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Why does ethnic partition foster violence? Unpacking the deep historical roots of civil conflicts

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Gaku Ito

Graduate School of Humanities and Social Sciences, Hiroshima University

Abstract

Recent literature highlights the role of historical events in shaping contemporary political and economic outcomes. This article joins the growing debate by utilizing disaggregated data and mediation analysis to explore the causal mechanisms bridging ethnic partition by modern international borders and the risks of postcolonial civil conflicts in Africa. I argue that split ethnic groups are more likely to experience armed conflicts with the central government during the postcolonial age, and the conflict-escalating effect is particularly acute for large-sized split groups. When coupled with sizable demographic forces, ethnic partition heightens the political salience of the corresponding ethnic cleavage while generating greater information and commitment problems. The empirical results provide considerable support for the theoretical claim: first, ethnic partition increases the likelihood of armed conflicts between politically excluded ethnic groups and incumbent; and second, a major part of the conflict-escalating effect is attributable to the indirect and causal interaction effects induced by contemporary group size. The empirical analysis also reveals that the role of the primary alternative mechanism, political discrimination against split groups, in generating the historical treatment effect remains limited.

Keywords

Africa, civil wars, colonial institutions, conflict, mediation analysis, spatial data

Does ethnic partition by political boundaries alter the risks of contemporary civil conflicts? A longstanding but under-investigated debate in conflict literature is the importance of Africa's arbitrary border design in shaping the risks of conflicts and other contemporary political outcomes. The arguably arbitrary designs of colonial borders often split or 'dismember' pre-existing ethnic groups across political boundaries, and put together or 'suffocate' distinct groups within boundaries of postcolonial states (Geertz, 1963). Empirical literature has examined the arbitrariness of African border designs (e.g. Green, 2012) and their impacts on postcolonial conflict and economic development (e.g. Alesina, Easterly & Matuszeski, 2011; Bosker & de Ree, 2014; Englebert, Tarango & Carter, 2002), mainly at the country level. Recent studies have rigorously extended the empirical investigation by exploiting the subnational-level variations across ethnic and political geography rooted in the colonial period (e.g. Michalopoulos & Papaioannou, 2014, 2016). For example, drawing on the ethnolinguistic map in Murdock (1959), Michalopoulos & Papaioannou (2016) examine how ethnic partition by modern political boundaries shapes the group-level risks of armed conflicts, interventions from neighboring countries, and political discrimination from central government during the postcolonial period.

These studies are a part of the broader literature on the long-run impacts of historical events on contemporary armed conflicts (e.g. Besley & Reynal-Querol, 2014; Paine, 2019; Wig, 2016; Wig & Kromrey, 2018) and economic development (e.g. Acemoglu, Johnson & Robinson, 2001; Michalopoulos & Papaioannou, 2013; Nunn, 2007, 2008). Yet, while highlighting the persistent nature of the causal effects, with several

Corresponding author:
gkit@hiroshima-u.ac.jp

exceptions (e.g. Nunn, 2007), underlying causal mechanisms remain relatively under-studied. Given the time gaps between when the treatment occurred (e.g. ethnic partition by colonial borders) and when the outcomes were observed (e.g. postcolonial conflict), the literature remains incomplete without explaining how the effect was realized (Acharya, Blackwell & Sen, 2016: 526). Indeed, unpacking the long-term effects requires not only leveraging the as-if random, largely exogenous nature of relevant historical shocks (e.g. arbitrary border designs), but also a careful examination of how posttreatment, as well as pretreatment, forces participate in the underlying causal pathways. One may expect, for example, that historical treatments alter contemporary determinants of the outcomes (e.g. demographic size of ethnic groups), thereby influencing the modern outcomes indirectly. Alternative explanations cannot be washed out without carefully examining such indirect effects and causal interactions induced by posttreatment factors.

This article aims to fill this gap by unpacking the causal mechanisms linking ethnic partition rooted in colonial border designs and postcolonial civil conflicts in Africa. It argues that split ethnic groups are more likely to experience armed conflicts with the central government, but the conflict-escalating effect is particularly acute for large-sized split groups. Intuitively, we would observe a systematic association between ethnic partition and civil conflict when rebels are mobilized along the ethnic cleavage *and* fail to strike a war-avoiding bargain with the incumbent. Although cultural cleavages do not necessarily matter for politics, certain cleavages retain political salience when coupled with sizable demographic forces by serving as effective 'vehicles' for political competition (Posner, 2004). Sizable and politically salient ethnic lines increase uncertainty over rebels' capabilities due to the acute collective action problem while serving as a pretext of outbidding. Moreover, as large-sized split groups serve as an effective basis of mobilization and political demands, such cleavages are likely to incentivize rebel entrepreneurs to take over the leadership to achieve their own political goals. Successful mobilization of large communities can also induce power shift and thereby undermine rebels' incentives to follow through prior agreements, which undercuts the credibility of the current leadership's commitments. These strategic interactions in turn invite bargaining failure by exacerbating information and commitment problems.

This article tests the theoretical claim against the empirical records of group-level conflict onset during the post-WWII period. Following previous studies, it relies on anthropological works and recent data construction efforts to combine the group-level traits during the precolonial, colonial, and postcolonial periods into a single georeferenced dataset. The estimation starts with investigating the persistent effects of ethnic partition on postcolonial conflicts, broadly following the setup of Michalopoulos & Papaioannou (2016). It departs from previous studies by systematically exploring the underlying causal mechanisms and the role of posttreatment variables, with close attention to contemporary demographic size and political salience of ethnic cleavages, via a causal mediation approach.

The major results are threefold. First, confirming the earlier finding in Englebert, Tarango & Carter (2002) and Michalopoulos & Papaioannou (2016), ethnic partition increases the likelihood of postcolonial civil conflicts. Second, and more importantly, the major part of the conflict-escalating effect of ethnic partition is mediated and conditioned by contemporary demographic size of split groups. Ethnic partition positively affects the risks of civil conflicts, but the effect is less visible for demographically small-sized groups. Finally, political discrimination against split groups, or an alternative explanation stressed in the literature, plays a minor role in generating the historical treatment effect.

