Power-sharing: Institutions, Behavior, and Peace*

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Abstract

Grievances that derive from the unequal treatment of ethnic groups are a key motivation for civil war. Ethnic power-sharing should therefore reduce the risk of internal conflict. Yet conflict researchers disagree on whether formal power-sharing institutions effectively prevent large-scale violence.We can improve our understanding of the effect of power-sharing institutions by analyzing the mechanisms under which they operate. To this effect, we compare the direct effect of formal power-sharing institutions on peace with their indirect effect through power-sharing behavior. Combining data on inclusive and territorially dispersive institutions with information on power-sharing behavior, we empirically assess this relationship on a global scale. Our causal mediation analysis reveals that formal power-sharing institutions affect the probability of ethnic conflict onset mostly through power-sharing behavior that these institutions induce.

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Power-sharing institutions are a commonly prescribed means of fostering peace in ethnically divided and otherwise fragile societies. They are designed to provide political elites with incentives to share power, within the central government or among federal units. In short, power-sharing institutions should induce behavior that makes civil war less likely. Yet, the existing literature on power-sharing has produced inconclusive or even contradictory results. Some empirical analyses find that particular forms of power-sharing reduce the risk of violent conflict, but they do not agree which types of power-sharing prove effective (e.g., Hartzell & Hoddie 2003, Jarstad & Nilsson 2008, Mattes & Savun 2009). Other prominent studies present evidence that power-sharing is either irrelevant or even detrimental (e.g., Roeder 2005, Toft 2010, Selway & Templeman 2012).

We argue that these contradictory findings result from paying insufficient attention to the mechanisms through which formal power-sharing institutions affect the likelihood of civil conflict onset. Conflict researchers generally assume that formal power-sharing institutions induce cooperative behavior among political elites, for example in coalition governments, thereby increasing the likelihood of peace. Yet, existing studies rarely specify this causal chain and instead directly link institutions to armed conflict. The causal chain from institutions to peace, nonetheless, may be disrupted in three ways. First, formal power-sharing institutions may fail to induce the expected behavior. Second, even if institutions generate the expected behavior, this behavior may not increase the likelihood of peace. Third, formal power-sharing may affect the likelihood of peace independently of behavioral practices, for example, by influencing expectations about future cooperation. If any of these conditions apply, ignoring how formal power-sharing institutions affect power-sharing practices is likely to lead to inconclusive results.

In this paper, we examine the causal chain from power-sharing institutions to peace through power-sharing behavior. More specifically, we estimate the effect of *de jure* power-sharing on power-sharing practices and their respective effects on conflict onset.[1] Based on these estimations, we employ causal mediation analysis that allows us to assess whether formal power-sharing institutions influence the likelihood of civil war onset indirectly *through* power-sharing practices, or whether they exert an independent effect.

Our empirical findings suggest that particular forms of formal power-sharing institutions affect particular behavioral practices, and thereby particular types of conflict. For example, we find that inclusive power-sharing institutions that enable diverse groups to gain access to government do indeed make governments more ethnically inclusive, and that this practice of inclusion reduces the likelihood of rebellions by excluded groups, but this comes at the cost of raising the odds for infighting. By contrast, we find less evidence that formal power-sharing institutions affect the likelihood of conflict through other means. Thus, by untangling the sometimes offsetting relationships among specific types of power-sharing institutions, various power-sharing practices, and different types of civil war, our findings reconcile many of the conflicting claims about the effects of power-sharing.

The next section briefly reviews the contradictory claims about the effects of power-sharing on peace. We then discuss how formal institutions affect the likelihood of civil war onset through their effect on power-sharing practices. After

[1]We also denote institutions as power-sharing *rules* or *de jure* power-sharing, and similarly refer to behavior as power-sharing *practices* and *de facto* power-sharing.

introducing our data, we present the empirical tests and our findings regarding the effects of Lijphart's (2002, 39) "primary characteristics" of power-sharing, namely power-sharing at the executive level (inclusion) and segmental autonomy (dispersion). We conclude by discussing future steps in our research on formal and informal power-sharing.

The literature on power-sharing and conflict

Much of the literature on power-sharing pays tribute to Lijphart's (1969, 1975) notion of consociationalism, which responded to scholars who questioned the viability of democracy and stability in "plural societies" (e.g., Dahl 1971, Rabushka & Shepsle 1972). Lijphart argued that the combination of a grand coalition, mutual veto rights, segmental autonomy and proportionality should allow for the peaceful coexistence of distinct social groups (see most recently, Martin 2013).[2]

Lijphart's (1969, 1975) early writings conceived of consociationalism largely as a set of behavioral practices of political elites (Andeweg 2000). For instance, grand coalitions as practiced in Switzerland are not formally prescribed by the constitution. Lijphart (1985, 158) himself noted that "[t]here is also a general difference . . . between laying down the basic rules of power-sharing in formal documents – such as constitutions, laws, or semi-public agreements – and relying on merely informal and unwritten agreements and understandings among the leaders of the segments." Later, Lijphart and others turned their attention to formal power-sharing institutions and their impact on outcomes such as conflict, and economic and social performance (see, e.g., Lijphart 1999).

[2]Binningsbø (2013) provides an excellent review of this literature.

This focus on formal institutions is at least partially rooted in the desire to offer policy advice and the belief that institutions are often the most suitable means (Lijphart 1985). If power-sharing practices have positive effects and can be induced by power-sharing institutions, then scholars should try to understand what institutions are most conducive to such practices. Yet most recent studies of power-sharing institutions leapfrog the link between institutions and practices such as elite cooperation in coalitions. Instead, conflict researchers usually evaluate a direct link from institutions to armed conflict, which implicitly assumes that power-sharing behavior is affected by institutions and this in turn affects the likelihood of fighting.

The lack of attention to power-sharing behavior is evident in the literature on intrastate conflict. Research on civil war onset tends to focus on the impact of electoral rules, parliamentary or presidential regimes, and the design of federal structures (e.g., Cohen 1997, Reynal-Querol 2002, Roeder 2005, Schneider & Wiesehomeier 2008, Selway & Templeman 2012). In contrast, studies of conflict recurrence spearheaded by Hartzell & Hoddie (2003) usually examine the individual or joint effects of a mix of political, military, and territorial power-sharing provisions in peace agreements (see Walter 2002, Mukherjee 2006, Hartzell & Hoddie 2007, Jarstad & Nilsson 2008, Jarstad 2009, Mattes & Savun 2009, Mehler 2009, Martin 2013). Partly due to different operationalizations of power-sharing, scholars have yet to reach consensus on its actual effects. Some scholars even argue that power-sharing institutions endanger peace by generating incentives for ethnic outbidding and intransigence (Roeder 2005, Selway & Templeman 2012). Many of these studies only examine the pacification effect on countries that have experienced conflict.

In order to reach more definitive insights concerning the effects of power-

sharing institutions on civil peace, we argue that three issues deserve particular attention. First, it is necessary to understand the causal chain from institutions to peace or conflict outcomes through behavior. Although existing research notes the difference between *de jure* power-sharing provisions in peace agreements and their *de facto* implementation (Hoddie & Hartzell 2003, Jarstad & Nilsson 2008, Ottmann & Vüllers 2015, Strøm, Gates, Graham & Strand 2017), our empirical understanding of the latter is still underdeveloped. For example, Jarstad & Nilsson (2008, 215) find that about 75 % of political pacts contained in peace agreements are implemented, but this percentage drops to 55 % for territorial pacts, and 34.5 % for military pacts. Yet, most studies of civil war onset and recurrence do not consider whether power-sharing rules result in cooperative behavior (e.g., Reynal-Querol 2002, Hoddie & Hartzell 2003).

A second issue that could explain contradictory empirical findings concerns the possibility that different kinds of power-sharing institutions may have different effects on civil peace. Gates, Graham, Lupu, Strand & Strøm (2016) find that only constraining power-sharing institutions significantly and robustly enhance civil peace, whereas inclusive institutions have beneficial effects only in societies that have recently undergone civil conflict. When evaluating institutional effects we need to compare different types of power-sharing institutions. Finally, our insights concerning these institutions and their effects should be tested against a sufficiently broad and representative set of data. This is important because existing results might be unduly influenced by cases, such as post-conflict environments, that pose particularly difficult challenges for power-sharing institutions.

We address these three points in a global study of both pre-conflict and post-conflict cases with Strøm et al.'s (2017) data on inclusive and dispersive power-sharing institutions and Cederman, Wimmer & Min's (2010) data on power-

sharing practices. We thereby tackle a critical source of the conflicting evidence regarding the effects of power-sharing on conflict: the lack of attention paid to actual power-sharing behavior or practices, which arguably stems from the difficulty of collecting such data.[3]

The lack of attention paid to behavioral practices obscures the causal mechanisms that link institutions to peace and conflict outcomes. Formal power-sharing institutions might fail to reduce the likelihood of civil war either because they fail to induce power-sharing behavior, as the failed 1991 peace agreement in Angola attests to (Doyle & Sambanis 2006, 3), or because the practices have no conflict-reducing effect, as for example in Lebanon where the grand coalition between Sunni, Shia, and Maronite Christians could not prevent civil war in 1975 (Makdisi & Sadaka 2005, 61–63). Conversely, peace might result from power-sharing practices induced by institutions or from direct institutional effects, which, for example, operate through increased citizen confidence or expectations of future implementation (see Hale 2008). It also remains difficult to assess the effects of power-sharing behavior that occurs in the absence of formal power-sharing rules – a not uncommon constellation.

On top of these challenges, studies of conflict recurrence fail to analyze multiple cases of power-sharing in ethnically divided states such as Belgium that have no history of armed conflict since 1945 (Lijphart 1977, 15). Similarly, states that have experienced recent intrastate wars sometimes engage in power-sharing practices that are unrelated to the past conflict issue, such as the Nigerian arrangement since 1999 of alternating the presidency between northerners and southern-

[3]Strøm et al. (2017) also provide some information on whether formal institutions were implemented.

ers (Ibrahim 2007, 6). Thus, to account for these forms of accommodation we must examine power-sharing practices globally and include practices that emerge independently of peace agreements or other mandates.

Due to the lack of information on *de facto* power-sharing behavior, almost all existing studies on power-sharing fail to trace the mechanisms that link power-sharing institutions to civil war. A recent study by Cederman, Gleditsch & Buhaug (2013) has begun to examine ethnic power-sharing behavior, but this work has correspondingly paid less attention to formal institutions. By merging these data on elite behavior with the previously mentioned data on power-sharing institutions, we can explore in finer detail the causal pathway from power-sharing institutions through behavior on the risk of ethnic civil war.

Power-sharing institutions and practices

We expect formal power-sharing institutions to affect conflict in large part through their effects on practices of power-sharing between relevant ethnic groups. Indeed, when scholars argue that formal power-sharing induces peace, it is primarily such indirect or mediated effects they have in mind. However, formal power-sharing institutions may also affect peace in other ways, for example, by altering expectations about the future. If formal rules that promise future reserved legislative seats for minority groups have been adopted, the expectation of future inclusion may discourage armed rebellion, even if elections under those rules have yet to take place and the minority group currently has no representation.[4] Future ex-

[4]For a related argument about promises of autonomy and secession in the former Soviet Union, see Hale (2008, Ch.4).

pectations are particularly important during transitions when new formal rules have not yet been implemented, or when rules previously in force have been temporarily suspended, such as during a period of martial law. While we maintain that the main effect of power-sharing institutions is mediated by practices, we recognize that other effects, not mediated by current practices, may exist.

Which power-sharing institutions and practices are most effective in reducing the likelihood of conflict? To answer this question, we use Strøm et al.'s (2017) conceptualization and focus on those elements of power-sharing that Lijphart (2002, 39) considers as "primary characteristics" (see also Lijphart 1995, 856): the sharing of executive power and group autonomy. These two features closely resemble two dimensions proposed by Strøm et al.'s (2017) conceptualization, namely inclusive and dispersive power-sharing.[5] Thus, *inclusive* institutional arrangements mandate the participation of several parties or groups in particular offices or decision-making processes. Closely aligned with core elements of Lijphart's (1969, 1975) consociationalism, they range from reserved legislative or executive positions for representatives of minority groups to mandates of military inclusiveness. *Dispersive* power-sharing institutions distribute power by decentralizing decisions across regions or sectors of society, such as federalism. They delegate power away from the central government toward regional authorities and

[5]We focus on these "primary characteristics" and adopt a narrow definition of their institutional elements to render our theoretical argument as precise and explicit as possible. We explicitly do not consider such institutions as a proportional representation electoral system or a parliamentary regime, as the former is not part of the "primary characteristics" and as both are only remotely related to Lijphart's (1969, 1975) elements ("proportionality" and "grand coalition").

guarantee the autonomy of these governments as well as their accountability to regional constituencies (see Gurr, Harff, Marshall & Scarritt 1993, Hechter 2000).

In the following, we identify the specific power-sharing behavior we expect each type of institution to induce. Together with the effect of this behavior on conflict they determine the indirect effect of the institution on conflict. We refer to other mechanisms between institutions and conflict as *direct effects* but do not model their specific content (see Imai, Keele, Tingley & Yamamoto 2011, 769). Figure 1 summarizes the direct and the indirect effects of institutions on conflict, which we discuss in more detail below.

Figure 1 about here.

Inclusive power-sharing institutions. The purpose of inclusive power-sharing institutions is to enable diverse ethnic groups, particularly minorities, to gain access to government power. Absent such power-sharing institutions, these groups might not be adequately represented and therefore lack a say over policies that affect them. Exclusion along ethnic lines triggered armed conflicts in Apartheid South Africa, Assad's Syria, Amhara-dominated Ethiopia before 1991, and racially divided Guatemala (Cederman, Gleditsch & Buhaug 2013, 63&82). Often, inclusive power-sharing institutions explicitly require particular ethnic groups to be represented in government, for example in Bosnia and Lebanon. Yet it is not uncommon to find only general rules, for instance if minority parties have to be included in the government even if they are not required to form a majority. Empirically, we expect to see a larger share of ethnic groups represented in government in states with inclusive institutions that prescribe and implement power-sharing behavior. Figure 1 depicts both this indirect effect and the direct impact of inclusive institutions on the likelihood of conflict. Whereas Gates et al.

(2016) find no evidence for an unmediated negative effect of inclusive institutions on civil conflict, little is known about the indirect causal pathway through power-sharing behavior. Although Cederman, Wimmer & Min (2010) find evidence that inclusive practices reduce conflict, they also consider a large number of power-sharing practices that exist independently of formal institutions (see also Cederman, Gleditsch & Buhaug 2013). It thus remains unclear whether institutions are really responsible for the lower risk of conflict.

Hypothesis 1 (H1)**.** *Inclusive power-sharing institutions decrease the likelihood of conflict onset by making governments ethnically more inclusive.*

While this general hypothesis follows quite directly from recent research on political inclusion and Lijphart's (1969, 1975) path-breaking work, some of this research also shows that more inclusive arrangements may heighten the tensions within the government coalition. Wimmer, Cederman & Min (2009) distinguish between civil wars among partners in power-sharing arrangements, i.e. infighting as for example in Lebanon (Makdisi & Sadaka 2005), and those conflicts in which an excluded group fights against the state. Roessler (2011, 2016) explores this dynamic in Sudan and other Sub-Saharan African cases and argues that power-sharing induces a commitment problem between individual coalition members, who each want to rule alone. Put differently, power-sharing in the present does not rule out defections by one of the power-sharing partners in the future (also see Walter 2002, Dal Bó & Powell 2009). Lebanon's extensive but failed power-sharing regime between Maronites, Sunni, and Shi'a provides one tragic example of infighting. Hence, we refine our first hypothesis to capture this more nuanced effect of inclusive power-sharing and propose the two following sub-hypotheses:

Hypothesis (H1a)**.** *Inclusive power-sharing institutions decrease the likelihood*

of conflict onset between the government and politically excluded groups by making governments more ethnically inclusive.

Hypothesis (H1b)**.** *Inclusive power-sharing institutions increase the likelihood of conflict onset among the power-sharing partners by increasing the number of ethnic groups in power.*

Dispersive power-sharing institutions. Dispersive power-sharing institutions increase the policy authority of subnational governments, as well as their accountability to subnational constituencies. While dispersive institutions have important effects on governance even in mono-ethnic societies, we here focus specifically on the effect of dispersive institutions on regional autonomy for ethnic groups. Because ethnic groups are frequently regionally concentrated, regional governments in multiethnic societies are likely to be controlled by specific ethnic groups (e.g., Christin & Hug 2012). Thus, empowering such regional governments and devolving particular important policy authorities to the subnational level make it likely that ethnic groups profit and gain regional autonomy. By gaining political power through dispersive institutions, ethnic groups such as the Acehnese in Indonesia should be less likely to rebel against the state (Cederman, Hug, Schädel & Wucherpfennig 2015).

