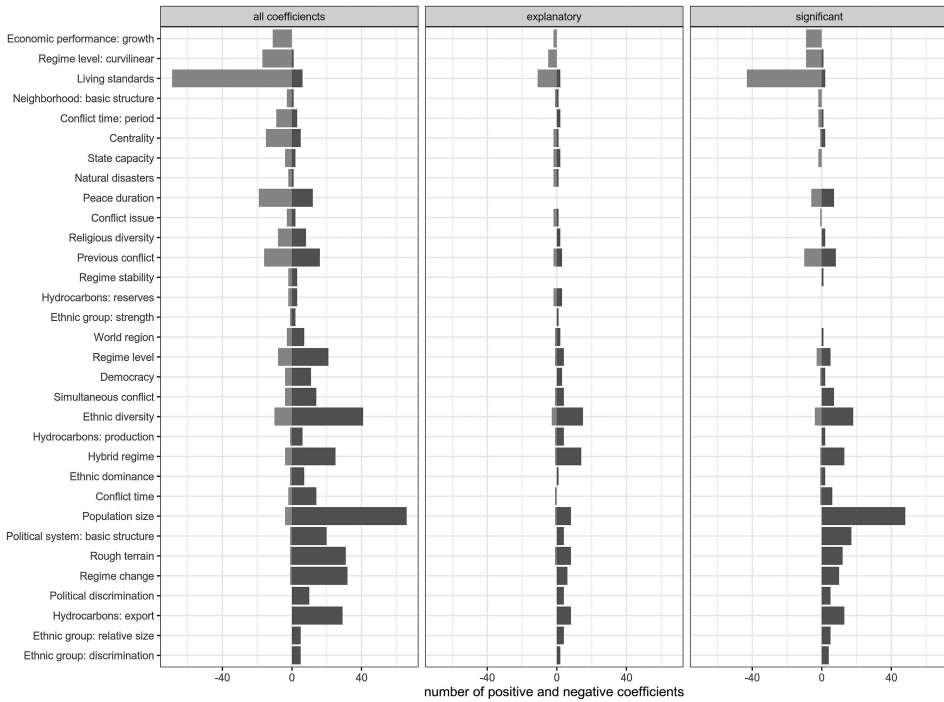
Figure 7. Explanatory and significant variables related to conflict onset

period (e.g., post–Cold War). Further research is needed to decide which direction of causality applies (if any) or whether these variables have different effects for different types of conflict.

A further fifty-six variables appeared only five times or less in onset analyses. Even though rare variables might in fact be important, we simply do not know enough about them to be sure.

While the first data column of figure 7 reflects the full information contained in figure 6, the second column details the number of positive and negative coefficients for those variables that were accorded explanatory status by the respective authors, thus excluding control variables. Ethnic diversity, hybrid regimes, and low living standards are mostly associated with conflict onset among explanatory variables. Column 3 shows the direction of influence for the statistically significant variables (explanatory and control variables combined). In the studies under review, population size and low living standards were regularly the most significant factors. The information given for explanatory and significant variables allows the reader to add additional criteria when assessing a variable's robustness.

Table 1 compares the results of our synthesis with the seminal review by Dixon (2009). The latter used vote counting to establish which factors could be considered established. Shown here are those variables that turned out as consensus determinants either in Dixon's or our review. Clear-cut cases are living standards (primarily GDP), economic growth, hydrocarbon export (especially oil), population size, regime change, and curvilinear regime levels (whose problematic nature was discussed above.) The direct comparison tells us that this constitutes the set of the most robust results in country-level intrastate conflict onset research so far. Secondary members of this set, which only attained medium levels of confidence in Dixon's analysis but turned out as consensus determinants in our review, are ethnic diversity, recent independence, and rough (especially mountainous) terrain.