These findings speak to the civil war literature as well as the growing literature on persistent nature of historical institutions. The empirical analysis reveals that historical treatment and contemporary, posttreatment factors shape the risks of civil conflicts in tandem. Although ethnic partition by colonial borders matters in altering postcolonial risks of civil conflicts, contemporary demographic size of ethnic groups, which itself is affected by the historical treatment, plays a critical role in generating the treatment effect. The analysis illuminates how careful decomposition of direct, indirect, and causal interaction effects helps us understand the historical roots of contemporary armed conflicts.

The empirical results also have implications for studies on the group-level determinants of civil conflicts. Previous studies have demonstrated how contemporary group-level characteristics such as political discrimination alter the risks of civil conflicts (e.g. Cederman, Wimmer & Min, 2010). This article presents a possible explanation for why some ethnic cleavages are relevant to domestic conflicts while others are not. Despite objective cultural differences, ethnic cleavages emerge as an important determinant of civil conflict only when coupled with significant demographic forces for political competition. This essentially mirrors the earlier case-based, quasi-experimental findings in Posner (2004) that the political salience of cultural differences in the domestic

political arena depends on groups' size and their utility as the vehicles for political competition. Cultural and historical factors shape civil conflicts, but material determinants such as demographic size also play a key role.

On the colonial origins of postcolonial conflict

Scholars have increasingly investigated the historical origins of contemporary armed conflicts. While suggestive, previous studies, with few exceptions, have paid less attention to the underlying causal pathways bridging historical treatments and contemporary outcomes.

Empirical patterns

An important finding in previous studies is the conflict-escalating effect of ethnic partitioning. The analysis in Englebert, Tarango & Carter (2002) is among the pioneering attempts exploring the long-run impacts of ethnic partition by colonial borders on the risks of contemporary armed conflicts. Drawing on the insights of Geertz (1963), Englebert, Tarango & Carter (2002) categorize the arbitrariness of African boundaries into 'dismemberment', or how borders partition pre-existing ethnic groups into different countries, and 'suffocation', or how borders bring together distinct pre-colonial groups within boundaries of postcolonial states. The country-level analysis demonstrates that dismemberment escalates international disputes while suffocation facilitates domestic conflicts, political instability, and secession movements. Focusing on the effects of dismemberment, Bosker & de Ree (2014) disaggregate civil conflicts into ethnic and non-ethnic conflicts and find that ethnic conflicts tend to spill over across borders and along ethnic lines. Leveraging geocoded datasets, Michalopoulos & Papaioannou (2016) take a closer look at the effect of ethnic partition on postcolonial conflicts. Their group-level analysis indicates that split groups are more likely to experience armed conflicts, interventions from neighboring countries, and political discrimination from the central government.

Causal pathways

While establishing the persistent nature of the causal effects, previous literature has paid relatively less attention to the mechanisms governing the long-run impacts of historical institutions. However, primarily due to the long temporal gaps between treatment (e.g. ethnic partition by colonial borders) and outcomes (e.g. postcolonial conflict), the literature remains incomplete without taking a closer look at underlying causal pathways (Acharya, Blackwell & Sen, 2016: 526). One may reasonably wonder, for example, how current political status of ethnic groups and group size mediate and condition the effect of ethnic partition on conflict risks. A key to such investigation is isolating the indirect and causal interaction effects induced by posttreatment factors in the total treatment effect.

Although limited, several possible mediating forces can be elaborated from the literature. A possible explanation for the conflict-escalating effect of ethnic partitioning focuses on the motivational forces. Englebert, Tarango & Carter (2002) and Michalopoulos & Papaioannou (2016) cite the irredentist and secessionist motivations as a key driving force bridging ethnic partitioning and domestic conflicts. Englebert, Tarango & Carter (2002: 1099–1100) argue,

> Postcolonial boundaries are believed to lead to domestic conflicts by their suffocating nature, that is [. . .] bringing together peoples that historically lived under different, if not inimical, systems. [. . .] Civil wars may also be affected by dismemberment, as partitioned people such as the Somali of Ethiopia's Ogaden region fight for secession.

While not all civil conflicts involve ethnic partition or border issues, these irredentist and secessionist motivations can be amplified by the political status of ethnic groups in postcolonial states. Indeed, Michalopoulos & Papaioannou (2016) highlight that split groups are more likely to be politically discriminated from the central government as well as engage in violent interactions with the incumbent. Split groups are often entrapped in a vicious cycle of political discrimination and armed conflicts, such that ethnic partitioning by colonial borders leads to political discrimination, which in turn motivates the discriminated ethnic groups to turn to armed uprising against central government.

Although the motivational forces are not sufficient to cause ethnic conflicts (Cederman, Wimmer & Min, 2010: 96), partitioned groups may also enjoy lower opportunity costs for armed uprising. Along this reasoning, Michalopoulos & Papaioannou (2016) emphasize the role of lower opportunity costs that split groups enjoy in facilitating rebellion against the central government. The members of partitioned ethnic groups obtain arms and other forms of assistance from the coethnics in neighboring countries, which leads to the observation that 'quite often episodes of repression lead to civil wars, as partitioned groups have a lower opportunity cost of conflict' (Michalopoulos & Papaioannou, 2016: 1810). As I turn to later, the discussion leads to a

possible channel linking ethnic partition and civil conflict: ethnic partition leads to the increased risks of postcolonial conflicts due to discrimination from the incumbent, coupled with amplified motivations and lower opportunity costs.