Hypothesis 2 (H2)**.** *Dispersive power-sharing institutions, by increasing regional autonomy for ethnic minorities, decrease the likelihood of conflict onset.*

Data and Method

To test our hypotheses, we combine two recent datasets that provide information on inclusive and dispersive institutions as well as power-sharing behavior. The

Inclusion, Dispersion, and Constraints (IDC) dataset provides information on *de jure* power-sharing institutions (Strøm et al. 2017), while the 2014 version of the *Ethnic Power Relations* (EPR) dataset captures information about the *de facto* allocation of power among ethnic groups within each state (Cederman, Wimmer & Min 2010, Vogt, Bormann, Ruegger, Cederman, Hunziker & Girardin 2015).

The IDC dataset offers annual data on 180 countries from 1975 through 2010.[6] This broad coverage allows us to assess the effect of power-sharing institutions in a global sample that includes states with and without a history of ethnic conflicts, thus avoiding potential selection problems due to a focus on post-conflict situations.[7] The dataset contains nineteen indicators of power-sharing, each of which is associated with one of the three dimensions of power-sharing that Strøm et al. (2017) identify theoretically: inclusive, dispersive, and constraining.

Because we follow Lijphart in focusing on inclusive and dispersive power-sharing, we focus on the eleven corresponding indicators. The measure of *inclusive* power-sharing thus incorporates two of Lijphart's (1969) components of consociationalism: grand coalitions and mutual veto. It also includes reserved seats or executive positions for specific minority groups in the central government and inclusiveness mandates for the armed forces.[8] The indicators of *dispersive*

[6]All independent states with populations greater than 250,000 are included.

[7]This feature of the IDC is a decisive advantage as most other studies draw inferences about the effects of power-sharing institutions by using information from peace-agreements. The latter, however, are only adopted in post-war settings, making broader inferences impossible. As other possible sources of selection bias are possible, we will discuss these more in detail below.

[8]The indicators for inclusive institutions are "Mandated Grand Coalition or

power-sharing cover three areas: (1) the powers allocated to sub-national governments; (2) whether sub-national governments are directly elected; and (3) the representation of sub-national constituencies in the upper house of the national legislature. The powers of sub-national government are coded based on whether state/provincial governments have the ability to levy their own taxes; whether state/provincial governments have control over education policy;[9] and whether subnational governments control their own police/paramilitary forces.[10] Based on factor analysis, the indicators combine into two indices – one for inclusive and one for dispersive power-sharing.[11]

Unity Government," "Mutual Veto," "Reserved Executive Positions," "Reserved Seats", "Mandated Military Inclusiveness."

[9]Education policy is of central concern with respect to powersharing because this single issue area contains service provision and opportunities for patronage, human capital development and the distribution of economic opportunity, as well as control of cultural, historical, and political narratives that are often central to political identities in multi-ethnic societies (Graham & Strøm 2014).

[10]The indicators for dispersive institutions are "Subnational Tax Authority," "Subnational Education Authority," "Subnational Police Authority," "State/Provincial Executive Elections," "State/Provincial Legislative Elections," and"Constituency Alignment (i.e. state/provincial representation in the upper house."

[11]More specifically, Strøm et al. (2017) conduct a factor analysis of indicators of power-sharing institutions and find that these indicators indeed cluster cleanly around three clearly interpretable latent variables: (1) *inclusive*,

We measure power-sharing behavior by drawing on the EPR data, which comprises information about ethnic groups in all states where ethnicity is relevant in national politics.[12] The EPR dataset provides information on political inclusion of group representatives into the highest executive body of each state, codes *de facto* regional autonomy, and provides data on group sizes relative to the ethnically relevant population.[13]

The EPR data categorizes power-access at the center for ethnic groups according to seven categories: the monopoly and dominant categories describe regimes in which representatives from one ethnic group rule alone. Representatives of groups with a senior- and junior-partner coding share power in multi-ethnic coalitions. All other groups, namely those considered to be powerless, discriminated or self-excluded, are considered as being excluded from executive power.[14] Members of powerless groups simply do not enjoy inclusion at the center, while discrimi-

(2) *dispersive*, and (3) *constraining* power-sharing. For details on index creation and the underlying indicators, see the IDC Codebook, available online at `http://dx.doi.org/10.7910/DVN/27961`.

[12]The EPR codebook notes that "[an] ethnic group is considered politically relevant if at least one political organization claims to represent it in national politics..." If no actor makes such claims, ethnic groups can still be in the data if the state politically discriminates against them such as in Apartheid South-Africa or in southern states in the United States under the Jim Crow regime (see `http://www.icr.ethz.ch/data/epr`).

[13]Token membership by ethnic elites who cannot or do not effectively represent a group does not qualify for an "inclusion" coding.

[14]The self-exclusion category captures a small number of groups that control a

nated groups face active, political persecution by the state such as the denial of citizenship rights.

In addition, the EPR dataset also provides data on territorial autonomy regardless of whether ethnic groups enjoy inclusion or exclusion into the central executive (Cederman et al. 2015). An ethnic group has *de facto* regional autonomy when a "meaningful and active regional executive organ operates below the state level but above the local administration, and group representatives exert actual influence on the decisions of this entity, acting in line with the group's local interests" (Vogt et al. 2015, 1331).

Since the EPR dataset only offers information on power-sharing behavior if at least two politically relevant ethnic groups exist in a state, we drop all observations from countries where ethnicity is not relevant. Restricting the sample to those states where ethnicity plays an important role in politics is warranted for two reasons. First, it is in line with our theoretical focus on the effects of power-sharing institutions on ethnic conflict as mediated by respective power-sharing practices. Ethnic conflicts do not happen in countries in which there are no politically relevant ethnic groups.[15] Second, we do not lose many cases as

particular territory of the state, which they have declared independent from the central government.

[15] This obviously does not shield us from concerns raised by Hug (2010, 2013) that some ethnic groups might be "politically relevant," but, due to missing information, not be part of the EPR data (see also Weidmann 2015). These concerns, however, are unlikely to affect our results. Omitted "politically relevant" ethnic groups, or only socially relevant ethnic groups (see Birnir, Laitin, Wilkenfeld, Waguespack, Hultquist & Gurr 2018), would only increase the num-

EPR classifies ethnicity as politically relevant in 141 states out of an overall population of 165 countries with a population greater than 500,000 (Vogt et al. 2015, 1336).[16]

Our unit of analysis is the country-year for two main reasons. First, our data on power-sharing institutions are coded at the country level. Second, and more importantly, the alternative of identifying the observations by ethnic group is not feasible since not all power-sharing institutions formally apply to ethnic groups. For instance, the Constitution of Fiji of 1997 requires the prime minister to form a multiparty cabinet. Depending on the composition of the parliament, this may lead to ethnic groups being explicitly integrated in a government, though this is not explicitly required (Fraenkel 2006, 321). Thus, in assigning particular institutional provisions to particular ethnic groups would be fraught with difficulties. Country-year analysis is likely to lead to conservative estimates as not all ethnic groups (in the case of provisions targeting explicitly such groups) are likely to profit from power-sharing. Many extant studies likely also report conservative estimates of the effect of power-sharing as these studies predominantly adopt the same strategy.

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ber of observations from countries with no ethnic conflict and most likely no power-sharing institutions (as many of these target ethnic groups specifically). For the countries covered, however, the concerns of misclassificatons raised by Hug (2010, 2013) might be a problem. As we use a conflict coding based on low intensity levels (25 battle deaths), (Gleditsch, Wallensteen, Eriksson, Sollenberg & Strand 2002, Themnér & Wallensteen 2014, for more, see below) this is, however, unlikely to affect our results.

[16]Table A1 in the appendix lists all country-years covered in the analyses.

Our dependent variable armed conflict onset derives from the UCDP/PRIO Armed Conflict Database (ACD), which codes a new civil war when at least 25 battle-deaths have occurred during a calendar year (Gleditsch et al. 2002, Themnér & Wallensteen 2014). We thus employ a minimalist definition of peace, that is, the absence of civil war. In order to distinguish new civil wars from dormant conflicts, we code a new onset only if governments and rebels did not fight each other for two years.[17] We identify civil wars as ethnic whenever rebel groups claim to fight on behalf of and recruit from a specific ethnic group (Wucherpfennig, Metternich, Cederman & Gleditsch 2012).

To empirically capture power-sharing behavior, we aggregate *de facto* inclusion and territorial dispersion of power from the group-level EPR data to the country-level. We measure *de facto* central power-sharing as the share of a country's politically relevant groups included in the government if the government consists of at least two groups. This operationalization includes any power-sharing government that is multiethnic, and assigns a higher value to those governments where a greater share of the ethnic groups in their countries are represented. In contrast, any government that includes leaders from only one ethnic group receives a *de facto* inclusion score of 0 even if it represents a large part of the population such as the Erdogan government in Turkey, which represents many Turks, but not the Kurdish population.[18]

[17]We keep country-years with ongoing conflicts in the analysis as in the course of one conflict another civil war between the government and another non-state actor might still erupt. Dropping the observations of ongoing conflicts does not affect our substantial results.

[18]We have explored a series of other operationalizations, including a dummy

We proceed in a similar manner to measure *de facto* dispersive power-sharing. Rather than adding up the share of included groups, we aggregate the share of the population belonging to ethnic groups with regional autonomy. As the EPR data codes regional autonomy only for groups that do not enjoy dominance or monopoly power at the center, our operationalization reflects whether the center shares power with at least one other peripheral group.

Since we wish to assess both the direct and indirect effect of power-sharing institutions, we draw on Imai et al. (2011) and their framework for causal mediation analysis (for a related discussion, see Bullock, Green & Ha 2010).[19] Our goal is to assess our theoretical expectation that the effects of power-sharing institutions on civil war risk mainly run through power-sharing behavior. Mediation analysis allows us to do exactly that. In contrast, a commonly used alternative strategy, employing interaction effects between institutions and practices, would not allow us to assess our hypotheses, as the estimated coefficients would only tell us

indicator of whether one or more than one ethnic group is in government, a variable that corresponds to the population share of ethnic groups represented in government if they are at least two (and 0 otherwise), and finally simply the population share of ethnic groups represented in government. We also restricted our classification of power-sharing to those governments that only represent a majority of the population. The substantive conclusions from all these analyses are identical to the ones presented below.

[19]Bullock, Green & Ha's (2010) critique causal mediation analysis in experimental settings due to the untestable assumptions on which it builds. We address this criticism by employing sensitivity analysis developed by Imai et al. (2011) and present the result in our appendix.

whether one variable moderates the influence of another, but not through what channels institutions affect conflict. The institutional powersharing literature implicitly assumes that institutions, through practices, affect policy outcomes. Mediation analysis is necessary to evaluate whether this assumption is valid.

To test our hypotheses, we estimate two regressions: first, a linear model that assesses the effect of institutions on behavior, and second, a logit model that estimates the effect of institutions and behavior on ethnic conflict onset. Using these models we then carry out a causal mediation analysis to assess whether institutions influence conflict directly or indirectly through power-sharing behavior, or not at all.

The underlying logic of such an analysis for linear models is to draw on standardzed regressions coefficients to estimate the effects of the paths depicted in Figure 1 (see, Asher 1983). More specifically, to estimate the mediated effect of institutions on conflict, the product of the standardized coefficients for the effect of institutions on practices and for the effect of practices on conflict is used. This estimate of the mediated effect can be subtracted from the total effect (which corresponds to the standardized coefficient for the effect of institutions on conflict) to estimate the direct, i.e., unmediated, effect. Thus, in our case, causal mediation uses the estimated regression coefficients to partition the total effect of institutions on conflict into (1) the partial effect that is mediated through practices, and (2) a remaining effect, referred to as the direct effect.[20] According

[20]We follow Imai et al. (2011, 769) in our analysis and also define the average direct effect (ADE) as the difference in outcomes between cases where the treatment (institutions) is present and those where it is absent holding the mediator (behavior) constant. The average causal mediation effect (ACME) is the differ-

to Imai, Tingley & Yamamoto (2013, 7), who generalize mediation analysis to cover many non-linear models (which we employ), direct effects derive from "all other possible mechanisms," or simply all those not mediated by power-sharing practices.

Our selection of control variables follows standard practices in the literature. Among the most robust predictors of civil war are GDP per capita and a country's population size (Sambanis 2002). To account for time dependence, we consider the number of prior civil wars and a cubic polynomial of time since the last conflict (Carter & Signorino 2010). Moreover, we add dummies for French or British colonial legacy to partially mitigate endogeneity concerns (for the role of colonial heritage and civil wars, see also Blanton, Mason & Athow 2001). Existing research demonstrates that colonial heritage affects the design and type of country's political institutions and variation in colonial heritage to address endogeneity concerns (Christin & Hug 2012, Cederman et al. 2015, Wucherpfennig, Hunziker & Cederman 2016). Finally, we add an indicator variable for all cases in which some of the underlying institutional indicators are missing (Greene 2003, 60).

Empirical analysis

Our first analysis investigates the link among inclusive power-sharing institutions, inclusive power-sharing practices, and ethnic conflict onset. In the first column of Table 1, we report the estimated effects of inclusive institutions on *de facto*

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ence in outcomes given changes in the mediator holding the treatment constant. See Equations (1) and (2) in Imai et al. (2011).

inclusion – i.e. on the share of ethnic groups represented in the central government, based on a linear regression model. In the second column, we report the estimated effects of both inclusive institutions and *de facto* inclusion on conflict onset stemming from a probit model.

The results of the first model are as expected: inclusive institutions significantly increase the share of ethnic groups included in power-sharing governments. This is evidence that inclusive institutions work as they are designed to in at least one key respect – they make inclusive power-sharing behavior more likely and more encompassing. We also find that countries with a British or French colonial past are more likely to have a larger share of ethnic groups included in government, while in larger countries this is less the case. Results for the second model are less in line with our expectations. Here, our estimation generates only a small and statistically insignificant negative coefficient of *de facto* inclusion on ethnic war onset. While inclusive institutions induce power-sharing behavior, we do not find a similarly strong effect of power-sharing behavior on the likelihood of ethnic civil war. The estimated coefficient for the effect of power-sharing institutions on ethnic conflict turns out to be positive, though this coefficient is also small and statistically insignificant.

Table 1 about here.

We follow up the results in Table 1 with a causal mediation analysis, the results of which, namely the mediated maximum effect through behavior and the direct maximum effect of institutions, as well as the total effect of the latter, are shown in Figure 2. These maximum effects correspond to the differences between a situation where the institutional variable is set to its minimum and a situation where it set at its maximum. We find little evidence of any mediated effect of

inclusive institutions on ethnic civil war onset. Although, as predicted by our first hypothesis, the estimated average mediated effect (ACME) is negative, it is small and statistically insignificant. The estimated average direct effect (ADE) of inclusive power-sharing institutions is actually positive, i.e. it increases the risk of civil war, though this also fails to reach statistical significance.[21]

Figure 2 about here.

One possible explanation for this null effect of inclusive institutions on civil war is that there are actually two offsetting effects at play, and these combine to produce a null net effect. H1a and H1b take into account this possibility by postulating that inclusive institutions indeed have diverging effects on different types of ethnic conflict. Models 3 and 4 in Table 1 report the results of models that distinguish between governmental conflicts due to infighting among ethnic groups within a governing coalition and those pitting an excluded group against the government using the same empirical specification as above.[22] Thus, column three reports the estimated effects of inclusive institutions and *de facto* inclusion on fighting between ethnic groups that share power in government. Column four reports the results of this same regression, but instead looks at conflict between the government and ethnic groups outside of government as the dependent

[21]As the distribution of the variable of inclusive power-sharing institutions is severely skewed, we carried out the same analyses as above with a dichotomous indicator of whether at least one power-sharing element is present. The results lead to exactly the same substantive conclusions.

[22]For a similar analysis, focusing on authoritarian regimes in Sub-Saharan Africa, see Roessler (2011, 2016).

variable.