Table 1. Cross-review comparison of variables determining conflict onset

Variable	Our review	Dixon
Economic performance: growth	C	X
Ethnic diversity	C	(X)
Ethnic dominance	Ø	(X)
Hybrid regime (anocracy)	C	O
Hydrocarbon export	C	X
Living standards (esp. GDP, GDP p.c.)	C	X
Mass education	Ø	(X)
Peace duration	Ø	X
Political discrimination	C	O
Population density	Ø	(X)
Population size	C	X
Previous conflict	Ø	(X)
Primary commodity exports	Ø	(X)
Rainfall	Ø	(X)
Recent independence	C	(X)
Regime change	C	X
Regime level: curvilinear	C	X
Religious diversity	Ø	(X)
Rough terrain	C	(X)
Simultaneous conflict	Ø	(X)
Soil degradation	O	(X)

C: consensus variable in our synthesis (adjusted p-value < 0.05), Ø: nonconsensus variable (adjusted p-value ≥ 0.05), X: highly consistently significant, (X): moderately consistently significant, O: not in review. GDP, GDP per capita, GDP growth, and GDP growth per capita appear together as "prosperity" in Dixon's findings. Ethnic, religious, and ethnoreligious fragmentation are subsumed by Dixon under "social fractionalization." Primary and secondary "clear-cut" consensus determinants in bold.

Across different review methodologies, these six plus three variables are those that turn out as most robust across studies in both reviews.

There has been evidence that the results regarding the explanatory power of the independent variables are not independent from the conflict dataset with which they were analyzed. This was demonstrated by Sambanis (2004b). The following compares results between conflict data provided by Fearon and Laitin and UCDP/PRIO as the most widely used datasets (see Figure 8). While a number of results are broadly similar and thus robust across datasets, considerable differences emerge with regard to the role of religious diversity, the (linear) regime level, previous conflict and centrality (which primarily covers the [non]contiguity of a territory). Choosing the specification of the dependent variable codetermines the findings for these independent variables. Making use of more than one conflict dataset in a single analysis thus appears to be recommended to increase the robustness of the results.

A point of concern might be whether our concept of violent intrastate conflict is valid or too broad. Subtypes of this concept, such as civil war and low-intensity conflict, might arise from different causal mechanisms and thus warrant distinct explanations. This might affect the validity of our test: the signs of the estimated coefficients only come from the same binomial distribution if we deal with one coherent phenomenon. To address this concern, we carried out a subset analysis, differentiating between AU focusing on civil wars as a potentially distinct phenomenon (with more than a thousand fatalities per year or conflict), on the one hand, and those AU that do not distinguish between intensity levels (but excluding civil wars), on the other hand. As only a single model analyzed the onset of conflicts with less than a thousand fatalities in isolation, it was not feasible to compare civil wars with
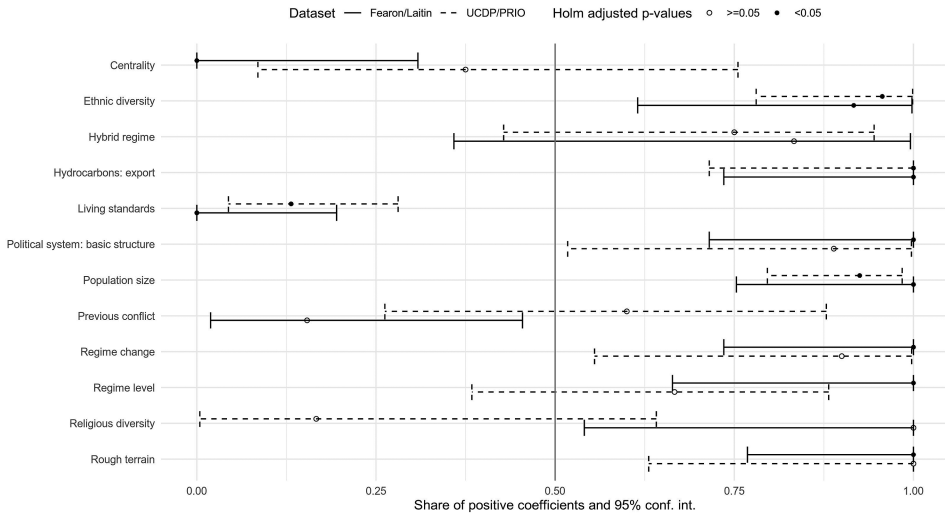
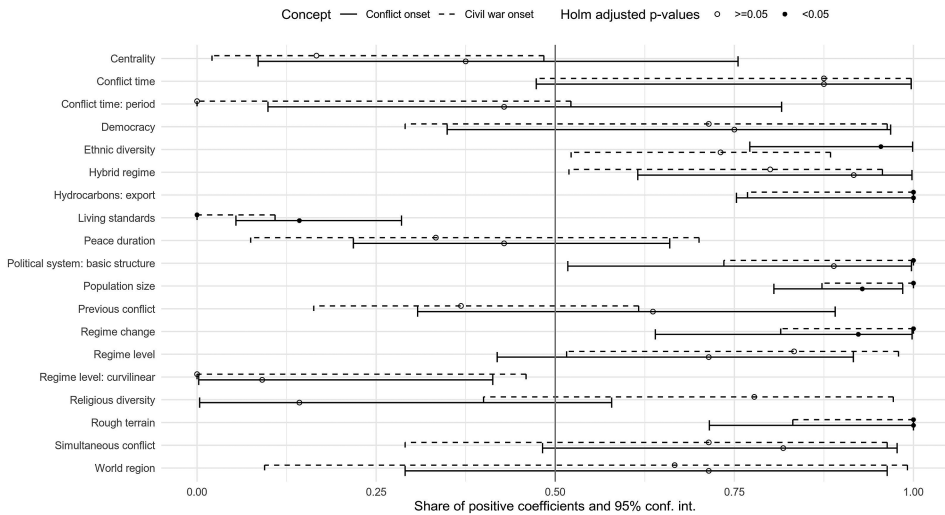**Figure 8.** Variables related to conflict onset by dataset