Ethnic partition, mobilization, and bargaining

Some civil conflicts are fought along cultural cleavages while others are not. Ethnic partition rooted in colonial borders creates easily observable cultural cleavages, but such cleavages do not necessarily emerge as a primary determinant of postcolonial politics (Dunning & Harrison, 2010; Posner, 2004). Moreover, heightened salience of a certain cleavage alone remains insufficient to cause inefficient fighting, as war imposes otherwise unnecessary costs on both disputants (Fearon, 1995). Therefore, we would observe a systematic association between ethnic partition and civil conflict when rebels tend to mobilize along the ethnic lines *and* fail to strike a war-avoiding bargain with the incumbent.

An explanation of the partition–conflict association similarly needs to be two-fold: first, when ethnic partition matters in shaping political mobilization, and second, why increased political salience of particular cleavages, in some situations, is followed by inefficient fighting. Rather than political discrimination, this article postulates that split ethnic groups are more likely to experience armed conflict with the central government, but the conflict-escalating effect is particularly acute for demographically large-sized groups.

There are two core building blocks behind the claim: ethnic mobilization and bargaining failure. First, demographic size of corresponding cleavages is the key determinant of political salience of observable cultural traits. As Horowitz (1985: 40) argues, 'whether ethnic politics is more parochial or more central is mainly a function of group size relative to state size'. Indeed, an important consequence of ethnic partition by colonial border design is that it alters the 'salience of the preexisting cultural cleavages *within* African countries' (Posner, 2004: 543, emphasis in original). Although cultural cleavages do not necessarily matter for politics, specific cleavages retain political salience when they are demographically large enough to be effective 'vehicles' for political competition (Posner, 2004: see also, Brubaker & Laitin, 1998). Both ethnic entrepreneurs and group members would have little incentive to instrumentally exploit ethnic lines and ethnicize political struggle when ethnic framing remains less profitable for achieving political power. Coupled with sizable demographic forces, ethnic cleavages with cross-border partition, among other domestic cleavages, emerge as an easily observable, effective instrument for political mobilization.[1]

Second, politically salient ethnic cleavages, paired with significant demographic forces, can lead to inefficient fighting by generating greater bargaining problems. The core obstacles to war-avoiding bargains include asymmetric information combined with incentives to misrepresent private information and the inability to credibly commit to war-avoiding agreements (Fearon, 1995; Powell, 1999, 2006). An important source of uncertainty is rebels' collective action problem. The collective action paradigm asserts that effective mobilization is harder for larger groups because they suffer more from free riders (Olson, 1965). Facing the challenge, rebel leaders commonly employ material and social incentives and coercive means selectively to mobilize resources and followers to fight (Eck, 2014; Lichbach, 1995). As such incentive provision efforts and their effects on mobilization remain hardly observable, the likely presence of acute collective action problems increases uncertainty about rebels' stated capabilities. Moreover, when group size is large enough to sustain multiple movements representing the population, it is more likely that different opposition movements make separate demands based on their stated capabilities and resolve, which further increases uncertainty over the reversion points (Cunningham, 2013: 663).

These sources of uncertainty create severe challenges for the government during the bargaining process. Observable, sizeable, and politically salient ethnic lines, consistent with rebels' capabilities or not, and 'invented' during the colonial era or not (Ranger, 2012), serve as a pretext for ethnic outbidding. Claiming the interests of the same, politically salient, and sizeable pool of constituencies, both weaker and stronger rebels have strategic incentives to make larger demands to gain concessions from the incumbent under uncertainty over rebels' actual demands. The informational asymmetries combined with incentives to misrepresent create strategic problems by making it more challenging for the incumbent to infer the reversion point that will settle the dispute. Even in such environments, the incumbent can in principle obtain a peaceful settlement by always granting

[1] Additional incentives for ethnic mobilization arise from the needs to limit the winners' coalition size not to dilute each winner's share (Fearon, 1999). Each winner's benefit can rapidly decrease if 'pork' is allocated based on a criterion that can easily be chosen by individuals such as party affiliation. The ascriptive mark of ethnicity serves as a criterion that can hardly be chosen by individuals.

extensive concessions that would satisfy stronger rebels. Such offers, however, undermine the incumbent interests when rebels are weak, which in turn invites a war-causing, risk–return trade-off (Powell, 1999).

Large-sized and politically salient ethnic cleavages can be conflict-provocative by affecting groups' ability to credibly promise to follow through peace deals. The political salience of a particular cleavage does not necessarily indicate the presence of uncontested leadership to monitor and constrain the behavior of group members. Even if leadership promises not to renege on a prior agreement, the incumbent would have little reason to expect the commitment to be enforced over time if the leadership lacks an effective control over the group population (Cunningham, 2013; Wig, 2016).

Politically salient ethnic cleavages with cross-border splits facilitate the fear of the incumbent about future threats of continuous challenges from split groups. Large-sized, observable cultural cleavages continuously incentivize rebel entrepreneurs to take over the leadership if doing so helps mobilize resources to achieve their own political goals. Moreover, successful mobilization and collective action can also induce a power shift between rebels and the incumbent and thereby generate incentives to renege on prior agreements. These probable future pathways undercut the credibility of the current leadership's commitments and make the incumbent reluctant to strike war-avoiding deals, leading to inefficient fighting. Such fear of tomorrow should remain uncoupled with ethnic partition when split groups remain demographically too small to facilitate mobilization along ethnic lines.

Combined, I expect that ethnic partition rooted in the colonial age acquires political salience and is followed by armed conflicts when coupled with sizable demographic forces. The heightened salience of large-sized ethnic cleavages in turn increases risks of civil wars by exacerbating information and commitment problems. When the corresponding group size remains small, other discernible cleavages, rather than ethnic lines, are likely to shape political competition in the domestic arena, leaving the partition–conflict association negligible.[2]

[2] An important limitation in the current empirical analysis is that it does not allow for distinguishing the two bargaining logics outlined above. The core component of the argument here is that ethnic partition leads to fighting only when coupled with sizable demographic forces by shaping the bargaining problems.