The results reported in Models 3 and 4 in Table 1 now reveal what our initial results based on a pooled conflict indicator could not: *de facto* inclusion increases the likelihood of one type of ethnic conflict while at the same time it reduces the likelihood of another type. In Model 3 we see that more inclusive power-sharing behavior, i.e. a larger share of ethnic groups represented in the central government increases the probability of civil conflict between coalition members. Conversely, in Model 4, we see that greater *de facto* inclusion leads to a lower incidence of conflicts between the government and excluded groups.[23] Thus, the larger the share of ethnic groups represented in an inclusive power-sharing arrangement, the likelier a conflict among these partners is. At the same time, more inclusion significantly decreases the likelihood of a conflict with excluded groups. Inclusive power-sharing practices do not significantly reduce the likelihood of ethnic conflict overall, but they do alter the nature of the conflict that occurs.

We depict the estimated effects resulting from the corresponding mediation analyses in Figure 3. The two panels of this figure show that inclusive institutions have no direct effect on ethnic conflict, but do have an indirect effect mediated by practices. Importantly, the direction of this effect depends on the type of conflict we consider, i.e. whether we focus on infighting or rebellions. These

[23]Strictly speaking the two models dealing with conflict among power-sharing partners and against excluded groups are obviously linked. For this reason we report in the appendix in Table C1 the results of a multinomial probit model (Imai & van Dyk 2005*a*, 2005*b*). While some small differences appear, the substantive insights regarding power-sharing remain the same. For this reason we retain the two probit models for the remainder of this study.

opposing effects suggest a trade-off and explain why we failed to uncover a relationship among inclusive institutions and practices and conflict onset in our first analysis, which did not distinguish between the two types of conflict. It is likely that previous studies of the link between inclusive power-sharing and civil war also reported inconclusive results due to these two opposing effects of inclusive institutions (see Roessler 2011, 2016).

Figure 3 about here.

Figure 4 about here.

Having identified two countervailing effects of inclusive power-sharing on civil war, we now assess the relative importance of these effects at the substantive level. In Figure 4 we report on the horizonal axis the values of our measure of inclusive power-sharing institutions, which stem, as discussed above, from a factor analysis. The lowest value, close to zero, corresponds to cases where none of the formal rules for inclusive power-sharing (discussed on page 12) exist. The hightest value, slightly above 7, corresponds to cases where all these rules are present. Figure 4 depicts the relevant information regarding the countervailing effects of inclusive power-sharing in two different ways. First, the mediated effect on the two types of conflict is reported as a function of the level of inclusive power-sharing institutions (left panel, 95 % confidence intervals depicted in grey). More precisely, instead of reporting only the maximum causal mediation effect, as in figure 3, we depict this latter effect for a continuum of changes in the "treatment," i.e., for different levels of inclusive institutions. Second, based on these causal mediation effects we generated changes in predicted probabilities (and their 95% confidence intervals, depicted in grey) of conflict onset (right panel). More specifically, for each observation we held all variables constant at

their sample values and only changed the predicted value of the mediator (i.e., power-sharing practices) due to the change in the treatment (i.e., power-sharing institutions).[24]

In both figures, we see that coalition infighting (solid line) and conflict with excluded groups (dotted line) are affected to a similar extent. For instance, if inclusive institutions were at their maximum, our model predicts that the probability of infighting in each country-year increases by 0.023 percentage points on average. The same change leads to a decrease in the average probability of a conflict against excluded groups by 0.017 percentage points. These changes also relate to the frequency of the two types of conflict, as the number of governmental conflicts among partners of power-sharing arrangements is small (15), while conflicts involving politically excluded groups are more numerous (35). Because governmental ethnic conflicts are fortunately rare, even these small changes in probabilities are substantively significant.[25]

[24]Thus, we calculated these probabilities by drawing on Gelman & Hill's (2007) proposal to generate "average predicted differences" in probabilities based on the sample values of the remaining covariates (see also Hanmer & Kalkan 2013). Consequently, we used the simulated mediated effects and combined these with the estimated coefficients of the conflict equation and let the degree of inclusiveness vary from its minimum to its maximum.

[25]As much of the literature on power-sharing and conflict deals with post-war settlements, we have also carried out analyses distinguishing between the effects in situations with no previous war and those with such previous conflicts. The direction of the effects in these two cases remain identical, however, we observe an attenuation effect in cases where no previous war had occurred (see appendix for

Table 2 about here.

Moving to our second hypothesis we assess whether dispersive power-sharing institutions increase the population of ethnic groups with regional autonomy and thereby decreases the likelihood of ethnic conflict onset. Employing the same empirical setup used in the test of H1 with a linear and a probit model, Models 5 and 7 in Table 2 show that dispersive institutions increase regional autonomy for ethnic groups. Contrary to inclusion, we find that British and French ex-colonies have smaller shares of the population of ethnic groups enjoying regional autonomy (see Cederman et al. 2015). Prior conflict in the country and the size of the country increase this share, while in richer countries the extent of regional autonomy is smaller. With respect to the effects of dispersive power-sharing on conflict (Models 6 and 8), we distinguish between all ethnic civil wars and those that are only fought over territory. For both outcomes, we find that lower ethnic regional autonomy implies a slightly lower risk of conflict onset. In contrast, the estimated coefficient for dispersive institutions is slightly positive for both types of conflict. Notably, all of these effects fail to reach statistical significance.

Figure 5 about here.

The more nuanced results from causal mediation analysis reveal that dispersive institutions decrease the risk of civil war indirectly through increasing the proportion of ethnic groups that enjoy regional autonomy. While the mediated effect of dispersive institutions on ethnic conflict onset in general is statistically

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the results). As these analyses focus on a much smaller set of cases and generate issues of quasi-complete separation, we refrain from exploring this interesting attenuation effect in more detail here.

significant, the estimated effect on ethnic territorial conflict barely misses significance. For both types of conflict, the direct effect is positive but statistically not significant (see Figure 5).[26]

Discussion

Our analyses provide considerable evidence that *de jure* power-sharing institutions affect the likelihood of ethnic conflicts by influencing civil war through power-sharing practices. Using causal mediation analysis, we find little support for formal institutions having an effect on conflict except through their impact on power-sharing practices. More specifically, if we consider *de jure* inclusive institutions, we can show that these affect power-sharing practices and thus increase, respectively decrease, the probability of infighting, respectively of conflicts against excluded groups. Also, when we look at the substantive effects of *de jure* dispersive institutions as mediated by practices, we find again that the probability of conflicts decreases.

These results hinge on two important assumptions. First, our analysis, as most observational studies, assumes conditional independence, which might be vi-

[26]Following up on Cederman et al.'s (2015) finding, we estimated these models again for cases with no previous war and those with previous conflict experiences. We find again similar results with an important exception, namely that dispersive institutions appear to affect positively the autonomy of ethnic groups only in cases with previous war experiences. The total effect of institutions on conflict remains, nevertheless, largely similar. For this reason we do not explore this peculiarity in more detail here and report the results in the appendix.

olated by the possible endogeneity of both power-sharing institutions and power-sharing practices. As in other studies we attempt to mitigate this problem in the main analysis by controlling for variables that are likely to influence both power-sharing institutions and practices, like colonial heritage (see Wucherpfennig, Hunziker & Cederman 2016). In the appendix we report on a series of analyses that include additional control variables, including a measure of democracy(note, however, works on autocratic power-sharing, see Gandhi 2008, Magaloni 2008, Svolik 2012), which might be linked to the presence of power-sharing institutions, that yield, however, largely similar results. To assess whether reverse causality might affect our results we also report in the appendix analyses for which we lagged our main dependent variables. Again our main findings remain intact.

As mediation analysis also allows for an explicit sensitivity analysis based on the correlation of the error terms of our two equations (one explaining practices, the other conflict), we also assess how large this correlation would need to be to lead us to reject our hypotheses. As our analyses in the appendix show, while some larger values for these correlations would make our results go away, the required signs of these correlations are much less plausible. This relates to our argument that if our results are biased, the biases should reduce the size of our coefficients and thus lead to weaker support for our hypotheses. First, we focus only on countries and years where power-sharing could effectively be deployed among ethnic groups. We do not consider how power-sharing institutions might also induce peace among actors who are separated by non-ethnic cleavages. Second, power-sharing institutions and practices are likely to be adopted especially in situations where tensions are so high that conflict is likely. Yet, if anything, this should tend to bias the results against our hypotheses, reducing the negative correlation between power-sharing institutions and subsequent civil war (evidence

for this mitigating effect appears in Cederman et al. 2015, Wucherpfennig, Hunziker & Cederman 2016).

The second assumption concerns the specification of the correct causal order. Most scholars would readily support our claim that power-sharing institutions affect conflict outcomes through power-sharing behavior. Others argue, however, that this relationship is far more complex because actors choose the institutional rules under which they govern. Pepinsky (2014), for example, explores this hypothesis with respect to power-sharing in dictatorships. We are not able to settle this debate here. Instead, we probe the causal order assumption empirically by reversing institutions and behavior in our main empirical models. More specifically, we re-estimate our models by considering power-sharing institutions as the mediator and power-sharing practices as the treatment for the analyses that underlie the causal mediation results depicted in Figures 4 (inclusive power-sharing) and 5 (dispersive power-sharing),[27] and present the results in Figure 6.

Figure 6 about here.

The four panels in Figure 6 display a striking commonality. Throughout, the average causal mediated effects are systematically small and the confidence intervals include zero. At the same time, the effects of power-sharing practices that are not mediated by formal institutions (direct effects in Figure 6) are systematically larger and the confidence intervals systematically exclude the value of zero, with the single (and only partial) exception of the effect of autonomy on territorial conflict. This suggests that the effect of power-sharing practices cannot be explained as being mediated by power-sharing institutions. Quite to

[27]We keep the exact same set of control variables.

the contrary, when we consider practices as being causally prior, the effects of these practices are almost entirely of a direct nature.

Substantively, we find that our estimated effects are in the expected direction. Inclusion increases the likelihood of infighting among power-sharing partners, while reducing the probability that an excluded group engages in a conflict. This finding to some extent probably explains some of the mixed results regarding the pacifying effects of power sharing. Finally, regional autonomy granted to groups excluded from central power-sharing decreases the likelihood of ethnic conflict in general and territorial conflict in particular, but the latter effect is not statistically significant.

Conclusion

Our results do much to clarify previously conflicting findings in the literature. In particular, our nuanced analysis of *de jure* inclusive power-sharing institutions reveals that these institutions, working primarily through inclusion of excluded groups, reduce the effects of conflict against excluded groups, but simultaneously increase the risk of fighting within government coalitions. Previous research generally did not distinguish between these two types of conflict. The failure to detect that the associated effects offset one another has led scholars to disagree about the effect of inclusive power-sharing on civil war onset (Lijphart 1977, Selway & Templeman 2012). Finally, our results demonstrate that *de jure* territorially dispersive power-sharing institutions have only weak effects on ethnic conflict, though there is some evidence that dispersion may decrease the likelihood of ethnic conflict in general, and territorial conflicts in particular, by increasing the population share of ethnic groups profiting from regional autonomy.

This research takes an important step toward identifying the mechanisms through which *de jure* power-sharing institutions affect conflict – something of substantive importance to practitioners seeking to induce peace in conflict-riven societies. Our findings are generally optimistic about the pacifying effect of power-sharing institutions.. Thus, while we demonstrate that power-sharing predominantly affects the likelihood of conflict through practices, establishing the conditions under which these effects are most pronounced is beyond the scope of this paper.

Also, research remains to be done on how political accomodation induces different social actors to behave peacefully. In the main, the institutions and practices examined in this paper directly benefit the leaders of vulnerable ethnic groups, offering them political influence, perquisites, and mutual security. But civil peace also depends on whether ordinary citizens in these (and other) groups gain protection from repression and exploitation. When such protection is on offer, otherwise vulnerable individuals are less likely to join or support insurgency movements, and the leaders of such movements will find it more costly to take up arms (Gates et al. 2016). Consistent with Gates et al. (2016), we find no significant direct effects for the two forms of institutional power-sharing examined here. The conditions under which power-sharing accords offer such protection to ordinary citizens in vulnerable groups are therefore a promising avenue for further research.

Future empirical research could also most likely profit from a disaggregated perspective by focusing on ethnic-group-years as units of analysis. As our analysis of inclusive institutions shows, however, this also raises considerable issues of interdependence. Wucherpfennig (2011) shows that power-sharing offers may well be made strategically to a subset of groups. How institutions affect this

strategic behavior is an important topic for future research. The implication for institutional design is profound.

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Tables

Table 1: Inclusive Institutions and Ethnic Conflict

	(1)	(2)	(3)	(4)
	Inclusion	All Ethnic Conflicts	Infighting	Conflict Against Excluded Group
Inclusive Institutions	0.090*	0.023	0.018	0.033
	(0.007)	(0.051)	(0.083)	(0.088)
De Facto Inclusion		−0.023	1.138*	−0.618*
		(0.134)	(0.344)	(0.222)
Log(GDP Per Capita)	−0.023	−0.294*	−0.446	−0.369*
	(0.012)	(0.100)	(0.246)	(0.162)
Log(Population)	−0.058*	0.209*	−0.201	−0.091
	(0.010)	(0.080)	(0.248)	(0.128)
Prior Conflicts	−0.004	0.076*	0.075	0.009
	(0.003)	(0.014)	(0.056)	(0.028)
British Colony	0.204*	−0.198	−4.207	−0.161
	(0.015)	(0.126)	(202.772)	(0.213)
French Colony	0.340*	0.083	−0.566*	0.318
	(0.016)	(0.128)	(0.282)	(0.173)
Institution missing data	−0.069*	0.119	0.049	0.135
	(0.012)	(0.096)	(0.261)	(0.152)
Peace Years	0.008*	−0.018	0.023	−0.024
	(0.002)	(0.020)	(0.049)	(0.030)
Peace Years^2	−0.000*	0.001	−0.000	0.001
	(0.000)	(0.001)	(0.002)	(0.002)
Peace Years^3	0.000*	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.707*	−2.507*	−0.505	−0.381
	(0.084)	(0.691)	(1.934)	(1.091)
N	4,104	4,104	4,104	4,104
R^2	0.188			
adj. R^2	0.186			
Residual SD	0.348			
AIC		953.724	154.862	368.822
BIC		1257.071	458.208	672.168
ℓ		−428.862	−29.431	−136.411

Note: Standard errors in parentheses. * indicates significance at $p < 0.05$

Table 2: Dispersive Institutions and Ethnic Conflict Onset

	(5)	(6)	(7)	(8)
Outcome Variable:	Autonomy	Ethnic Conflict	Autonomy	Territorial Conflict
Dispersive Institutions	0.009*	0.063	0.009*	0.096
	(0.001)	(0.063)	(0.001)	(0.080)
Autonomous Pop. Share		−1.277		−1.004
		(0.744)		(0.821)
Log(GDP per capita)	−0.008*	−0.291*	−0.005*	−0.255
	(0.002)	(0.103)	(0.002)	(0.134)
Log(Population)	0.013*	0.214*	0.013*	0.385*
	(0.002)	(0.086)	(0.002)	(0.108)
British Colony	−0.013*	−0.277*	−0.013*	−0.145
	(0.003)	(0.128)	(0.003)	(0.155)
French Colony	−0.010*	0.037	−0.010*	0.033
	(0.003)	(0.122)	(0.003)	(0.181)
Prior Conflicts	0.005*	0.081*	0.004*	0.056*
	(0.001)	(0.014)	(0.001)	(0.016)
Peace Years	0.001*	−0.002	0.001	−0.025
	(0.000)	(0.020)	(0.001)	(0.025)
Peace Years^2	−0.000	−0.000	−0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.001)
Peace Years^3	0.000	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Institution missing data	−0.001	0.138	−0.001	0.104
	(0.002)	(0.099)	(0.002)	(0.129)
Constant	−0.059*	−2.531*	−0.069*	−3.802*
	(0.018)	(0.740)	(0.018)	(0.976)
N	4,104	4,104	4,104	4,104
R^2	0.098		0.090	
adj. R^2	0.096		0.088	
Residual SD	0.067		0.067	
AIC		945.977		590.760
BIC		1249.324		894.106
ℓ		−424.989		−247.380

Note: Standard errors in parentheses. * indicates significance at $p < 0.05$

Figures

Figure 1: Direct and Indirect Effects of Power-Sharing Institutions.