Figure 9. Subset analysis of conflict versus civil war onset

low-intensity conflicts. Also, including qualitative conceptualizations in this disaggregated comparison was not feasible due to the low number of studies.

We find that in explanatory terms civil wars are a phenomenon not so distinct from violent intrastate conflicts in general as to warrant a disaggregation of our concept (see figure 9). Most results are robust across the two subsets. This supports our argument above that high and low intensities are not different types of conflict but different stages of escalation processes. Still, the analysis points to factors that might be of particular importance for the emergence of civil war–level violence. We find the largest difference in estimates for religious diversity, previous conflict, and geographic centrality. In all cases, however, statistical significance in the sign test is low. The asymmetry of our comparison (all versus high-intensity conflicts) effectively impedes a more informative analysis and the construction of a "causal narrative" of the escalation of intrastate violence to the level of war. More studies
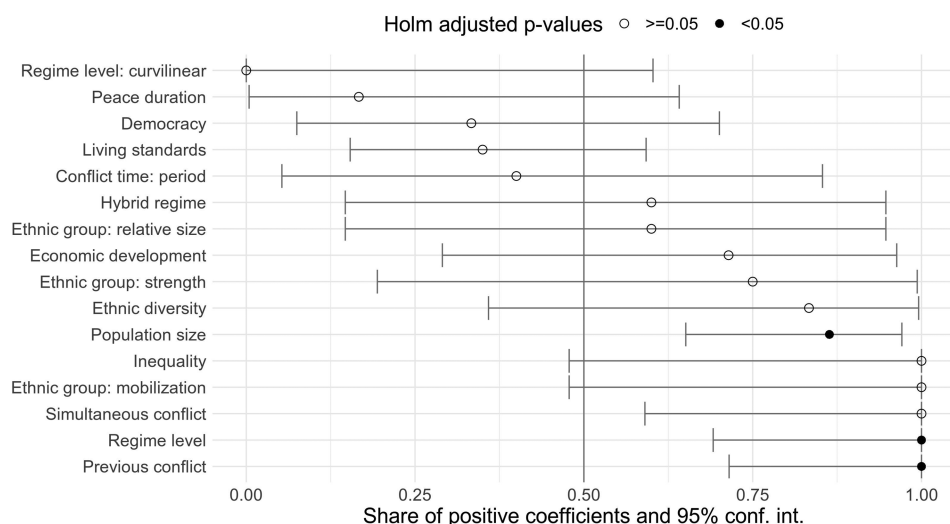
Figure 10. Variables related to conflict incidence

directly comparing high- and low-intensity conflicts are clearly needed to better understand what drives or intensifies escalation dynamics.