Testable predictions

The discussion generates several testable predictions. First, regardless of the underlying mechanisms, we would see a conflict-escalating effect of ethnic partition such that split groups, compared to non-split groups, are more likely to fight against the incumbent.

> *Hypothesis 1:* Ethnic partition increases the risk of an armed conflict between split ethnic groups and the central government.

Turning to causal pathways, the insights from previous studies lead us to expect ethnic partition to increase the risks of postcolonial conflict by the discrimination channel.

> *Hypothesis 2:* Ethnic partition has a stronger positive effect on the risk of an armed conflict for politically discriminated ethnic groups.

In contrast, this article stresses the role of group size. Ethnic partition by colonial borders makes split groups more conflict-prone, but the contemporary demographic size plays a key role in determining the treatment effect. Without significant demographic forces, ethnic partition would not turn out to be politically salient or conflict-provocative.

> *Hypothesis 3:* Ethnic partition has a stronger positive effect on the risk of civil conflicts for large-sized ethnic groups.

Research design

Any empirical investigation into the persistent impacts of historical events necessarily addresses two methodological concerns: non-random treatment assignment and the role of posttreatment variables in the causal pathway. This section first describes the coding of the key variables, and then introduces the empirical strategy to account for these concerns.

Data and measurement

Following previous studies, I utilize the ethno-linguistic map developed by Murdock (1959) and the shapefile provided by Nunn (2008) to measure the spatial patterns of ethnic settlement in Africa during the colonial period (primarily the 1860–1940 period). As in Figure 1(a), Murdock (1959) lists 843 unique regions with distinct ethnic settlements. I dropped from the dataset those regions coded as 'uninhabited'. This sample restriction

(a) Ethnic boundaries in Murdock (1959) (b) Ethnic partition by borders

Figure 1. Murdock's ethnic boundaries and ethnic partition by modern political boundaries

(a) Ethnolinguistic map in Murdock (1959), shapefile provided by Nunn (2008). (b) Split (dark gray) and non-split groups (light gray). Data sources: Nunn (2008) and Weidmann et al. (2010).

and additional list-wise deletion due to missing values in the variables below leave 825 unique groups.

Ethnic partition

The coding of ethnic partition broadly replicates the procedure in Michalopoulos & Papaioannou (2014, 2016) that overlays contemporary international borders onto Murdock's map. The binary treatment variable, *Partition*, is coded 1 for the groups with their traditional settlement areas split into more than one country and 0 otherwise. Figure 1(b) maps the resultant distribution of ethnic partition, with 213 (25.8%; *Partition* = 1) split and 612 (74.2%; *Partition* = 0) non-split ethnic groups.[3] To account for potential spatial externalities (Michalopoulos & Papaioannou, 2016), I also measure *Spillover*, or the fraction of split neighboring ethnic groups relative to total neighbors.

Unit of analysis

I rely on the ETH Ethnic Power Relations (EPR) Family datasets to measure contemporary characteristics of ethnic groups and conflict onset (Cederman, Wimmer & Min, 2010; Vogt et al., 2015). The unit of analysis is EPR group-year, matched with Murdock's traditional groups and nested by host countries where individual groups are located. Drawing upon an expert survey, the EPR 2018 dataset identifies roughly 820 politically relevant ethnic groups in all states that pass the 250,000 inhabitant threshold during the 1946–2017 period. An ethnic group is coded as 'politically relevant' if 'at least one political organization has claimed to represent its interests at the national level or if its members are subjected to state-led political discrimination' (Vogt et al., 2015: 1329). The dataset comes with detailed group-level information including demographic size and access to state power ranging from 'total control of the government' to 'overt political discrimination'. I matched the ethnic groups in Murdock (1959) to the EPR dataset based on the corresponding table developed by Michalopoulos & Papaioannou (2016: see also,-Wig, 2016). Allowing multiple matches, the corresponding table identifies 679 links between Murdock's (1959) ethnic groups and EPR groups.

The primary focus of the current analysis is on the interactions between central government and the ethnic groups excluded from or not represented in the incumbent coalitions, rather than 'infighting' or coups within incumbent (Wimmer, Cederman & Min, 2009: 321–322). The EPR dataset classifies the politically relevant ethnic groups into three major categories: (1) 'controls power alone', (2) 'shares power with other ethnic

[3] Following Michalopoulos & Papaioannou (2016: 1812), I used a threshold rule such that *Partition* is coded 0 if more than 90% of the settlement area falls within a single country. As in Online appendix C, a 95% threshold yields similar results. Michalopoulos & Papaioannou (2016) recognize 229 split groups. Possible reasons for the difference of $229 - 213 = 16$ groups include different border datasets and projection methods.

groups', or (3) 'excluded from executive state power' (Vogt et al., 2015: 1331).[4] Following Wig (2016), our dataset contains the groups in the third category and not represented in the central government. The list-wise deletion due to missing values in the variables below leaves 10,246 group-year observations with 239 unique groups (278 country-groups) in Murdock (1959) that were excluded from state power at some point during the study period.

One may wonder if ethnic partition affects political exclusion, and the criteria induce sample selection (collider) bias. Running counter to this concern, Table A.IV in the Online appendix fails to reveal a systematic association between *Partition* and political exclusion. The criteria allow for clarifying the scope of the analysis without inviting significant bias.

Conflict onset

The dependent variable, *Onset*, is a binary indicator of the group-level onset of postcolonial conflict derived from the EPR Family's ACD2EPR component. Based on the entries of internal armed conflicts in the Uppsala Conflict Data Program Armed Conflict dataset (Gleditsch et al., 2002), the ACD2EPR dataset defines a link between rebel organizations and ethnic groups by ethnic claim and recruitment, which is coded 1 if an organization makes ethnic claims and significantly recruits from the respective ethnic group (Cederman, Wimmer & Min, 2010: 101–102). *Onset* is coded 1 for an ethnic group if a rebel organization linked to the group enters an ACD conflict in year t and 0 otherwise. Of the 10,246 group-year observations, 175 (1.7%) are coded 1.