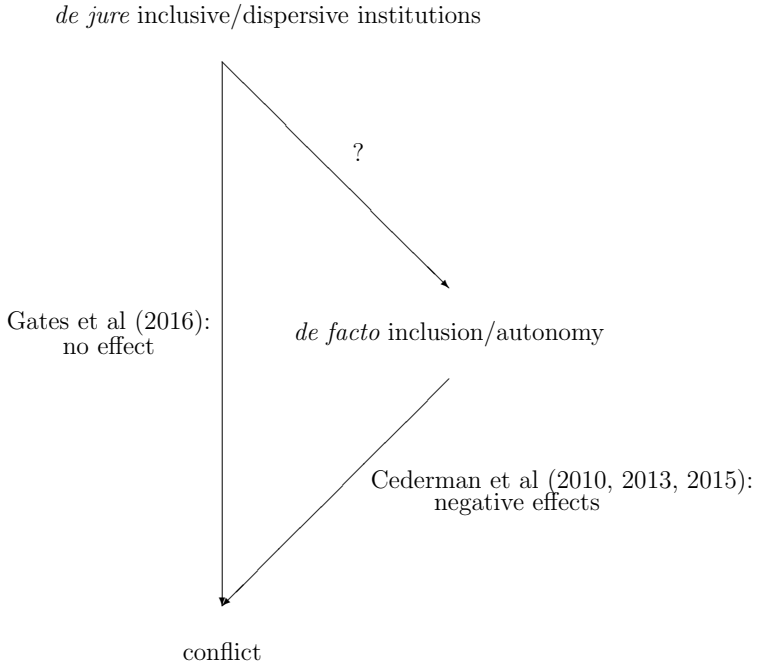


Figure 2: Maximum Effects of Inclusive Power-Sharing Institutions (Mediated by the Share of Ethnic Groups in Power-Sharing Coalitions) on Ethnic Conflict Onset

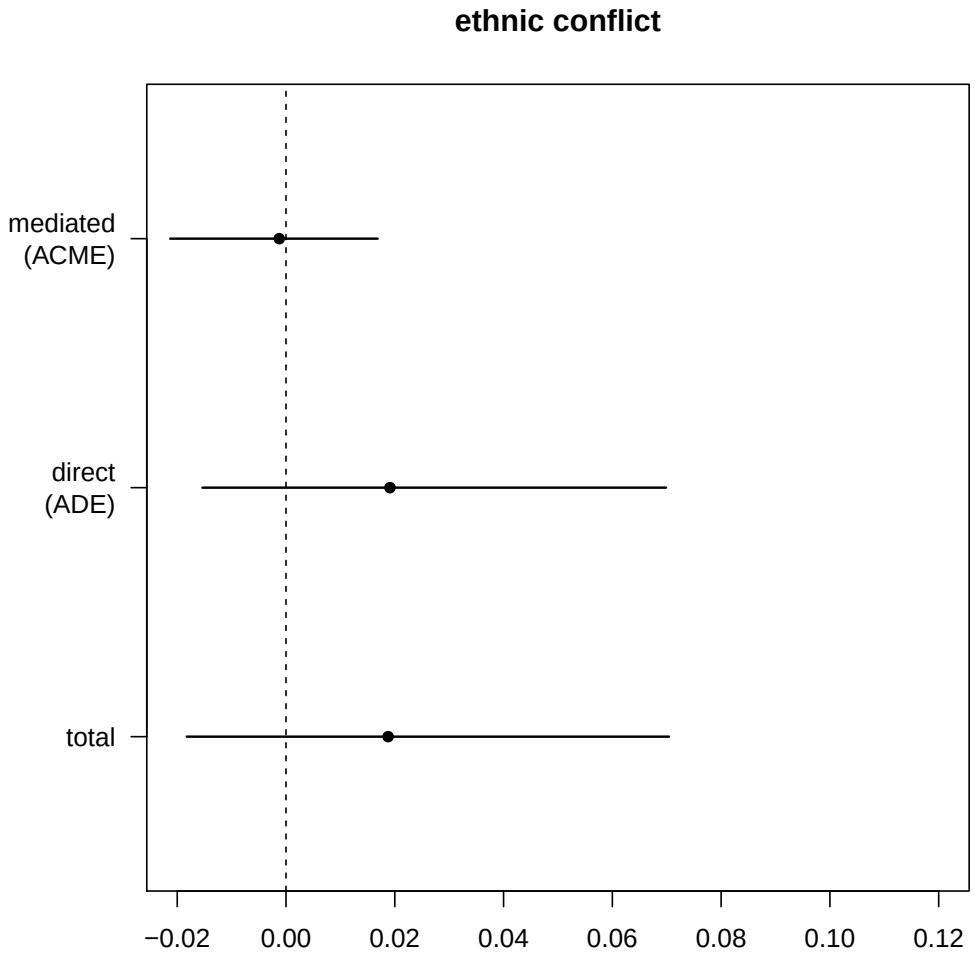


Figure 3: Maximum Effects of Inclusive Power-Sharing Institutions (Mediated by the Share of Ethnic Groups in Power-Sharing Coalitions) on Ethnic Conflict Onset

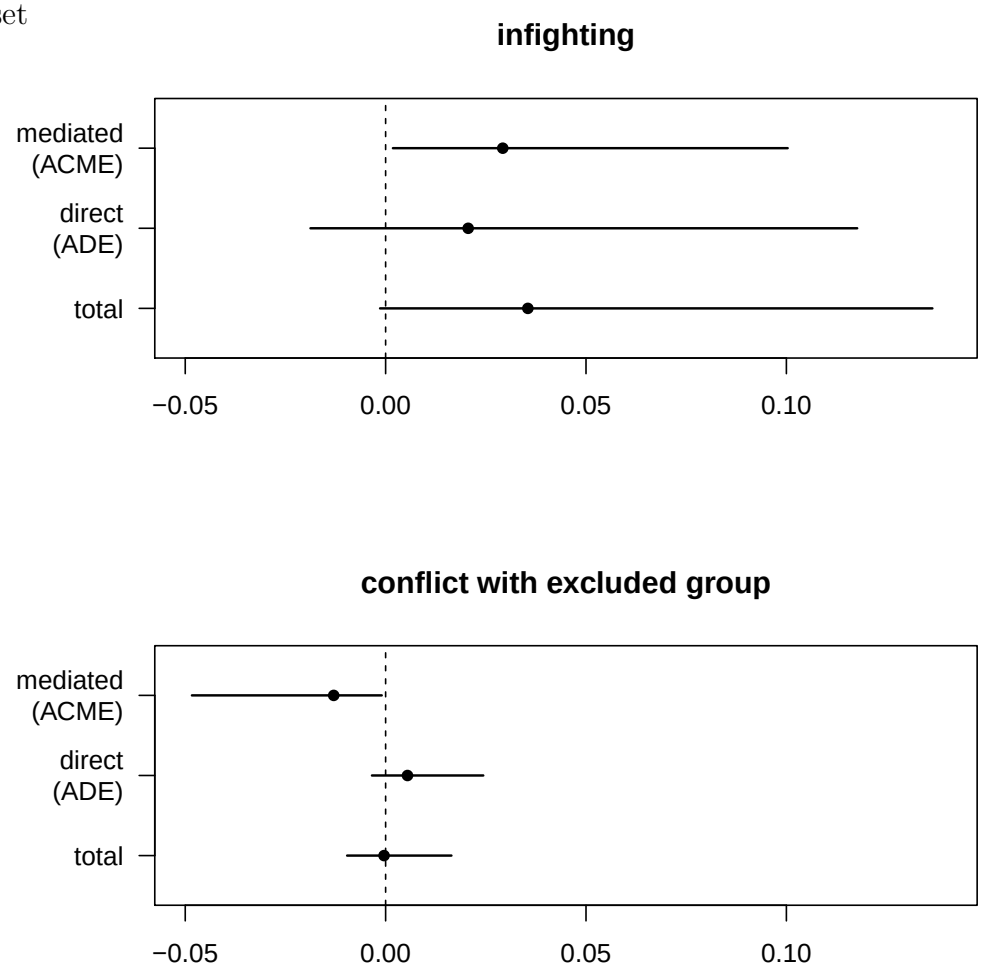


Figure 4: Effects of Inclusive Power-Sharing Institutions (Mediated by the Share of Ethnic Groups in Power-Sharing Coalitions) on Ethnic Conflict Onset with 95% Confidence Intervals

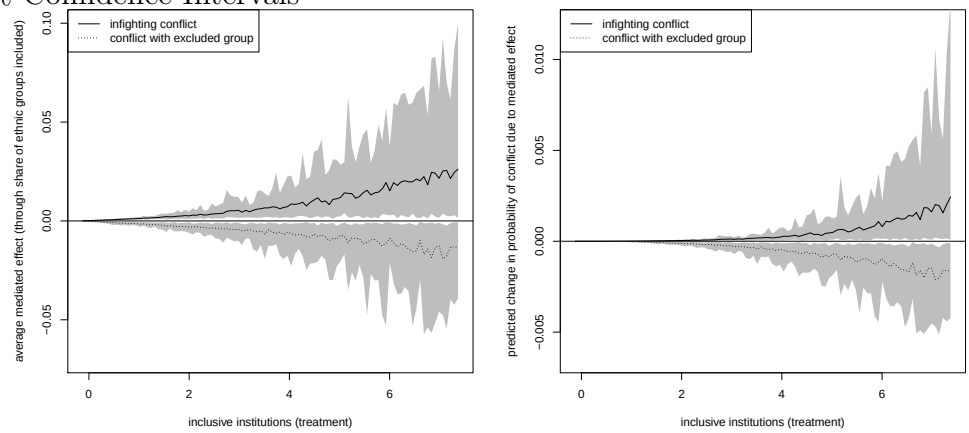


Figure 5: Maximum Effects of Dispersive Power-Sharing Institutions (Mediated by Population Share of Autonomous Ethnic Groups) on Ethnic Conflict Onset

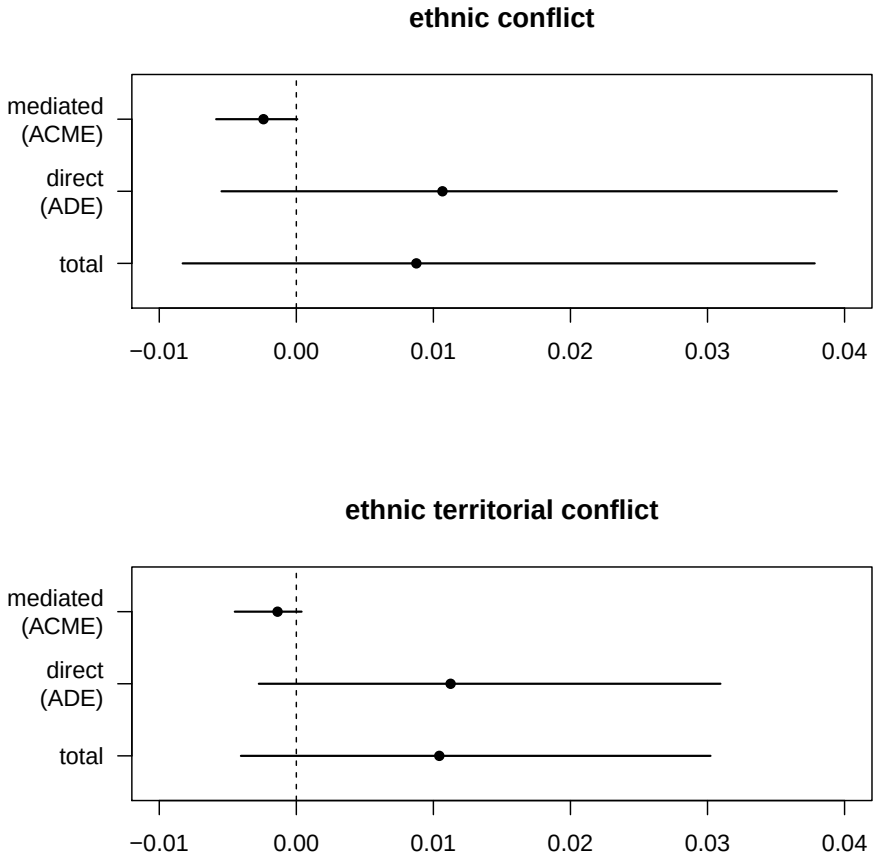
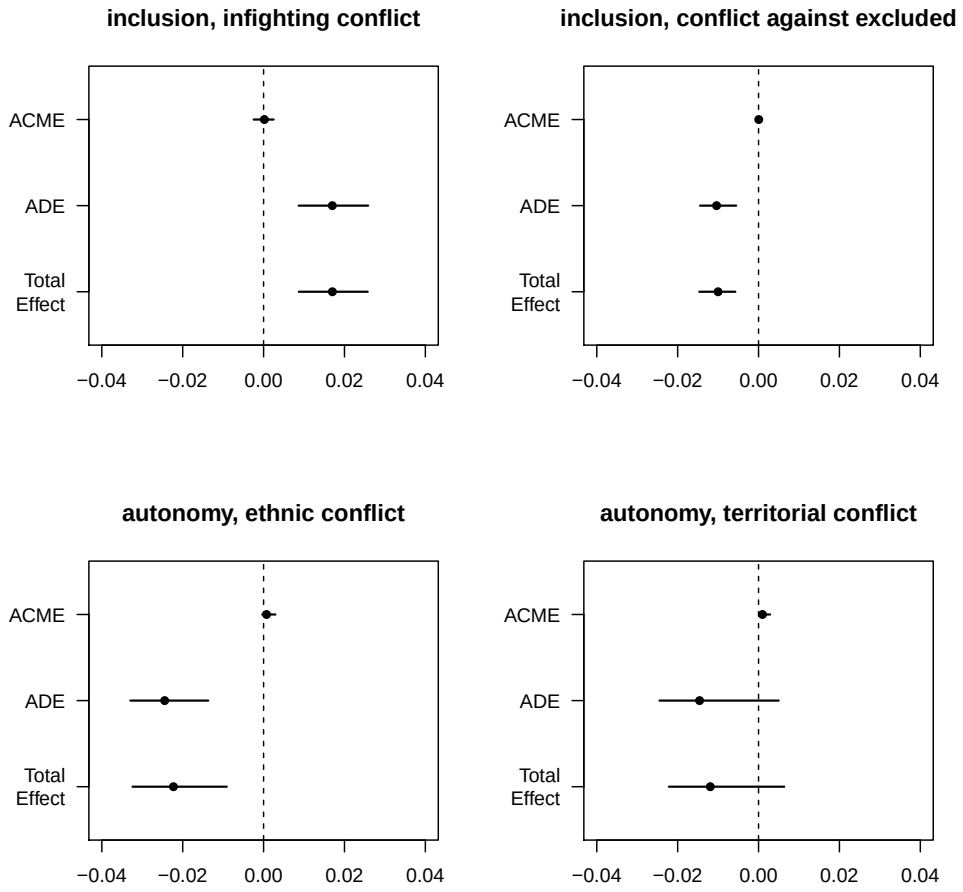


Figure 6: Maximum Effects of Power-Sharing Practices Mediated by Power-Sharing Institutions on Ethnic Conflict Onset



Online Appendix:

"Power-sharing: Institutions, Behavior, and Peace"

American Journal of Political Science

Appendix Contents:

Appendix A: Cases

In Table A1 we list the countries and time-periods covered in our empirical analyses, while A2 presents descriptive statistics of the variables used. Figure A1 shows the distribution of *de jure* powersharing in both democracies and non-democracies.