Turning from conflict onset to conflict incidence, figure 10 summarizes the predictors of conflict incidence. The consensus determinants emerging from our analysis are population size, the regime level, and previous conflict, which are positively associated with the occurrence of violent intrastate conflict. The results for all other variables point to a lot of uncertainty, owing to the relatively low number of studies dealing with conflict incidence.[11] Regan and Norton (2005) argue that the pool of possible rebels is larger in more sizable populations. Following this argument, the risk for each individual rebel to be punished is smaller, making rebellion more attractive. In a similar vein, Enterline and Greig (2008) contend that an authority's ability to exert control might depend on the size of the population. Warren and Troy (2015) use population size as a measure of state capacity and state-reach. Regime level is generally used to account for linear effects of political institutions (Fox 2004; Regan and Norton 2005; Iqbal and Starr 2008; Warren and Troy 2015). Previous conflict as a variable is intended to capture path dependence effects on the incidence of conflict (Cunningham 2013b).

Figure 11 details the information on the number of positive and negative coefficients for explanatory and significant variables. The information largely echoes the findings for conflict onset. Among explanatory variables, ethnic diversity is most often associated with conflict incidence (albeit with modest overall numbers). Population size and previous conflict, but also low living standards, are often statistically significant. Whereas the onset of violent conflict does not appear to be associated with previous conflict, this is the case with conflict incidence, pointing to conflict perpetuating processes.

Summary and Discussion

This article has provided a bird's-eye view on sixteen years of research into the onset and incidence of violent intrastate conflicts. By taking a step back, the article synthesizes studies from a field of research characterized by increasing thematic

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[11] Due to the low number of studies, a subset analysis as carried out for conflict onset was not feasible for conflict incidence.
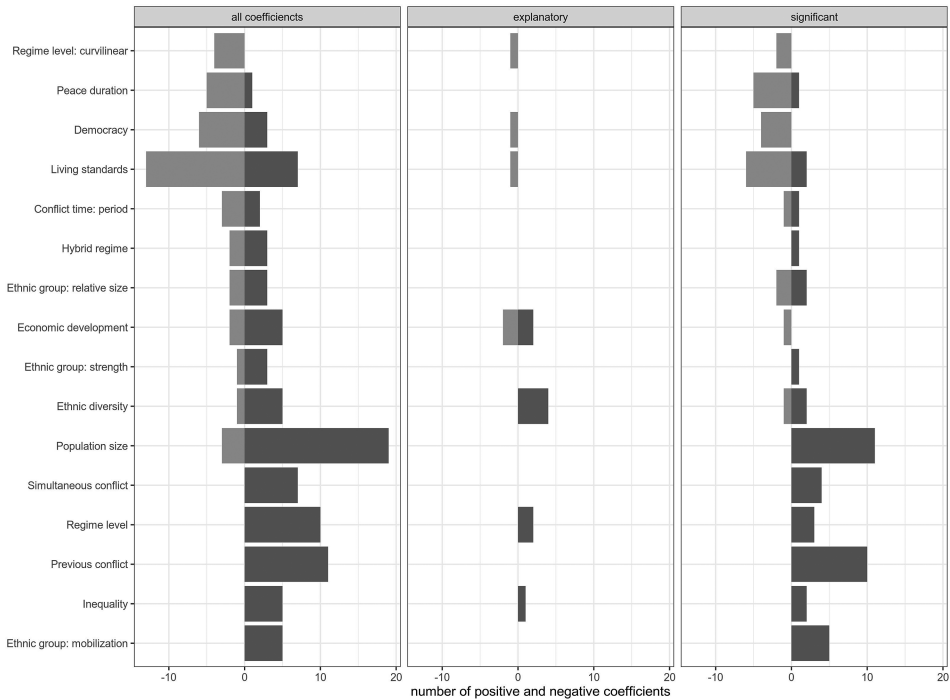
Figure 11. Explanatory and significant variables related to conflict incidence

differentiation and methodological sophistication. While poring over the results of hundreds of statistical analyses might not seem the most exciting of exercises, the findings are remarkable in several ways.