Pretreatment covariates

I employ two series of group-level features as pretreatment covariates. First, precolonial socio-economic conditions are primary determinants of colonial borders (Green, 2012; McCauley & Posner, 2015). I rely on the HYDE data version 3.1 (Kees et al., 2011) to measure (logged) precolonial population count within ethnic homelands *Precolonial population* (averaged over the 1400, 1500, 1600, 1700, and 1800 estimates). Logged percentages of cropland (*Precolonial cropland*) and grassland (*Precolonial grassland*) within settlement areas are similarly constructed by the HYDE data to measure agricultural potential as a proxy of economic prosperity. I also control for the distances in logged kilometers from the locations of historical kingdoms and conflicts to settlement locations to consider local realities of precolonial politics (Besley & Reynal-Querol, 2014; data taken from Michalopoulos & Papaioannou, 2016). Second, geographic factors also alter ethnic partition and contemporary conflict. The analysis therefore includes logged mean *Elevation* and *Ruggedness* (Shaver, Carter & Shawa, 2019; USGS, 1996), a binary indicator for the presence of lakes and rivers in settlement areas (*Water body*), distance from coastlines in logged kilometers (*Coast distance*), and total settlement areas in logged square kilometers (*Total area*).

Posttreatment variables

The analysis also accounts for contemporary or posttreatment variables via mediation analysis techniques. The primary mediator, *Group size*, is coded by multiplying the annual relative group size in the EPR dataset with the total population size of host countries in thousands (Gleditsch, 2002). As an initial robustness check, I replicate the analysis with an alternative group size measure, *Demographic balance*, or a fraction of group demographic size relative to the host country's total population. To evaluate the discrimination mechanism, I employ a binary indicator of political discrimination by central government, *Discrimination*, as the mediator. *Discrimination* takes the value of 1 if an ethnic group is coded as 'discriminated' in the EPR's power access variable.[5]

Other posttreatment variables capture socio-economic and geographical traits. Socio-economic variables include group population and cropland/grassland coverages in the late colonial period (*Population*, *Cropland*, and *Grassland* in 1940; Kees et al., 2011). Similarly, geographic variables include settlement area (in logged km^2 within host countries, *Area*) and distances from borders and capitals (*Border distance* and *Capital distance*). Country-level variables are per capita *GDP*, logged country population size (*Total population*), and logged years since independence (*State age*), constructed from Gleditsch (2002).

[4] The third category consists of three subcategories: 'powerless', 'discriminated', and 'self-exclusion' (Vogt et al., 2015: 1331). Note that the group status of political exclusion or represented in the central government coalitions does not immediately imply ethnic discrimination ('discriminated' status).

[5] Among politically excluded ethnic groups, the (sub)category of 'discriminated' indicates 'an active, intentional, and targeted discrimination by the state against group members in the domain of public politics' (Vogt et al., 2015: 1331).

Methodological challenges and identification strategy

Non-random treatment assignment

The methodological challenges here are twofold. First, despite the arbitrariness of African border design, the treatment may not be assigned randomly across ethnic groups (McCauley & Posner, 2015: 411). Empirical findings in previous studies also suggest that the determinants of colonial border designs include precolonial population size and geographical inaccessibility (e.g. Green, 2012; Michalopoulos & Papaioannou, 2016), which corresponds to the established determinants of contemporary civil conflicts (e.g. Collier & Hoeffler, 2004; Fearon & Laitin, 2003).

To guard against omitted variable bias, I draw on georeferenced data to examine imbalance between the treated (split) and control (non-split) groups across observed pretreatment covariates. I then evaluate the average treatment effect of *Partition* on postcolonial civil conflict via regression models controlling for observed confounders. The analysis also pays careful attention to unobserved confounding with formal sensitivity analysis techniques.

Posttreatment bias

The second challenge emerges from posttreatment variables. A common approach to deal with posttreatment variables such as contemporary group size is adding them to the right-hand side of regression models. Yet accounting for posttreatment variables is not straightforward because simply including these variables in regression models can invite biased estimates of treatment effects (Rosenbaum, 1984). In particular, the likely presence of the intermediate confounders, or the posttreatment variables affected by treatment while confounding the mediator-outcome association, further complicates the estimation by inducing 'intermediate variable bias', a special case of posttreatment bias (Acharya, Blackwell & Sen, 2016). On the one hand, omitting intermediate confounders invites omitted variable bias regarding the mediator's effect because they confound the mediator–outcome relationship. On the other hand, conditioning on these variables leads to posttreatment bias by blocking the causal pathway that 'Treatment → Intermediate confounders → Outcome', which constitutes a component of the controlled direct effect of a treatment specified below (Acharya, Blackwell & Sen, 2016: 518). For example, contemporary group size mediates and conditions the treatment effect of ethnic partition on postcolonial civil conflicts. At the same time, other posttreatment variables, such as political discrimination, affected by the treatment are likely determinants of contemporary group size.

Note that even in the presence these posttreatment variables, the *total* effect or (sample) average treatment effect (ATE) is identified provided that treatment assignment is ignorable given pretreatment covariates. Yet examining underlying mechanisms requires us to decompose the total effect and evaluate the role of posttreatment variables.