Table A1: Countries and years covered in the analysis

country	min(year)	max(year)	country	min(year)	max(year)
Afghanistan	1975	2002	Kyrgyzstan	1992	2009
Albania	1990	2009	Laos	1975	2009
Algeria	1975	1991	Latvia	1992	2009
Angola	2003	2008	Lebanon	1975	2009
Argentina	1978	2009	Liberia	1975	2009
Armenia	1992	2009	Libya	2007	2009
Australia	1975	2009	Lithuania	1992	2009
Austria	1975	2009	Macedonia	1994	2009
Azerbaijan	1996	2009	Madagascar	1975	2001
Bahrain	1975	2009	Malawi	1975	2009
Bangladesh	1975	2009	Malaysia	1976	2009
Belarus	1992	2009	Mali	1975	2007
Belgium	1975	2009	Mauritania	1975	2009
Benin	1975	2009	Mexico	1975	2009
Bhutan	1975	2009	Moldova	1992	2009
Bolivia	1975	2009	Mongolia	1975	2009
Bosnia and Herzegovina	1996	2009	Montenegro	2007	2009
Botswana	1975	2009	Morocco	1975	2009
Brazil	1978	2009	Mozambique	1976	2009
Bulgaria	1975	2009	Myanmar	1993	2004

Continued on next page

Table A1: Countries and years covered in the analysis

country	min(year)	max(year)	country	min(year)	max(year)
Burundi	1975	2009	Namibia	1991	2009
Cambodia	1976	2009	Nepal	1975	2009
Cameroon	1975	2009	New Zealand	1975	2009
Canada	1975	2009	Nicaragua	1975	2009
Central African Republic	1975	2009	Niger	1975	2009
Chad	1975	2005	Nigeria	1975	2009
Chile	1975	2009	Pakistan	1978	2004
China	1977	2009	Panama	1975	2009
Congo	1975	2009	Paraguay	1975	2009
Costa Rica	1975	2009	Peru	1975	2007
Croatia	1994	2009	Poland	1975	2009
Czechoslovakia	1975	1992	Romania	1975	2009
Democratic Republic of the Congo	1975	2009	Russia	1975	1999
Djibouti	1978	2009	Rwanda	1975	2009
Ecuador	1975	2009	Saudi Arabia	1975	2009
Egypt	1975	2009	Senegal	1975	2009
El Salvador	1975	2009	Sierra Leone	1975	2009
Eritrea	1994	2009	Slovakia	1994	2009
Estonia	1992	2009	Slovenia	1993	2009
Ethiopia	1993	1998	South Africa	1989	2009
Fiji	1975	2009	Spain	1975	2009
Finland	1975	2009	Sri Lanka	1975	2004
France	1975	2009	Sudan	1975	1983
Gabon	1975	2009	Switzerland	1975	2009
Gambia	1975	1993	Syria	1975	2009
Georgia	1994	2009	Taiwan	1975	2009

Continued on next page

Table A1: Countries and years covered in the analysis

country	min(year)	max(year)	country	min(year)	max(year)
Ghana	1975	2009	Tajikistan	1992	2009
Greece	1975	2009	Tanzania	1975	2009
Guatemala	1996	2009	Thailand	1983	2003
Guinea	1975	2009	Togo	1975	2009
Guinea-Bissau	1975	2009	Trinidad and Tobago	1975	2009
Guyana	1975	2009	Turkey	1975	1984
Honduras	1975	2009	Turkmenistan	1992	2009
Hungary	1975	2009	Uganda	1975	1993
India	1975	1979	Ukraine	1992	2009
Indonesia	1975	2009	United Kingdom	1992	2009
Iran	1975	2005	United States of America	1975	2003
Iraq	1997	2004	Uruguay	2006	2009
Israel	2000	2000	Uzbekistan	1992	2009
Italy	1975	2009	Venezuela	1975	2009
Ivory Coast	1975	2009	Vietnam	1975	2009
Japan	1975	2009	Yemen	1991	2009
Jordan	1975	2009	Yemen Arab Republic	1975	1989
Kazakhstan	1992	2009	Yugoslavia	1975	2002
Kenya	1975	2009	Zambia	1975	2009
Kuwait	1975	2009	Zimbabwe	1980	2009

Table A2: Descriptive statistics

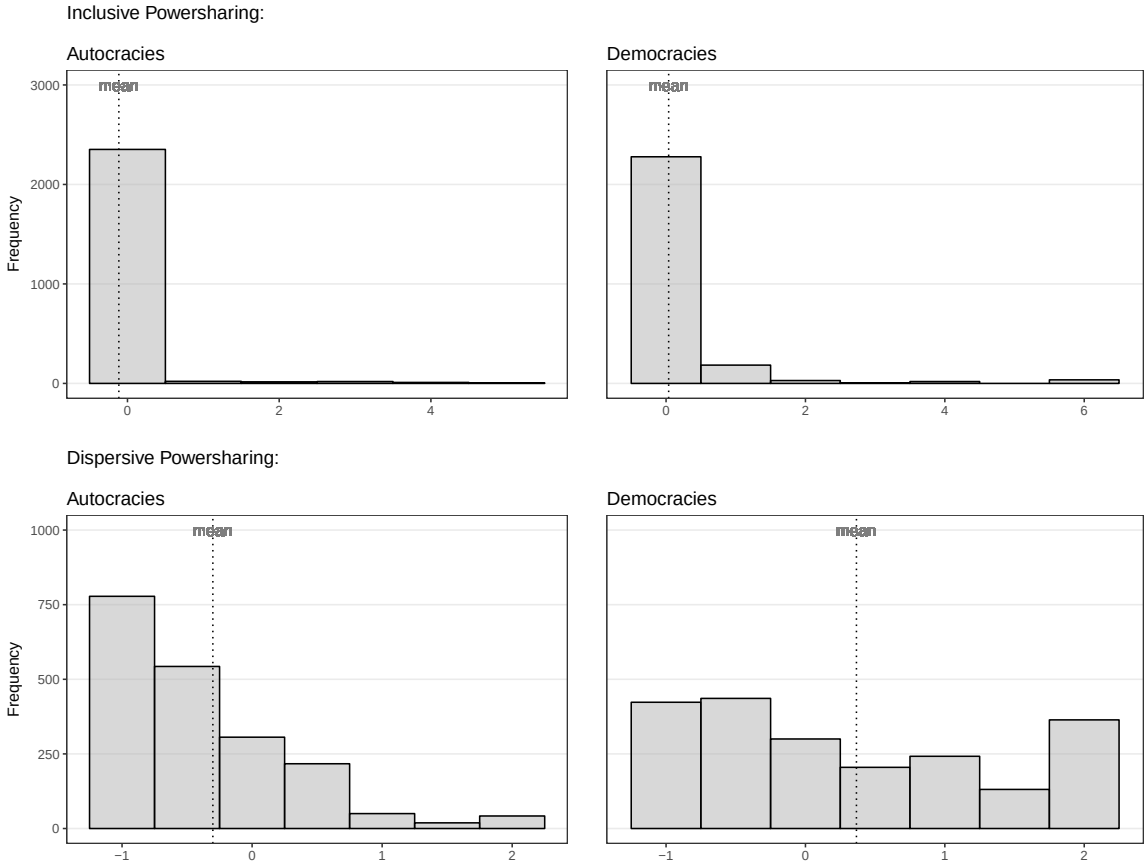
	N	minimum	maximum	mean	variance
Inclusive institutions	4104	-0.20	7.37	0.02	0.69
Dispersive institutions	4104	-0.78	2.10	0.07	0.86
De facto inclusion	4104	0.00	1.00	0.30	0.15
Log(GDP Per Capita)	4104	2.07	4.90	3.54	0.31
Log(Population)	4104	5.40	9.12	7.06	0.43
Prior conflicts	4104	0.00	22.00	1.57	6.72
British colony	4104	0.00	1.00	0.20	0.16
French colony	4104	0.00	1.00	0.18	0.15
Institution missing data	4104	0.00	1.00	0.47	0.25
Ethnic civil war onset	4104	0.00	1.00	0.03	0.03
Ethnic civil war onset infigthing	4104	0.00	1.00	0.00	0.00
Ethnic civil war onset against excl. group	4104	0.00	1.00	0.00	0.00
Peace Years ethnic conflict	4104	0.00	63.00	24.88	372.96
Peace Years ethnic conflict^2	4104	0.00	3969.00	991.72	1173290.34
Peace Years ethnic conflict^3	4104	0.00	250047.00	44639.58	3754992762.83
Peace Years ethnic territorial conflict	4104	0.00	63.00	29.46	347.86
Peace Years ethnic territorial conflict^2	4104	0.00	3969.00	1215.50	1224314.49
Peace Years ethnic territorial conflict^2	4104	0.00	250047.00	55516.20	4160614907.69
Autonomous Population Share	4104	0.00	0.56	0.02	0.00

Figure A1 shows the distribution of inclusive and dispersive powersharing in both democracies and autocracies. We introduce this figure to show that powersharing institutions vary widely within regime type – it is not simply the case that democracies have high levels of powersharing and autocracies have low levels.

The top portion of Figure A1 shows that most countries lack inclusive powersharing institutions of any kind – institutions such as reserved legislative seats for minority groups are quite rare. Importantly, the figure also shows that substantial numbers of both democratic and autocratic country years with high levels of inclusive powersharing, and the mean level of powersharing is similar across the two groups.

Looking at dispersive powersharing, we see substantially higher levels among democracies. However, the existence of a substantial number of democracies with low levels of dispersive powersharing and a substantial number of autocracies with

Figure A1: The distribution of inclusive and dispersive powersharing in both democracies and autocracies



high levels of dispersive powersharing shows that dispersive powersharing is not simply a characteristic of democracies – it is a set of institutional arrangements that can be and is present in both democracies or autocracies.[28]

[28]To divide the sample between democracies and autocracies we use a binary version of the x-polity data from Vreeland (2008).

Appendix B: Sensitivity analyses

For the results of our main analyses to be unbiased the "sequential ignorability" assumption has to hold. Imai et al. (2011, 770) present this assumption as follows:

> First, given the observed pretreatment confounders, the treatment assignment is assumed to be ignorable – statistically independent of potential outcomes and potential mediators. This part of the assumption is often called no-omitted-variable-bias, exogeneity, or unconfoundedness. ... The second part ... implies that the observed mediator is ignorable given the actual treatment status and pretreatment confounders.

The degree to which our observational study violates these demands is difficult to assess. As suggested by Imai et al. (2011) we assess how our estimated mediated effect would change as a function of the correlation term of the errors of our two equations. Figures B1 and B2 depict the results of these sensitivity analyses for the mediated effect of inclusive, respectively dispersive institutions. As the figures show, our results are potentially sensitive to the omission of a confounder that would influence both power-sharing practices and conflict onsets. If the omitted confounder induces a particular relationship among the error terms in these two equations, this would render our estimated mediated effect insignificant or even inverse its sign. However, the figures also demonstrate that the opposite is also possible, such that possible biases actually weakened our results. As we have no way of resolving this question empirically, we draw on extant theory to infer which direction a possible bias is likely to take. As it turns out (detailed below), we have good reason to be confident in our results, which are likely to be conservative estimates.

Regarding inclusive power-sharing and infighting, Figure B1 shows that our estimated positive mediated effect would survive as long as the correlation of the error terms falls within the range of -1 to approximately 0.2. Following Roessler (2011, 2016), governments tend to exclude other groups in their government when there is a high risk of infighting. In other words, expected infighting should plausibly lead to less inclusion, and thus a negative correlation coefficient for the error terms.

Regarding inclusive power-sharing and a conflict against excluded groups, our negative mediated effect remains positive as long the error correlation is above -0.1. Again, Roessler's (2011, 2016) argument suggests that governments are particularly likely to exclude groups where there this is least "risky," i.e. where governments can get away with exclusion. Thus, expectations of a conflict should lead to more inclusion both *de jure* and *de facto*, resulting in a positive correlation in the error terms.

Finally, for dispersive institutions the results of the sensitivity analysis depicted in Figure B2 suggests that the negative mediated effect of these institutions on ethnic or ethnic territorial conflict depends on a error correlation that is positive. The argument and the empirical results presented by Cederman et al. (2015) regarding regional autonomy suggest that governments offer such concessions to stave off potential conflict. This, however, would again make a positive correlation among the error terms more plausible.

In sum, for each of our analyses extant theory suggests that we can be reasonably confident that our results are conservative.

Figure B1: Sensitivity analyses for mediated effect of inclusive power-sharing institutions (H1a and H1b)

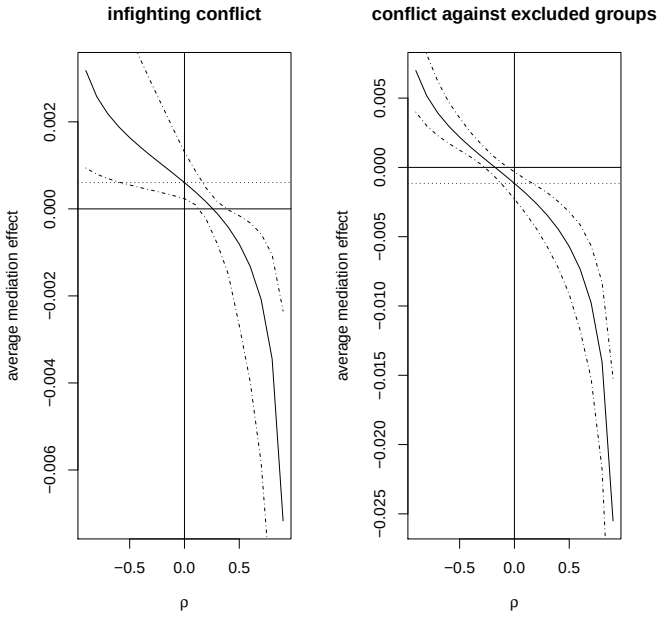
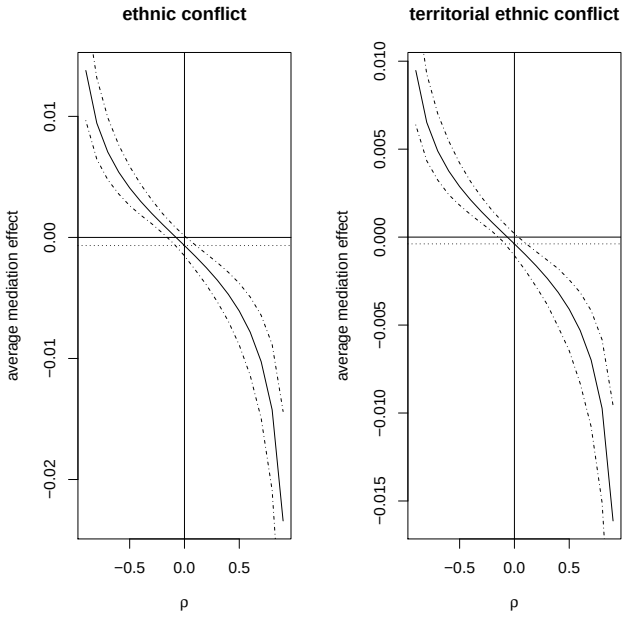


Figure B2: Sensitivity analyses for mediated effect of dispersive power-sharing institutions (H2)



Appendix C: Additional analyses

In this appendix we report the results of a series of additional analyses. First, in Table C1 we report the results of a multinomial probit model (Imai & van Dyk 2005*a*, Imai & van Dyk 2005*b*) to take into account that infighting and conflict with an excluded group are competing risks due to inclusive institutions. As the table shows, taking these competing risks into account barely affects the results presented in Table 1 in the main text. Second, Third, as in tables C2 and C3 we report results from models that consider the moderating effect of practices on institutions, by estimating models with interaction terms. Third, in tables C4, C5 and C6 we report analyses that replicate our main analysee (see tables 1 and 2) use as additional control the measure of democracy proposed by Vreeland (2008) building upon the well-known Polity-measure. Finally, we report results from replications of our main analyses (see tables 1 and 2), but split the sample in observations without prior conflict and those without (see tables C7, C8 and C9).

Following up in subsequent sections we assess the robustness of the results of our mediation analyses by adding additional control variables and lagging our main explantory variables to alleviate endogenity concerns.