The ninety-four articles under review made use of a remarkably long and varied list of 107 distinct independent variables. This list, however, quickly boils down to a relatively small set of *consensus determinants.* The onset of conflict is associated with hydrocarbon export (primarily oil), political discrimination, regime change, rough (especially mountainous) terrain, population size, the basic structure of political systems (primarily recent independence), anocracies (hybrid regimes and "inverted-U" effects), ethnic diversity (mostly fragmentation), low living standards (mostly GDP per capita), and low economic growth. A direct comparison with the seminal review by Dixon (2009) supports the robustness of all but one of these variables. (There is not a separate entry for political discrimination in Dixon's list of consensus determinants.) These factors are frequently and robustly associated with the onset of violent intrastate conflict.

Analyses of conflict incidence were far less prevalent. The amount of uncertainty surrounding the determining factors is accordingly much greater. Population size, regime level, and previous conflict are the only variables that "survive" our analysis. More research is clearly needed to arrive at a level of knowledge comparable to conflict onset.

In all likelihood, these variables are not the only factors determining the onset or incidence of intrastate violence. However, they are the only ones where previous research allows us to say something certain. When future research expands our knowledge about other, so far understudied variables, they might be added to our lists of consensus determinants.

A close reading of the arguments brought forward by the authors for including a specific variable in a regression model shows that, in a comparative perspective, we

still know remarkably little about the underlying causal mechanisms of the genesis of violent intrastate conflict—that is, *why* the variables that turn out significant in statistical analyses should have any effect: "Ultimately, empirical work should aim to distinguish which of the competing theoretical mechanisms best explain the incidence, conduct, and nature of civil war, but this goal is still far from being realized" (Blattman and Miguel 2010, 22). This has a lot to do with the nature of quantitative research itself. While the level of idiosyncratic detail in many qualitative studies often prevents us from seeing the wood for the trees, large-N studies are generally too remote from the causal processes. The analysis of actual conflict trajectories is exceedingly rare in the articles under review. This is specifically relevant as the identification of causal relations depends on adequate causal models (Pearl 2000).

This problem is aggravated by the fact that conflict researchers are accustomed to proceeding in their theorizing from actor-centered foundations on the individual level while measuring their explanatory concepts by means of structural indicators on the societal level:

> The already significant gap between micro-level behavior and their macro-level explanation is magnified when . . . micro-macro relationships are studied solely through cross-national statistical analyses. Such studies often overlook information about causal pathways that link individual or group behavior with the outbreak of civil war. (Sambanis 2004a, 259)

This discrepancy could be solved either by including individual or at least group-level variables in the analysis or by taking the theoretical implications of societal structures seriously. The first strategy has taken root in recent years, with conflict research increasingly focusing on microlevel dynamics of intrastate conflict (for an overview see Verwimp, Justino, and Brück 2009; Haer, Vüllers, and Weidmann 2019). The collection and integration of event data (Zhukov, Davenport, and Kostyuk 2019) and the use of surveys (Lyall, Graeme, and Imai 2013; Brück et al. 2016) have contributed to better data. Nonetheless, theoretical and methodological challenges in linking micro- and macrolevel research remain (Balcells and Justino 2014).