Model specification

Total effect estimate

The analysis relies on two series of estimations. First, I regress onset of contemporary civil conflict on the treatment and pretreatment covariates above to derive the model-based ATE estimate. All models include country fixed effects, a quadratic polynomial of geographic coordinates of settlement areas, and cubic polynomials of calendar year and group-specific years since last conflict to screen out the unobserved heterogeneity at cross-section and temporal levels. Specifically, I rely on the specification that

$$\mathbb{E}[Y_{ijt}] = \alpha_j + \tau D_{ij} + \boldsymbol{X}_{ij}\boldsymbol{\beta} + f_1(h_{ijt}) + f_2(s_{ij}) + f_3(t), \quad (1)$$

where Y_{ijt} is a binary indicator of conflict onset linked to group i in country j in year t, D_{ij} is the binary indicator of ethnic partition, and $\boldsymbol{X}_{ij}$ denotes a vector of covariates. α_j represents country fixed effects, and $f_1(h_{ijt})$, $f_2(s_{ij}) = f_2(\text{lon}_{ij}, \text{lat}_{ij})$, and $f_3(t)$ are polynomials of peace years, settlement locations, and calendar year.

Mediation analysis

Second, I utilize the sequential *g*-estimator (Acharya, Blackwell & Sen, 2016; Joffe & Greene, 2009; Vansteelandt, 2009) to decompose the total causal effect while accounting for the concern of posttreatment bias. In the first stage, the estimator derives the mediator's impact on outcome adjusting for treatment and intermediate as well as pretreatment confounders. It then transforms the outcome variable by subtracting the estimated mediator's effect. Finally, it derives the average controlled direct effect (ACDE) proportion of the overall treatment effect by regressing the transformed outcome on the treatment and pretreatment confounders. Importantly, this approach allows for conditioning on intermediate

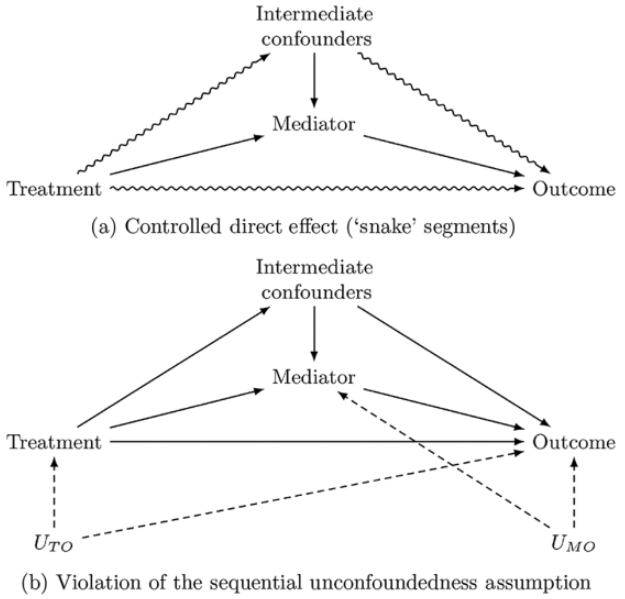

Figure 2. Controlled direct effect and the sequential unconfoundedness assumption

(a) 'Snake' segments represent the controlled direct effect. Pretreatment covariates are omitted for brevity (assuming that we include enough variables). (b) Dashed segments indicate the violation of the sequential unconfoundedness assumption (Acharya, Blackwell & Sen, 2016: 516, 519–520, Figure 3).

confounders while flexibly modeling the treatment–mediator interaction.

The quantity of interest is the ACDE represented by the 'snake' segments in Figure 2(a), which measures the treatment effect with a mediator set to the same value for all units, or the treatment effect *not* mediated or conditioned by the mediator. Following the notation in Acharya, Blackwell & Sen (2016), the ACDE of treatment D with mediator M fixed at m can be expressed as the difference between two potential outcomes with treatment values d and d' (e.g. *Partition* = 1 and 0),

$$\text{ACDE}(d, d', m) = \mathbb{E}[Y(d, m) - Y(d', m)]. \quad (2)$$

In the current context, the ACDE measures the ceteris paribus average direct effect of *Partition* with mediator *Group size* or *Discrimination* fixed at value m for all observations. In other words, the ACDE quantifies the counterfactual effect of *Partition* on conflict onset, for example, with demographic size of ethnic groups set to the sample mean.

Specifically, I include *Group size* and its squared term to allow the treatment effect to vary across mediator values and account for possible non-linear association in the first stage:

$$\mathbb{E}[Y_{ijt}] = \alpha_j + \tau D_{ij} + \Gamma(D_{ij}, M_{ijt}; \boldsymbol{\gamma}) + \boldsymbol{X}_{ij}\boldsymbol{\beta} + \boldsymbol{Z}_{ijt}\boldsymbol{\eta} + \boldsymbol{Z}_{jt}\boldsymbol{\psi} + f_1(h_{ijt}) + f_2(s_{ij}) + f_3(t), \quad (3)$$

$$\Gamma(D_{ij}, M_{ijt}; \boldsymbol{\gamma}) = \gamma_1 M_{ijt} + \gamma_2 M_{ijt}^2 + \gamma_3 M_{ijt} D_{ij} + \gamma_4 M_{ijt}^2 D_{ijt}, \quad (4)$$

where $\boldsymbol{Z}_{ijt}$ and $\boldsymbol{Z}_{jt}$ are group- and country-level intermediate confounders.[6]

Analogous to an instrumental variable approach, the *g*-estimator then transforms the *outcome* variable by subtracting the mediator's effect to construct counterfactual outcomes:

$$\widetilde{Y}_{ijt} = Y_{ijt} - \widehat{\Gamma}(D_{ij}, M_{ijt}; \widehat{\boldsymbol{\gamma}}). \quad (5)$$

Because the mediator's role has been blipped-down, the remaining covariation between transformed outcome $\widetilde{Y}_{ijt}$ and treatment is attributable to the direct effect *not* through or interacted with the mediator. The ACDE is then obtained by regressing the transformed outcome on the treatment and other right-hand side variables in Equation 1.

I primarily rely on linear probability models (LPMs) which are consistent with the mean difference framework (Angrist & Pischke, 2008: 102–107). To address the concern for clustering at cross-section and temporal levels, I report standard errors robust to multiway clustering on Murdock ethnic groups, countries, and years (Cameron, Gelbach & Miller, 2011). As a first-cut robustness check, I also report the estimation with an alternative group size measure, *Demographic balance*, or logged fraction of group population relative to host country's total population.