Table C1: Inclusive institutions and ethnic conflict among and against power-sharing partners

	infighting	outfighting
Inclusive PS	0.031	−0.018
	(0.095)	(0.107)
Institution missing data	0.110	0.156
	(0.390)	(0.155)
Included share of ethnic groups	1.733	−0.842
	(0.689)	(0.391)
Log(GDP p.c.)	−0.625	−0.419
	(0.639)	(0.237)
Log(Population)	−0.225	−0.066
	(0.366)	(0.133)
Prior Conflicts	0.070	−0.000
	(0.060)	(0.031)
Peace Years	−0.009	−0.029
	(0.046)	(0.033)
Peace Years^2	0.001	0.001
	(0.002)	(0.002)
Peace Years^3	−0.000	−0.000
	(0.000)	(0.000)
British Colony	−5.806*	−0.122
	(4.320)	(0.222)
French Colony	−0.624	0.367
	(0.354)	(0.189)
constant	−0.440	−0.395
	(2.634)	(1.113)
σ_1	0.933	
	(0.330)	
σ_2	1.067	
	(0.330)	
$\sigma_{1,2}$	0.331	
	(0.629)	
N	4104	

Standard errors in parentheses
1000 burnins, 5000 MCMC draws thinned by 5

Table C2: Models with interactions: inclusive power-sharing

	ethnic conflict	infighting	conflict against excl. group
constant	−2.513*	−0.701	−0.544
	(0.691)	(1.981)	(1.104)
Inclusive PS	0.035	0.259	0.121
	(0.093)	(0.192)	(0.110)
Included PS Pop. Share	−0.021	1.174*	−0.716*
	(0.134)	(0.356)	(0.319)
Log(GDP p.c.)	−0.292*	−0.451	−0.339*
	(0.100)	(0.247)	(0.166)
Log(Population)	0.209*	−0.177	−0.085
	(0.080)	(0.254)	(0.130)
Prior Conflicts	0.076*	0.075	0.013
	(0.014)	(0.056)	(0.028)
Institution missing data	0.119	0.054	0.148
	(0.096)	(0.264)	(0.153)
British Colony	−0.200	−4.238	−0.176
	(0.127)	(202.273)	(0.217)
French Colony	0.082	−0.590*	0.303
	(0.128)	(0.284)	(0.175)
Included PS Pop. Share * Inclusive PS	−0.023	−0.337	−0.934
	(0.146)	(0.265)	(1.297)
Peace Years	−0.018	0.026	−0.020
	(0.020)	(0.050)	(0.030)
Peace Years^2	0.001	−0.000	0.001
	(0.001)	(0.003)	(0.002)
Peace Years^3	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
N	4104	4104	4104
AIC	955.700	155.678	369.151
BIC	1284.326	484.304	697.776
$\log L$	−425.850	−25.839	−132.575

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table C3: Models with interactions: dispersive power-sharing

	ethnic conflict	ethnic, terr. conflict
constant	−2.543*	−3.878*
	(0.748)	(0.991)
Dispersive PS	−3.271*	−2.843
	(1.533)	(1.649)
Autonomous Pop. Share	0.036	0.065
	(0.065)	(0.084)
Log(GDP p.c.)	−0.320*	−0.282*
	(0.104)	(0.136)
Log(Population)	0.226*	0.405*
	(0.087)	(0.110)
Prior Conflicts	0.082*	0.057*
	(0.014)	(0.016)
Institution missing data	0.146	0.116
	(0.100)	(0.131)
British Colony	−0.285*	−0.153
	(0.128)	(0.156)
French Colony	0.020	0.012
	(0.122)	(0.181)
Autonomous Pop. Share * Dispersive PS	1.862	1.640
	(1.063)	(1.131)
Peace Years	−0.001	−0.024
	(0.020)	(0.025)
Peace Years^2	−0.000	0.000
	(0.001)	(0.001)
Peace Years^3	−0.000	0.000
	(0.000)	(0.000)
N	4104	4104
AIC	944.320	590.355
BIC	1272.945	918.980
$\log L$	−420.160	−243.177

Standard errors in parentheses
* indicates significance at $p < 0.05$

Table C4: Inclusive institutions and ethnic conflict, controlling for democracy

	inclusion	ethnic civil war
constant	0.716*	−2.303*
	(0.086)	(0.728)
Inclusive PS	0.092*	0.073
	(0.009)	(0.067)
Log(GDP p.c.)	−0.035*	−0.343*
	(0.013)	(0.113)
Log(Population)	−0.061*	0.210*
	(0.010)	(0.084)
Prior Conflicts	−0.002	0.073*
	(0.003)	(0.014)
Peace Years	0.011*	−0.023
	(0.002)	(0.021)
Peace Years^2	−0.000*	0.001
	(0.000)	(0.001)
Peace Years^3	0.000*	−0.000
	(0.000)	(0.000)
Institution missing data	−0.052*	0.085
	(0.012)	(0.103)
British Colony	0.219*	−0.206
	(0.015)	(0.131)
French Colony	0.368*	0.092
	(0.017)	(0.137)
Democracy (xpolity)	0.012*	0.002
	(0.001)	(0.013)
Included PS Pop. Share		−0.030
		(0.143)
N	3912	3912
R^2	0.197	
adj. R^2	0.195	
Resid. sd	0.346	
AIC		872.291
BIC		1198.425
$\log L$		−384.146

Standard errors in parentheses
* indicates significance at $p < 0.05$

Table C5: Inclusive institutions, ethnic conflict and infighting, controlling for democracy

	inclusion	infighting	conflict against excl. group
constant	0.716*	0.802	−0.309
	(0.086)	(2.250)	(1.168)
Inclusive PS	0.092*	−0.093	0.078
	(0.009)	(0.170)	(0.105)
Log(GDP p.c.)	−0.035*	−0.935*	−0.255
	(0.013)	(0.345)	(0.175)
Log(Population)	−0.061*	−0.109	−0.130
	(0.010)	(0.286)	(0.136)
Prior Conflicts	−0.002	−0.003	0.000
	(0.003)	(0.082)	(0.030)
Peace Years	0.011*	−0.008	−0.030
	(0.002)	(0.058)	(0.032)
Peace Years^2	−0.000*	0.001	0.000
	(0.000)	(0.003)	(0.002)
Peace Years^3	0.000*	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
Institution missing data	−0.052*	−0.296	0.019
	(0.012)	(0.326)	(0.160)
British Colony	0.219*	−4.638	−0.117
	(0.015)	(304.251)	(0.217)
French Colony	0.368*	−0.658*	0.214
	(0.017)	(0.321)	(0.188)
Democracy (xpolity)	0.012*	−0.002	−0.038
	(0.001)	(0.037)	(0.021)
Included PS Pop. Share		1.173*	−0.640*
		(0.389)	(0.251)
N	3912	3912	3912
R^2	0.197		
adj. R^2	0.195		
Resid. sd	0.346		
AIC		131.459	333.365
BIC		457.593	659.499
$\log L$		−13.730	−114.683

Standard errors in parentheses
* indicates significance at $p < 0.05$

Table C6: Dispersive institutions and ethnic conflict, controlling for democracy

	autonomy	ethnic conflict	autonomy	territorial conflict
constant	−0.059*	−2.142*	−0.068*	−3.822*
	(0.019)	(0.774)	(0.019)	(1.030)
Dispersive PS	0.008*	0.087	0.009*	0.088
	(0.002)	(0.069)	(0.002)	(0.088)
Log(GDP p.c.)	−0.008*	−0.361*	−0.005*	−0.296*
	(0.002)	(0.114)	(0.002)	(0.145)
Log(Population)	0.012*	0.199*	0.013*	0.392*
	(0.002)	(0.089)	(0.002)	(0.113)
Prior Conflicts	0.005*	0.077*	0.005*	0.061*
	(0.001)	(0.015)	(0.001)	(0.017)
Peace Years	0.002*	−0.010		
	(0.000)	(0.021)		
Peace Years^2	−0.000*	0.000		
	(0.000)	(0.001)		
Peace Years^3	0.000*	−0.000		
	(0.000)	(0.000)		
Institution missing data	0.000	0.116	−0.001	0.135
	(0.002)	(0.106)	(0.002)	(0.138)
British Colony	−0.013*	−0.280*	−0.014*	−0.159
	(0.003)	(0.133)	(0.003)	(0.165)
French Colony	−0.007*	0.043	−0.007*	0.093
	(0.003)	(0.129)	(0.003)	(0.187)
Democracy (xpolity)	0.001*	0.004	0.001*	0.017
	(0.000)	(0.013)	(0.000)	(0.018)
Autonomous Pop. Share		−1.198		−1.004
		(0.763)		(0.833)
Peace Years			0.002*	−0.027
			(0.001)	(0.026)
Peace Years^2			−0.000*	0.000
			(0.000)	(0.001)
Peace Years^3			0.000*	−0.000
			(0.000)	(0.000)
N	3912	3912	3912	3912
R^2	0.107		0.099	
adj. R^2	0.104		0.096	
Resid. sd	0.067		0.067	
AIC		866.603		550.909
BIC		1192.737		877.043
$\log L$		−381.302		−223.454

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table C7: Inclusive institutions and ethnic conflict, pre- and post-war observations

	pre-conflict		post-conflict	
	inclusion	ethnic conflict	inclusion	ethnic conflict
constant	0.661*	−0.862	1.098*	−3.503*
	(0.141)	(1.555)	(0.097)	(0.774)
Inclusive PS	0.165*	0.178	0.071*	0.001
	(0.016)	(0.163)	(0.007)	(0.058)
Log(GDP p.c.)	0.045*	−0.746*	−0.087*	−0.309*
	(0.020)	(0.299)	(0.014)	(0.112)
Log(Population)	−0.066*	0.228	−0.090*	0.396*
	(0.017)	(0.197)	(0.011)	(0.086)
Peace Years	−0.003	−0.107	0.013*	−0.010
	(0.006)	(0.067)	(0.003)	(0.025)
Peace Years^2	−0.000	0.005	−0.000*	−0.001
	(0.000)	(0.003)	(0.000)	(0.002)
Peace Years^3	0.000	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Institution missing data	−0.116*	0.214	−0.052*	0.073
	(0.019)	(0.248)	(0.014)	(0.107)
British Colony	0.057*	−4.074	0.306*	0.056
	(0.027)	(191.400)	(0.017)	(0.126)
French Colony	0.238*	0.003	0.398*	0.018
	(0.032)	(0.291)	(0.018)	(0.153)
Included PS Pop. Share		0.087		−0.104
		(0.281)		(0.161)
N	1740	1740	2364	2364
R^2	0.110		0.356	
adj. R^2	0.105		0.353	
Resid. sd	0.380		0.301	
AIC		182.819		802.849
BIC		423.131		1056.646
$\log L$		−47.409		−357.424

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table C8: Inclusive institutions, ethnic conflict and infighting, pre- and post-war observations

	pre-conflict			post-conflict		
	inclusion	infighting	conflict against excl. group	inclusion	infighting	conflict against excl. group
constant	0.661*	2.415	−22.277	1.098*	−1.740	0.074
	(0.141)	(4.491)	(1852.503)	(0.097)	(2.464)	(1.361)
Inclusive PS	0.165*	0.155	−112.065	0.071*	0.002	0.030
	(0.016)	(0.515)	(9378.819)	(0.007)	(0.097)	(0.091)
Log(GDP p.c.)	0.045*	−0.367	−0.705	−0.087*	−0.315	−0.261
	(0.020)	(0.632)	(0.429)	(0.014)	(0.281)	(0.194)
Log(Population)	−0.066*	−0.579	−0.003	−0.090*	−0.093	−0.198
	(0.017)	(0.615)	(0.263)	(0.011)	(0.286)	(0.163)
Peace Years	−0.003	−0.074	−0.079	0.013*	0.026	0.013
	(0.006)	(0.137)	(0.087)	(0.003)	(0.063)	(0.045)
Peace Years^2	−0.000	0.003	0.004	−0.000*	−0.000	−0.002
	(0.000)	(0.006)	(0.003)	(0.000)	(0.004)	(0.003)
Peace Years^3	0.000	−0.000	−0.000	0.000	−0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Institution missing data	−0.116*	−0.336	0.465	−0.052*	0.229	0.074
	(0.019)	(0.624)	(0.381)	(0.014)	(0.340)	(0.177)
British Colony	0.057*	−4.075	−4.510	0.306*	−4.542	0.084
	(0.027)	(464.626)	(3476.075)	(0.017)	(261.706)	(0.236)
French Colony	0.238*	0.160	0.098	0.398*	−0.977*	0.344
	(0.032)	(0.612)	(0.370)	(0.018)	(0.375)	(0.211)
Included PS Pop. Share		0.640	−0.121		1.555*	−0.851*
		(0.631)	(0.392)		(0.465)	(0.282)
N	1740	1740	1740	2364	2364	2364
R^2	0.110			0.356		
adj. R^2	0.105			0.353		
Resid. sd	0.380			0.301		
AIC		47.728	106.096		120.377	271.340
BIC		288.040	346.409		374.173	525.137
$\log L$		20.136	−9.048		−16.188	−91.670

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table C9: Dispersive institutions and ethnic conflict, pre- and post-war observations

	pre-conflict			post-conflict		
	autonomy	ethnic conflict	terr. conflict	autonomy	ethnic conflict	conflict conflict
constant	0.028	−0.763	−6.438	−0.191*	−3.535*	−3.475*
	(0.017)	(1.716)	(3.344)	(0.027)	(0.808)	(1.085)
Dispersive PS	−0.001	0.040	−0.258	0.018*	0.020	0.094
	(0.001)	(0.177)	(0.275)	(0.002)	(0.069)	(0.087)
Log(GDP p.c.)	−0.007*	−0.668*	−0.428	−0.004	−0.226*	−0.341*
	(0.002)	(0.302)	(0.482)	(0.004)	(0.113)	(0.145)
Log(Population)	−0.000	0.170	0.824*	0.032*	0.372*	0.423*
	(0.002)	(0.203)	(0.379)	(0.003)	(0.091)	(0.113)
Peace Years	−0.000	−0.109	−0.154	−0.000	−0.005	−0.028
	(0.001)	(0.067)	(0.119)	(0.001)	(0.021)	(0.025)
Peace Years^2	0.000	0.005	0.007	0.000	−0.000	−0.000
	(0.000)	(0.003)	(0.006)	(0.000)	(0.001)	(0.001)
Peace Years^3	−0.000	−0.000	−0.000	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Institution missing data	0.002	0.277	0.262	−0.001	0.102	0.071
	(0.002)	(0.256)	(0.365)	(0.004)	(0.110)	(0.143)
British Colony	−0.006	−3.978	−3.820	−0.025*	−0.017	0.045
	(0.003)	(195.240)	(286.811)	(0.004)	(0.124)	(0.148)
French Colony	0.006	−0.007	−0.045	−0.012*	−0.058	−0.068
	(0.003)	(0.285)	(0.501)	(0.005)	(0.145)	(0.218)
Autonomous Pop. Share		−11.092	−0.779		−0.828	−0.586
		(14.320)	(6.790)		(0.717)	(0.831)
N	1740	1740	1740	2364	2364	2364
R^2	0.058			0.149		
adj. R^2	0.053			0.146		
Resid. sd	0.041			0.078		
AIC		183.372	94.945		792.316	514.096
BIC		423.684	335.257		1046.112	767.893
$\log L$		−47.686	−3.472		−352.158	−213.048

Standard errors in parentheses

* indicates significance at $p < 0.05$

Replicating Results from Mediation Analysis with Additional Controls

In the following sections, we replicate the main mediation analysis from the body of the paper (i.e Figures 3 & 5) while adding a range of additional control variables. In the analyses in the body of the paper, we control for GDP per capita and population (both logged), colonial origin, polynomials of the time since the most recent conflict, and a dummy variable for whether any of the underlying indicators in the IDC data are missing for that observation. In these analyses we add additional controls in batches – we do not add all of the additional controls into the model together to avoid multicollinearity problems and because a number of the additional control variables have substantial numbers of missing values, which reduces the sample size. Each figure in this section produces a series of plots, the first of which reproduces the results from the primary specification in body of the paper, with subsequent plots adding in batches of additional control variables. We add additional controls for: fuel dependence (% of GDP, from Ross (2013)) and state capacity (an aggregation of 24 variables capturing administrative and coercive capacity, from Hanson & Sigman (2013)); economic growth and inequality (World Development Indicators); conflicts in neighboring countries (sum of all Major Episodes of Political Violence (MEPVs) in neighboring states, from (Marshall 2012); dummy variable for a religious government (Cruz, Keefer & Scartascini 2016) and dummy variables geographic region; and type of prior civil conflict, where we control for three characteristics: whether the war was a separatist conflict (Themnér & Wallensteen 2012), whether it resulted in a peace settlement (Sarkees & Wayman 2010), and whether international peacekeepers were involved (Fortna 2008). Drawing on the IDC and EPR-ETH powersharing

datasets, we also add controls for the level of ethno-linguistic fractionalization in the country and for whether there has been a recent change in the level of *de jure* powersharing[29].

The control for fuel dependence reflects the concern that resource dependence may make civil conflict more likely and may also alter the incentives to adopt specific types of powersharing arrangements, particularly depending on the geographic location of natural resource wealth relative to where different ethnic groups reside within the country. State capacity may related to the resource curse in some cases, and may affect the probability of conflict directly. Similarly, powersharing may require a certain level of state capacity to be successfully adopted.

Economic growth and inequality may potentially affect both conflict outcomes and powersharing adoption. Conflict in neighboring countries is controlled for because conflicts sometimes diffuse across borders. While the link between neighboring conflicts and powersharing adoption is less clear, we acknowledge that such effects are possible. We control for the nature of past conflict as this may both shape the likelihood of recurrence and the likelihood of powersharing adoption. Similarly, both conflict and powersharing adoption may vary across religious and non-religious governments and different regions of the world. Powersharing and conflict may both be more likely in countries with higher levels of ethno-linguistic fractionalization, and recent changes in powersharing may indicate both less institutional stability and a higher probability of conflict onset.

[29]This variable takes a value of one if there has been a year-to-year change of at least .5 standard deviations in either inclusive or dispersive powersharing at any time in the past five years and zero otherwise.