The second strategy, a *substantive* analysis of social structures, was often impeded by the fact that historically such an analysis was in the domain of overly "structuralist" approaches, which detached structures from the individual level. This legacy has been overcome by understanding society as a reality *emerging* from the complex networks of interindividual interactions (see Archer 1995; Sawyer 2005). Conflict as a social-level phenomenon is seen as neither separated from nor fully described by individual choices. Rather, conflicts emerge from processes internal to conflict dynamics themselves (Clauset, Young, and Gleditsch 2007; Bohorquez et al. 2009). This echoes the observation by Kalyvas (2007) as well as Cederman and Vogt (2017) that violent intrastate conflicts are "deeply" and "inherently" endogenous processes. In such an emergentist perspective, structural factors are not so much independent "root causes" exogenous to conflict but rather descriptors of the multidimensional structure from which conflict processes emerge.

Even though society-level structural data have dominated the models of quantitative research during the last fifteen years or so, relevant sections of the social world have rarely been considered. For instance, information on tax revenues, social variables referring to health, or data on the demographic composition of a population ("youth bulge") have rarely been used, at least in our sample. There are some analyses that address these variables (e.g., Pinstrup-Andersen and Shimokawa 2008; Thies 2010; Østby and Urdal 2014). However, the relative rarity of the factors is remarkable since two concepts pivotal to many theories on intrastate conflict, citizen satisfaction ("grievance") and state capacity (see Fearon and Laitin 2003), are directly reflected in these variables. Despite the fact that grievance has ostensibly been thoroughly analyzed, it is therefore still severely understudied. The

renewed interest in "horizontal inequalities" between ethnic groups (Østby 2008; Stewart 2008; Cederman, Weidmann, and Gleditsch 2011) holds out the prospect of progress in this field. However, the problem is not limited to the level of independent variables. It is already present at the level of conflict data compilation. As Dixon (2009, 728) observes,

> there is a serious barrier to delving further into grievances. None of the most-commonly used datasets on civil war (PRIO, COW, Collier and Hoeffler, Fearon and Laitin, Doyle and Sambanis) include government policy as an issue in civil wars. Note that the PRIO/Uppsala definition of civil war actually excludes all such cases from its conflict list, for it defines armed conflict as "a contested incompatibility that concerns government and/or territory."

In view of this, researchers need to take into account a wider array of issues contested in intrastate conflicts (Trinn and Wencker 2018).

Since our review covers an extended period of time, it is not limited to the most recent developments. The availability of more detailed information on conflictive behavior owing to the new media revolution has led to profound changes in recent years, away from the highly aggregated country-year data utilized in the "standard" variables. The *disaggregation* of conflict information has primarily affected two areas: First, it allows for the compilation of georeferenced conflict event data. Examples include UCDP's Georeferenced Event Dataset (GED) and PRIO's Armed Conflict Location and Event Data (ACLED). The unprecedented level of spatiotemporal precision enables the observer to come ever closer to the concrete "flow" of conflicts. This trend is likely to increase as automatic coding procedures advance (Hammond and Weidmann 2014). Spatial disaggregation also aids the research into transnational dynamics and diffusion effects—for example, by more clearly defining conflict zones and establishing transborder ethnic links (see Cederman et al. 2013).

However, conflict disaggregation also poses methodological and theoretical challenges. The measurement of independent variables should ideally mirror or at least approximate the level of precision of the conflict data. This is not the case for most variables, but their numbers are rising (e.g., Nunn and Puga 2012; Alegana et al. 2015; Stevens et al. 2015). Yet it should be kept in mind that some concepts, such as "democracy," are generally only meaningful on a national level. This requires advanced statistical techniques such as multilevel modeling, an approach that has very rarely been applied in conflict research. A high resolution of measurement also has important conceptual repercussions. The question is whether we can reduce a high-level concept such as conflict to separate incidents. A conflict is more than an aggregation of the events associated with it. A single attack does not make it a war. The intensity of a conflict rather arises from the iterated interplay of actors and the consequences ensuing from it (Trinn and Wencker 2018). The "big picture" only emerges when we take a step back.