The key identification assumption here is the sequential unconfoundedness assumption, or a two-fold no omitted variable assumption illustrated in Figure 2(b) (Acharya, Blackwell & Sen, 2016: 519). The assumption requires that there are no omitted variables for (1) the treatment–outcome relationship given pretreatment covariates (Treatment $\leftarrow U_{TO} \rightarrow$ Outcome) and (2) the mediator–outcome association given pretreatment covariates, treatment, and intermediate confounders (Mediator $\leftarrow U_{MO} \rightarrow$ Outcome). Although this assumption

[6] *Discrimination* is included in $\mathbf{Z}_{ijt}$ in the baseline estimation. In the estimation with *Discrimination* as the mediator, the first-stage model is estimated with *Group size* included in Z_{ijt} and $\Gamma(D_{ij}, M_{ijt}; \gamma)$ is specified as $\Gamma(D_{ij}, M_{ijt}; \gamma) = \gamma_1 M_{ijt} + \gamma_2 M_{ijt} D_{ij}$ as both treatment and mediator are binary.

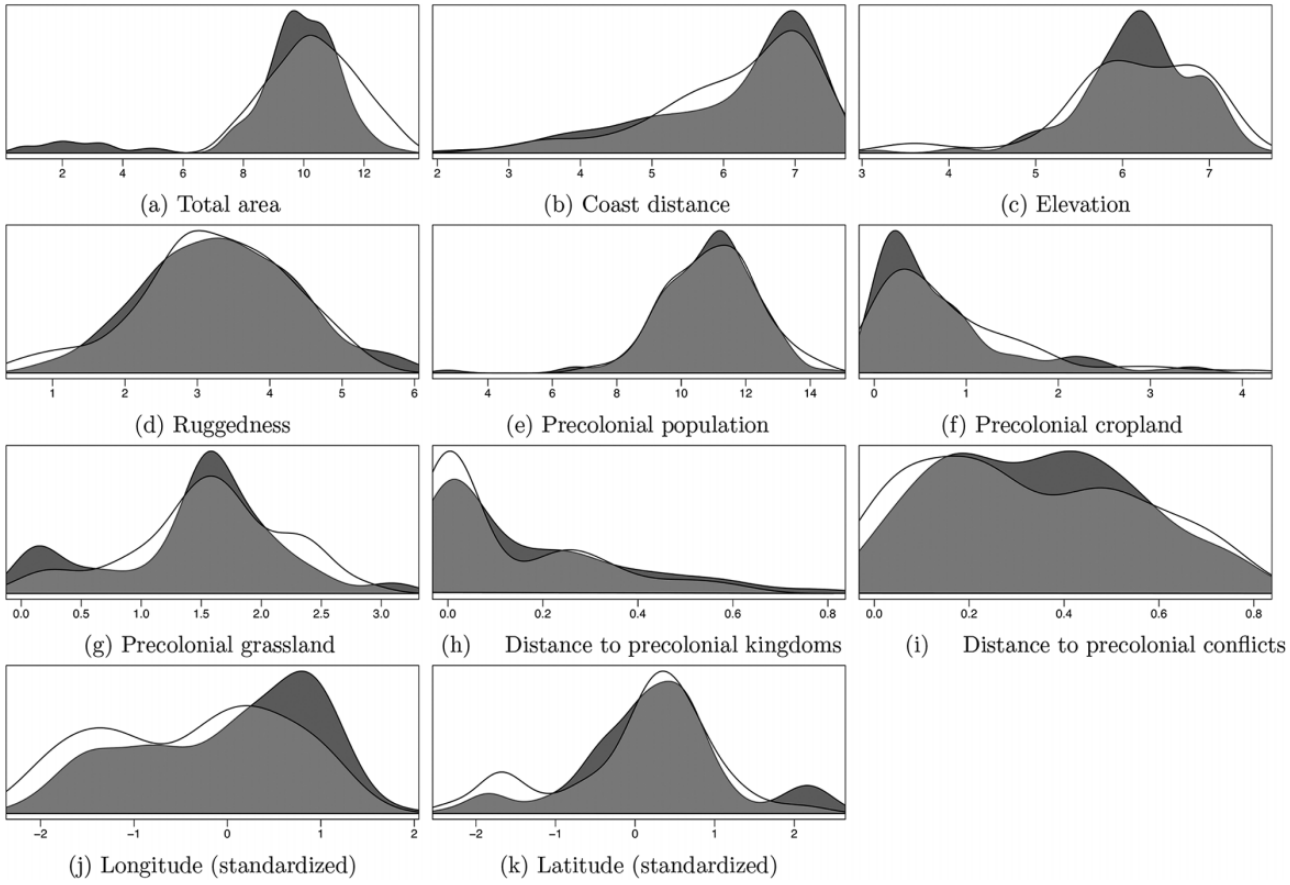

Figure 3. Density plots for the pretreatment covariates

The light gray density curves represent the distributions of pretreatment covariates for the treated (split) ethnic groups, whereas the dark gray curves indicate the covariate distributions of control (non-split) groups.

cannot be tested directly, the sensitivity analysis in Online appendix B.2 sheds light on its validity.

ACDE and mechanisms

The difference between the ATE and ACDE, or $\Delta\tau = \text{ATE} - \text{ACDE}$, serves as a summary indicator of a mediator's role in a causal mechanism. $\Delta\tau$ can further be decomposed into (1) the indirect effect through the mediator and (2) the causal interaction – how the direct effect of the treatment depends on the mediator at the individual level (Acharya, Blackwell & Sen, 2016: 517). Although these two components cannot be distinguished without further assumptions, the ACDE and $\Delta\tau$ speak to the causal mechanisms in two ways (Acharya, Blackwell & Sen, 2016: 517–518). First, as $\Delta\tau$ summarizes the role of a mediator, a non-zero $\Delta\tau$ provides support for the corresponding causal pathway. For example, we would see a non-zero $\Delta\tau$ for group size if it significantly participates in the causal pathway linking ethnic partition and conflict. Second, the ACDE, which combines all alternative mechanisms, also allows for ruling out alternative mechanisms. A non-zero ACDE implies that at least some proportion of treatment effect works through another set of mediators or mechanisms. In the current context, we would see a non-zero ACDE if the treatment effect of ethnic partition is *not* due to the group size mechanism exclusively.