Replicating effects of inclusive powersharing on ethnic conflict onset (infighting)

In Figure C1 we test the robustness of the results linking inclusive powersharing to infighting between ethnic groups in power (i.e. the upper plot in Figure 3 in the main paper). The primary finding with respect to infighting in the body of the paper is that the indirect effect of inclusive powersharing is to increase infighting, and that result is robust across all sets of alternative controls. The estimates of the mediated effect (ACME) are consistent across specifications. The direct effect of *de jure* inclusive powersharing and is not statistically significant in the primary specification, and varies in sign with different sets of controls. In a number of specifications the direct effect (and hence also the total effect) are estimated extremely imprecisely, primarily due to reduced sample sizes, though it is estimated to be positive in all specifications. The key takeaway from Figure C1 is that, even though several batches of controls reduce the precision of estimation, the positive indirect effect of inclusive powersharing on infighting is robust, further increasing our confidence in Hypothesis 1b.

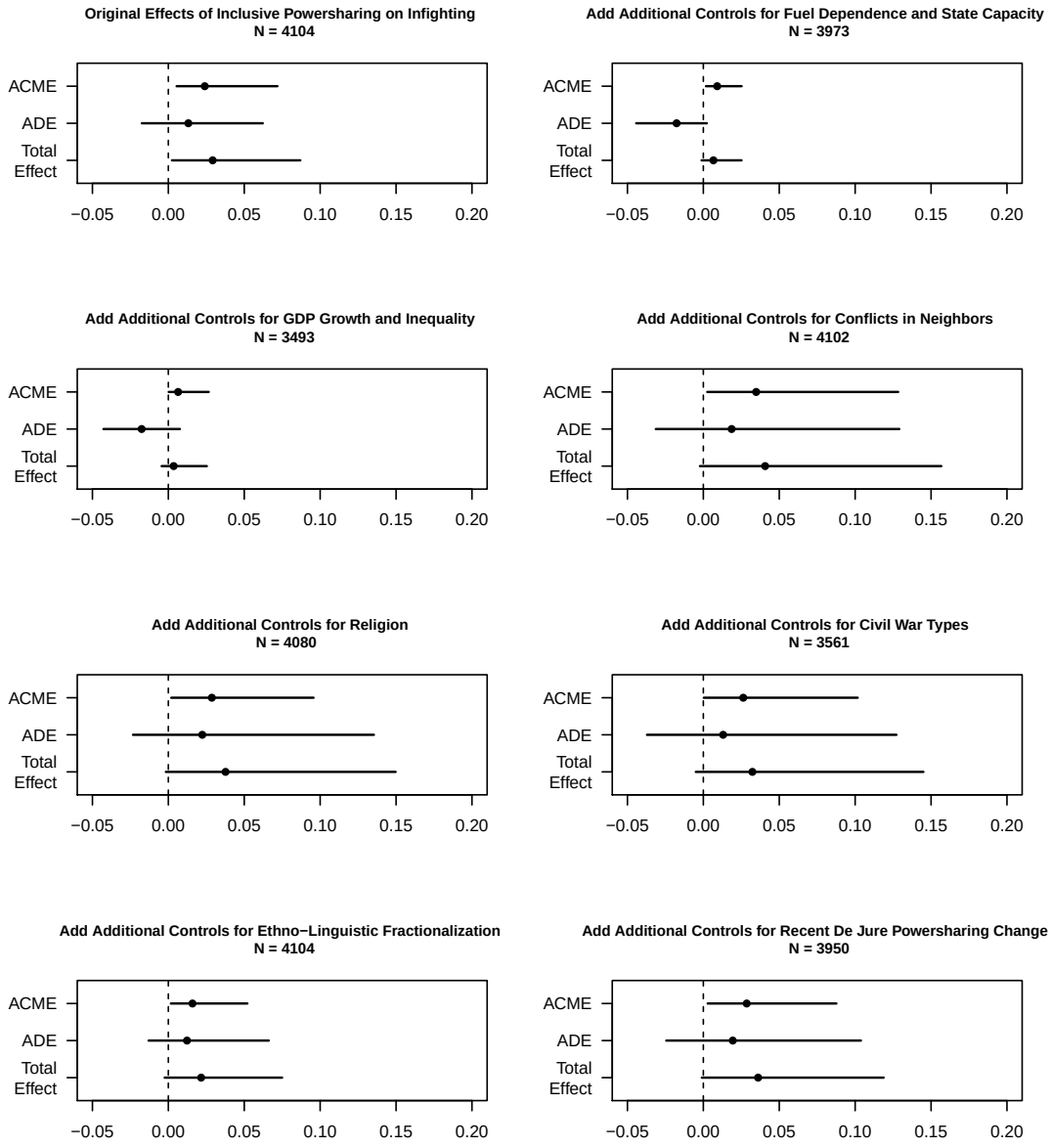


Figure C1: Replicating Effects of Inclusive Powersharing on Ethnic Conflict Onset (infighting): Additional Controls.

Replicating effects of inclusive powersharing on ethnic conflict onset (conflict with excluded groups)

Figure C2 probes the robustness of our findings in support of H1a, which predicts that the indirect effect of inclusive powersharing is to reduce conflict between the government and excluded groups. Consistent with the results in Figure 3 in the main paper, we estimate a negative and statistically significant indirect effect (ACME) across all specifications. Neither the direct effect or total effect estimated is statistically significant in either the primary specification or in any of the alternative specifications. These results lend further support to Hypothesis 1a. While the indirect effect of inclusive powersharing is to increase the risk fighting between parties in government, it reduces the probability of conflict with excluded groups.

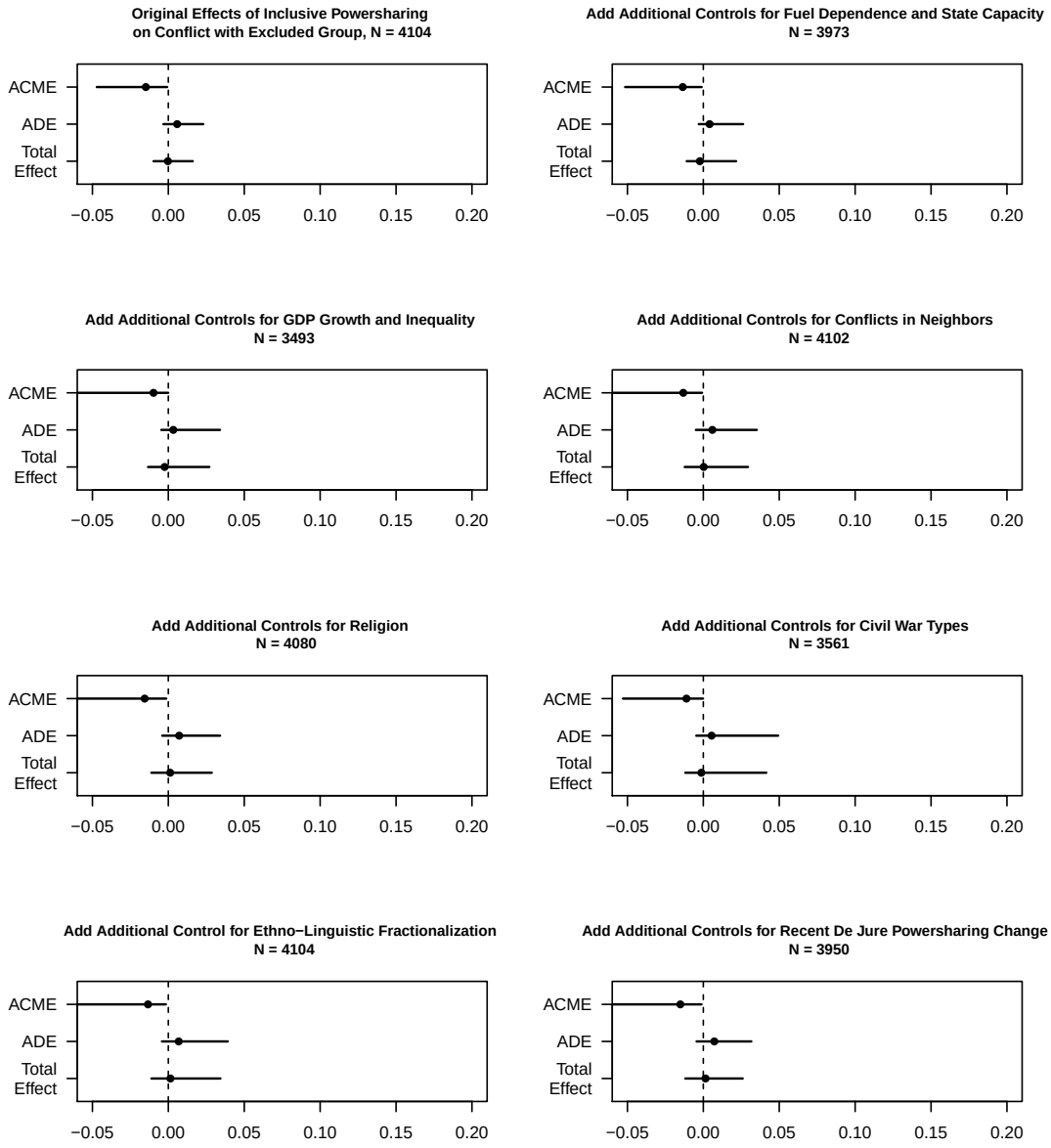


Figure C2: Replicating Effects of Inclusive Powersharing on Conflict with Excluded Groups: Additional Controls.

Replicating effects of dispersive powersharing on ethnic conflict

In the primary specification (Figure 5 in the main text, upper left plot in Figure C3), we find support for the expectation that dispersive powersharing has an indirect negative effect on ethnic conflict by increasing regional autonomy for minorities. We estimate that the direct and total effects of dispersive powersharing are positive, but these estimates are not statistically significant. These results are reasonably robust.

When controlling for the nature of the prior civil war or the religious make-up of the country, the negative mediated effect we estimate slips just below statistical significance, while remaining similar in magnitude to the primary specification. The direct and total effects, which are positive but statistically insignificant in the primary specification, are also positive and insignificant in the alternative specifications.

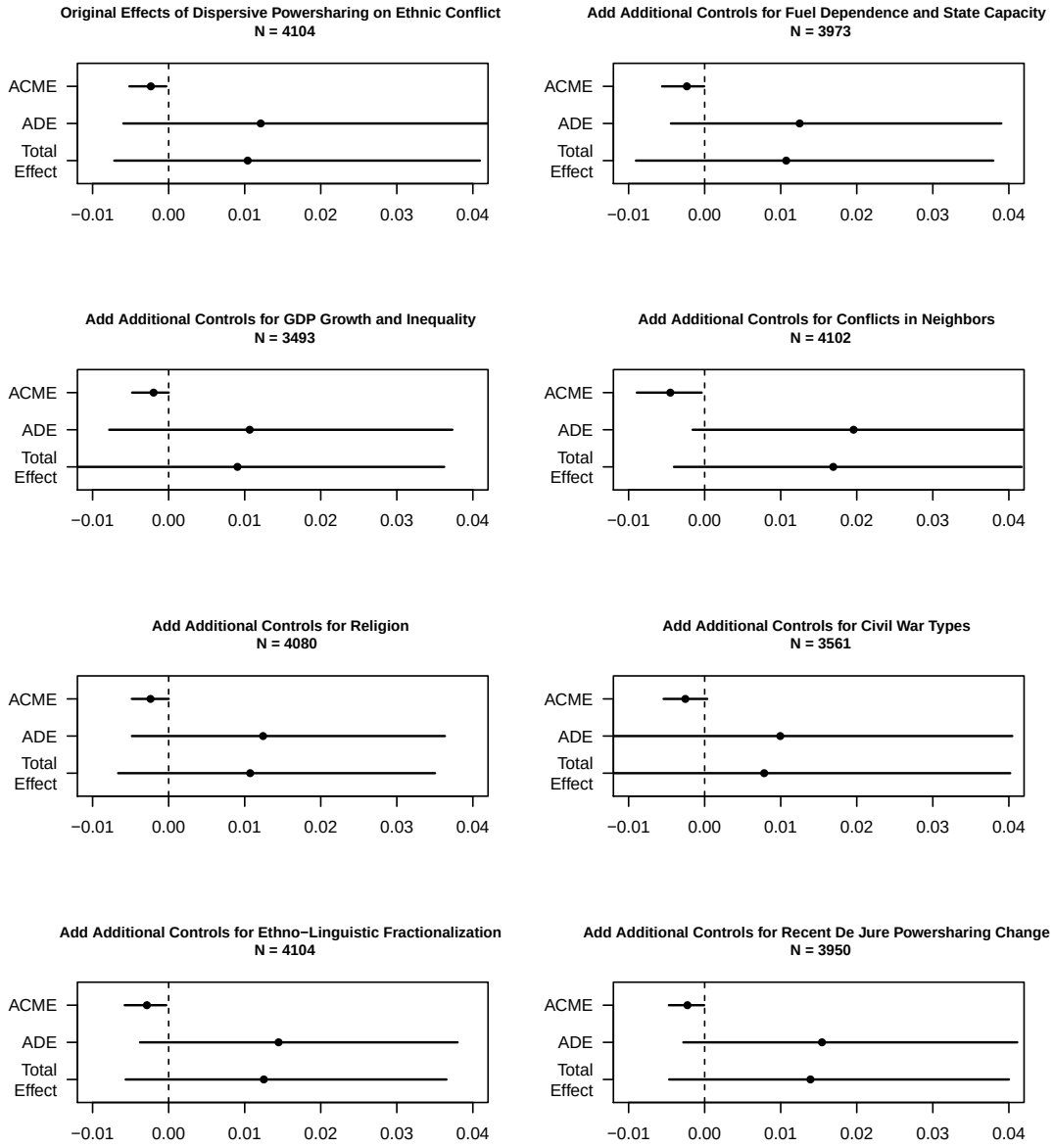


Figure C3: Replicating Effects of Dispersive Powersharing on Ethnic Conflict: Additional Controls.

Replicating effects of dispersive powersharing on ethnic territorial conflict

In the main text (Figure 5) we estimate a negative but statistically insignificant mediated effect (ACME) of dispersive powersharing on territorial ethnic conflict, and positive but statistically insignificant direct (ADE) and total effects. Figure C4 subjects these estimates to the same battery of robustness tests as our other results, and the findings are consistent. All estimated effects are insignificant, but consistent in sign with the primary specification.

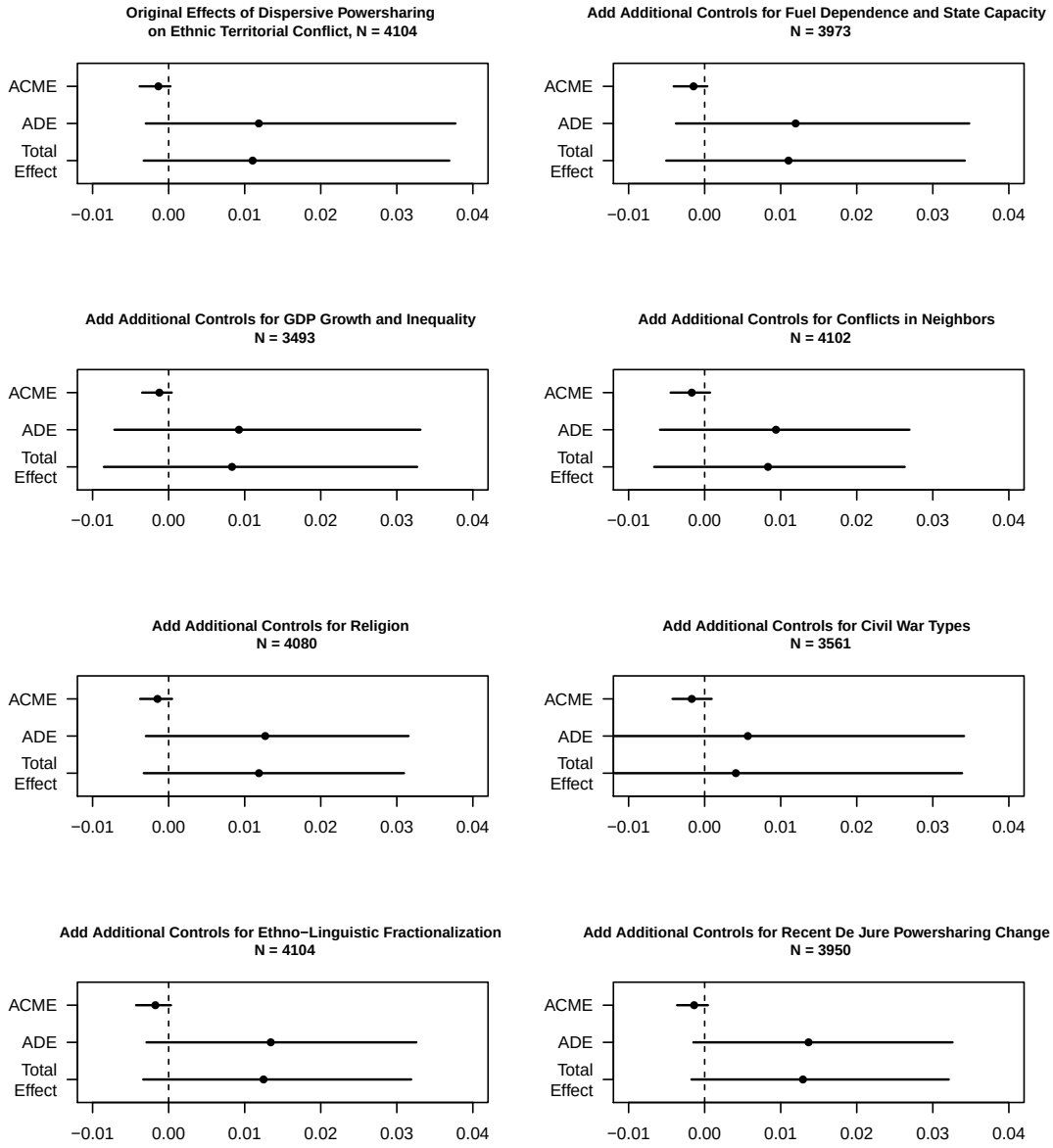


Figure C4: Replicating Effects of Dispersive Powersharing on Ethnic Territorial Conflict: Additional Controls.