The second area affected by conflict disaggregation is the gathering of structural data on specific ethnic groups and organizational information on specific rebel groups (e.g., Cederman, Buhaug, and Rød 2009; Weidmann 2009; Wucherpfennig et al. 2012; Cunningham 2013a; Lacina 2014). To some extent, this alleviates the mismatch, described above, between the dominating agency-centered theories of violent intrastate conflict and the widespread country-level measurement of societal structures. However, as most rationalist explanations focus on *individual* decisions, group-level data alone cannot genuinely solve this problem. Still, the characteristics of state or group leaders have so far been of no particular importance in the modeling of intrastate conflicts.

Although country-year level regression analyses have certainly not become obsolete (Blattman and Miguel 2010), the findings gathered in this article demonstrate

the limited usefulness of this approach. Even after several years of research and enormous efforts by the scholars involved, we have to acknowledge that we still do not know very much about the causes of violent intrastate conflicts.

Modern methods of inference and analysis might crucially shape not only how conflict research is done but also which findings it produces. Observational research has not yet adequately taken up the potentials of quasi-experimental designs as exemplified by the potential outcome framework (Rubin 1974; Holland 1986) and the elaborated logic of causal inference developed by Pearl (2000). These approaches have convincingly shown that carefully developed causal models should guide case and variable selection. While classical regression analysis often aimed at increasing the number of observations to boost statistical power and at adding sets of common control variables, these approaches more strongly focus on creating conditions that mirror experimental settings. Examples include matching, difference-in-difference, and regression discontinuity designs (Morgan and Winship 2007).

At the same time, disaggregated data have increasingly become available, as we have seen. Spatiotemporal disaggregation has reached an unprecedented level with the "big data" generated by, for instance, modern telecommunication, social networking services, household censuses, or satellites. Information from these and similar sources are likely to play a major role in the next chapter of conflict research (Gleditsch, Metternich, and Ruggeri 2014). The fact that "much of big data is social data" (Monroe et al. 2015) signifies its great potential for social science research.

With regard to methods of analysis, it is foreseeable that predictive analytics will more and more come to the fore (Ward et al. 2013; Schneider, Hadar, and Bosler 2017). Schrodt (2014) even elevated out-of-sample predictions to a quality criterion of conflict research. Schneider, Gleditsch, and Carey (2011) distinguish three different approaches to prediction: the general risk assessment of geographical units, similar to earthquake forecasting (Rustad et al. 2011); the forward projection of dynamics and magnitudes based on time-series data from ongoing conflict processes (Brandt, Freeman, and Schrodt 2014; Chadefaux 2014); and the expert-based assessment of political options in decision-making processes (Bueno de Mesquita 2011). While these approaches come with different merits and shortcomings, they all have to take into account events that are simultaneously "rare," "surprising," and "dramatic" (Schneider, Gleditsch, and Carey 2011) as they result from the emergent and endogenous dynamics in complex networks mentioned above.

The high degree of mathematization implied by these methods will certainly not alleviate all problems in conflict research. The fact remains that quantitative studies are generally too remote from causal processes, as we observed above. Sometimes "big data" will allow us to witness causality unfolding step by step. Yet the individual case might become ever more invisible within increasingly larger quantities of data. Researchers are advised to spend more attention on individual conflict trajectories to uncover the actual mechanisms at work. This suggests a meaningful integration of qualitative methods, such as process tracing, into quantitative research designs. As Cederman and Vogt (2017, 17) observe, it is all about balance as "conflict scholars will have to steer a middle course between overgeneralized macromodels and myopic microinvestigations." The close integration of within-case and cross-case inference in multimethod designs might provide a way forward (Humphreys and Jacobs 2015; Goertz 2017).

Our look back on one and a half decades of conflict research has carved out a set of consensus variables potentially linked to the onset and incidence of violent intrastate conflicts. At the same time, it has laid bare the limits of the quantitative regression approach. In spite of the tremendous efforts, we are still far from a substantive and widely accepted understanding of the causes and dynamics of intrastate conflict. The list of consensus variables presented here does not reach far beyond common sense. Still, there is no reason for pessimism. The field is characterized by a burgeoning number of innovative approaches promising significant advances.

We can trust the ingenuity of the researchers to push ever deeper toward a more complete picture of intrastate conflict.

Acknowledgements

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