Varying lags of independent variables

The figures in this section replicate the core mediation analyses from the main paper (Figures 3 & 5) while varying the lags of the independent variables. Both the IDC data and the EPR data are coded as of January 1 of a given year. In the primary specifications in the paper, which do not lag the independent variables, we are using powersharing institutions and practices as of January 1 to predict conflict onsets in the entire calendar year. Thus, the independent variables in the primary analysis are temporally prior to the dependent variable. However, because the onset of conflict is sometimes predictable based on events months, or even a couple of years, prior to the onset of actual fighting, is is possible that powersharing institutions and behaviors are sometimes shaped by conflicts that are about to start, but have not yet started. Such anticipation introduces the possibility of reverse causation into our analysis. If we are able to show that our results are robust to lagging the independent variables by several years, we are able to show that such anticipation is unlikely to be driving our results.

Each figure in this section contains four plots. The upper-left plot reproduces the results reported in the main text, while the remaining three plots replicate these results while lagging all independent variables by one, three, and five years. Across all the analyses in this section, we see that lagging the independent variables affects our results very little and does not change their substantive interpretation.

Figure C5 probes the robustness of Hypothesis 1b, which asserts that inclusive powersharing institutions increases the risk of infighting between ethnic groups in the governing coalition by increasing the share of ethnic groups that are represented in government. These results are remarkably stable as we increase the lag on the independent variables by one, three and five years. Most importantly, in all

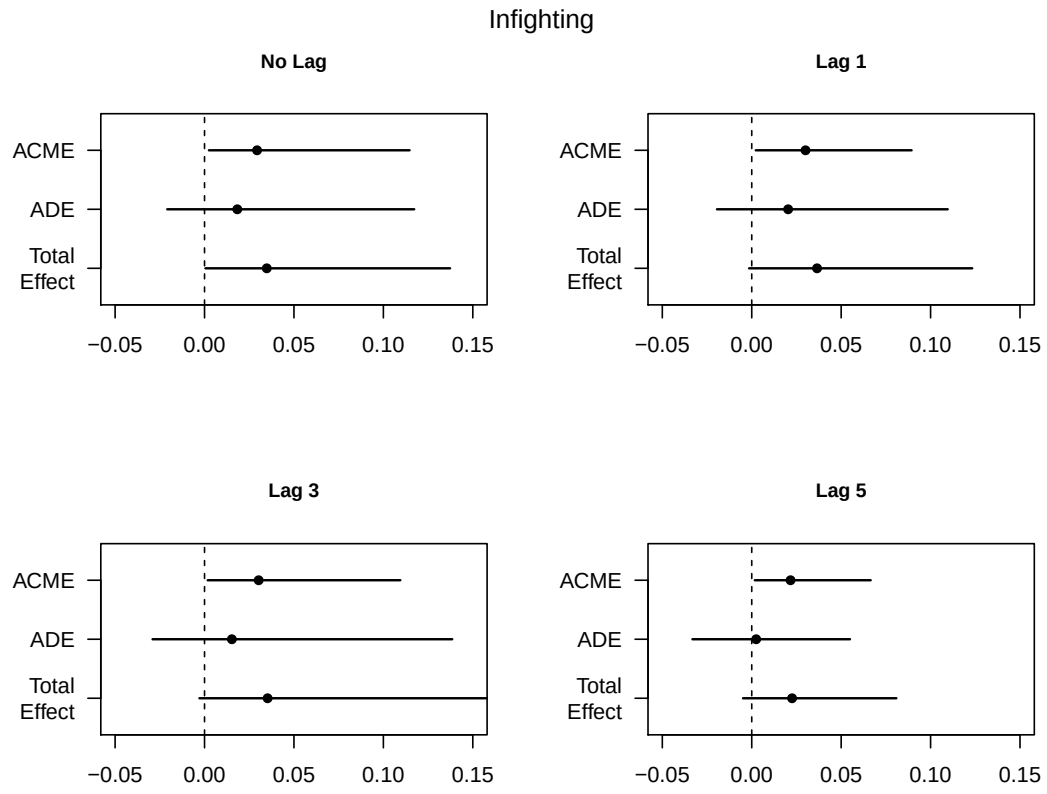


Figure C5: Replicating Effects of Inclusive Powersharing on Ethnic Conflict Onset (infighting): Varying Lags.

specifications we estimate a positive and statistically significant mediated effect (ACME). This increases our confidence that the support we find for Hypothesis 1b is not driven by reverse causation.

The total effect is also estimated to be positive in all specifications, though with the additional lags this result is just short of statistical significance.

Figure C6 tests the robustness of Hypothesis 1a, which predicts that inclusive powersharing institutions decrease the risk of conflict between the government and excluded groups by increasing the share of ethnic groups that are included in government. Again, these results are stable as the lags are varied. In all specifications we estimate a negative and statistically significant mediated effect

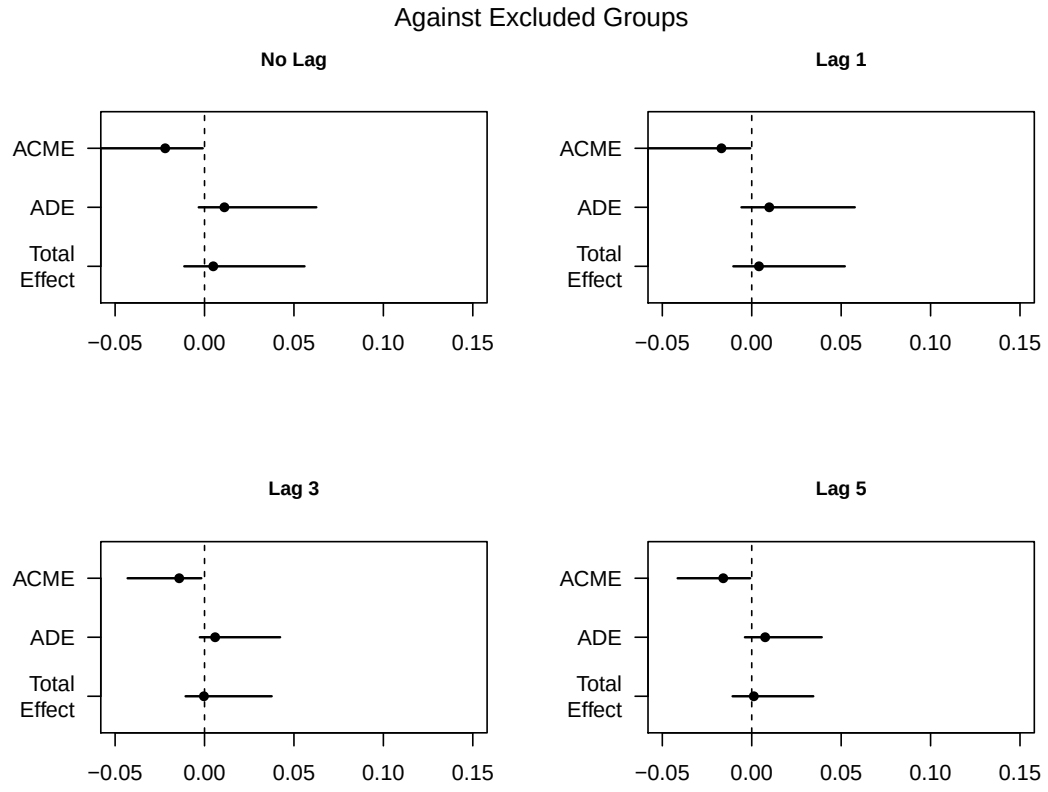


Figure C6: Replicating Effects of Inclusive Powersharing on Conflict with Excluded Groups: Varying Lags.

(ACME) of inclusive powersharing on conflict with excluded groups.

Figure C7 and Figure C8 probe the robustness of Hypothesis 2, which asserts that dispersive powersharing institutions decrease the risk of ethnic conflict by increasing the share of ethnic groups that enjoy regional autonomy. Figure C7 replicates the results examining the effect on ethnic conflict and Figure C8 replicates the results examining ethnic territorial conflict.

The estimated effects in C7 are consistent in sign and similar in magnitude as we increase the lags, but the negative mediated effect (ACME) that we estimate, while statistically significant in the primary specification, falls from significance as the length of the lag on the independent variables is increased. Thus, these

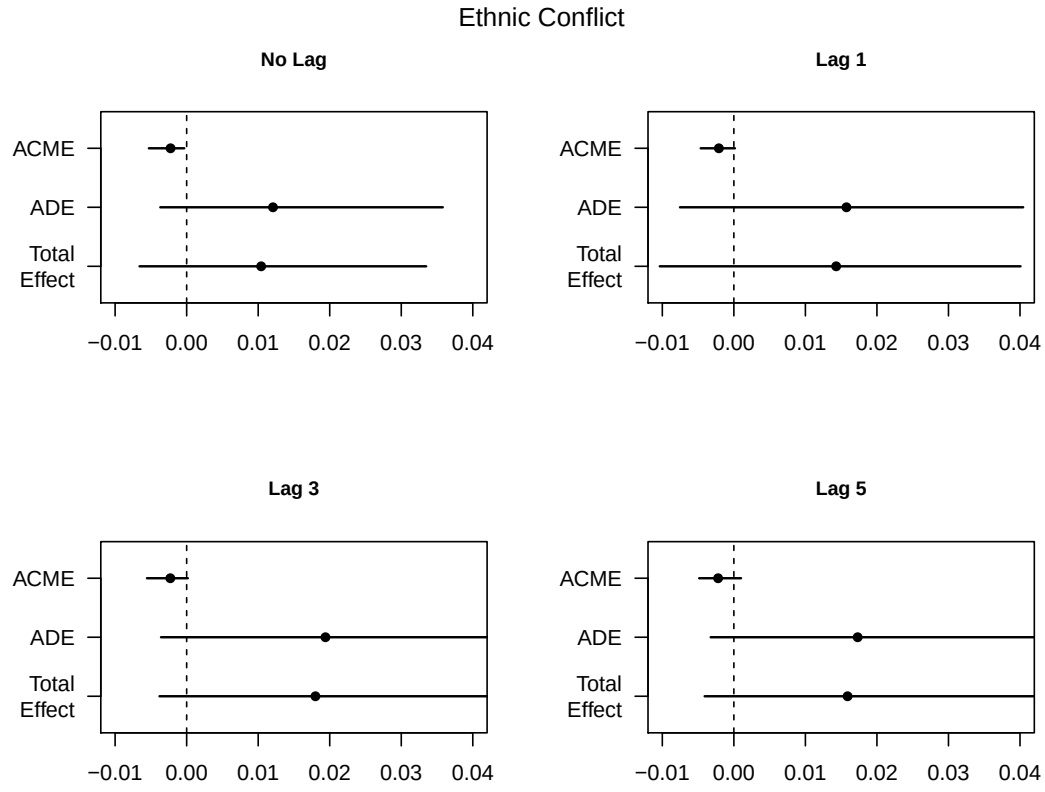


Figure C7: Replicating Effects of Dispersive Powersharing on Ethnic Conflict: Varying Lags.

results remain consistent with Hypothesis 2, but these findings are not as robust as those for H1a and H1b.

The estimated mediated effect of dispersive powersharing on ethnic territorial conflict is in the expected direction but short of statistical significance in the primary specification in the main text (Figure 5); Figure C7 shows that this estimated effect also falls short of statistical significance as the lag increases.

Notably, in the primary specifications (Figure 5, main text and upper left plot, Figures C7 & C8, we estimated direct effects and total effects that are substantively large, but not statistically significant. In Figure C7, these positive effects are statistically significant with a three year lag. This serves as a useful

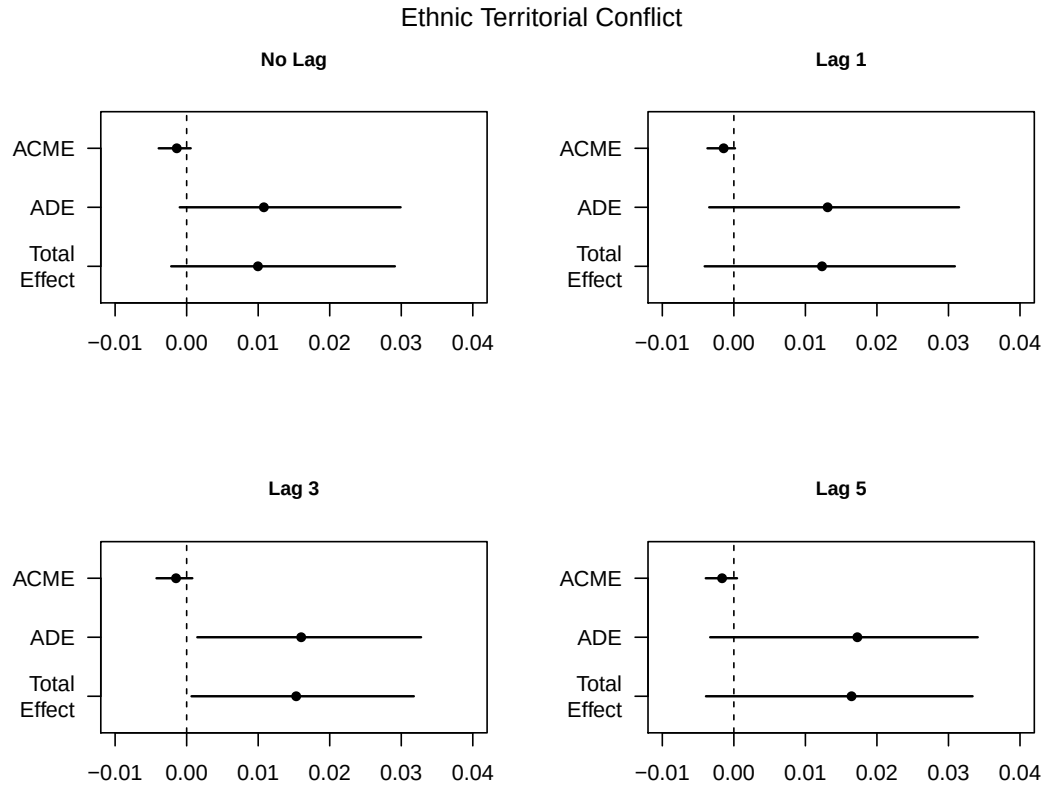


Figure C8: Replicating Effects of Dispersive Powersharing on Ethnic Territorial Conflict: Varying Lags.

reminder that, in the case of dispersive powersharing, the direct effect and mediated effects go in different directions. Our results suggest that *de jure* dispersive powersharing decreases the risk of conflict by granting regional autonomy to more groups, but the direct effect of these *de jure* institutions, which grant more power to regional governments in general, may actually lead to an overall increase the risk of ethnic conflict and ethnic territorial conflict.

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