Results

I report the main findings in two steps. First, I investigate the total effect of ethnic partition on postcolonial civil conflicts. I then utilize the sequential *g*-estimator to explore the underlying causal pathways.

Results I: Total effect

Covariate balance

As Michalopoulos & Papaioannou (2016: 1813) correctly point out, the key to deriving unbiased estimates of the partition effect is the absence of omitted

Table I. Total effect of ethnic partition

	Dependent variable: Onset			
	(1) Restricted LPM	*(2) Full LPM*	*(3) Full LPM w/ Spillover*	*(4) Full logit*
Partition	0.0088***	0.0082***	0.0077***	0.3512**
	(0.0032)	(0.0028)	(0.0026)	(0.1751)
Pretreatment covariates		✓	✓	✓
Country FEs	✓	✓	✓	✓
Calendar year polynomial	✓	✓	✓	✓
Peace year polynomial	✓	✓	✓	✓
Spatial polynomial (lon, lat)	✓	✓	✓	✓
Spillover			✓	
Observations	10,246	10,246	10,246	10,246
Adjusted R^2	0.0150	0.0174	0.0174	
Residual std error	0.1286	0.1284	0.1284	
	(df = 10,203)	(df = 10,193)	(df = 10,192)	
Log likelihood				−753.611
AIC				1,613.222

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. Robust standard errors with multiway clustering on ethnic groups, countries, and years in parentheses.

systematic differences between treated (split) and control (non-split) ethnic groups across pretreatment covariates that confound the partition-conflict association. Figure 3 and Tables A.I and A.II in the Online appendix report the balance statistics. Although we see some imbalance in the covariate central tendencies between the two groups, the kernel density plots in Figure 3 confirm that the distributions of observed pretreatment covariates of the two groups exhibit a fair balance without any adjustments. The two density curves in each panel indicate the covariate distributions of the treated (light gray) and control ethnic groups (dark gray), respectively. While not perfect, the large overlaps of the density curves suggest that the treated and control cases are generally balanced across observed covariates.

Table A.III in the Online appendix reports a series of regression estimates to evaluate how the pretreatment variables shape ethnic partition. In addition to geographic size of ethnic homelands stressed in Michalopoulos & Papaioannou (2016), although statistical significance varies, *Precolonial cropland*, *Elevation*, and *Ruggedness* are consistently and positively associated with the probability of ethnic partition, while *Precolonial population* yields a negative effect. As these group traits may also be correlated with contemporary conflicts, the following analysis carefully accounts for these potential confounders.

Partition effect

I expect the overall effect of ethnic partition on conflict onset to be positive. Model 1 in Table I is a minimal LPM with the treatment, fixed effects, and temporal and spatial polynomials, while Model 2 includes all pretreatment covariates. Model 3 accounts for spatial externalities by incorporating *Spillover*, and Model 4 re-estimates the specification of Model 2 with a logit estimator for a comparative purpose.

The baseline total effect estimate is consistent with the earlier findings of Michalopoulos & Papaioannou (2016). Across specifications, the coefficient for *Partition* remains remarkably stable and positively signed while retaining the statistical significance at the conventional 5% level. Substantively, the estimates indicate that ethnic partition results in a sizable increase in the probability that a politically excluded ethnic group experiences conflict with the incumbent in the postcolonial period, with the effect size of 0.82 percent point (95% confidence interval: 0.0027, 0.0136; Model 2). As in Table A.V in the Online appendix, the conflict-escalating effect remains stable when conflict types are disaggregated into territorial and governmental conflicts.

Confounding

Remaining methodological concerns include model dependence and unobserved confounding. If the total effect estimate suffers from these issues, the following mediation analysis would remain inconclusive at best. First, Online appendix B.1 addresses model dependence by estimating the empirical distribution of the ATE estimates obtained from every possible model specification where different combinations of ten pretreatment covariates enter the model with *Partition*, country fixed

Table II. Direct effect of *Partition* net *Group size*

	Dependent variable:			
	Onset			*Group size*
	(1) Naive LPM w/X and M	*(2) Naive LPM w/X, M and Z*	*(3) Sequential g-estimation*	*(4) OLS*
Partition	0.0049	0.0018	0.0034	–0.3480***
	(0.0036)	(0.0045)	(0.0043)	(0.1174)
Bootstrap			✓	
Pretreatment covariates	✓	✓	✓	✓
Mediator	✓	✓	✓ (first stage)	(DV)
Intermediate confounders		✓	✓ (first stage)	✓
Country FEs	✓	✓	✓	✓
Calendar year polynomial	✓	✓	✓	✓
Peace year polynomial	✓	✓	✓	✓
Spatial polynomial	✓	✓	✓	✓
Observations	10,246	10,246	10,246	10,246
Adjusted R^2	0.0176	0.0235	0.0155	0.7955
Residual std error	0.1284	0.1280	0.1284	0.6794
	(df = 10,189)	(df = 10,179)	(df = 10,193)	(df = 10,183)

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. *Group size* is recentered to its mean. Robust standard errors with multiway clustering on ethnic groups, countries, and years in parentheses. Bootstrapped standard error is reported for sequential g-estimation.

effects, and polynomial terms. The mean of the empirical distribution from $2^{10} = 1,024$ model specifications is 0.0083 (95% CI: 0.0069, 0.0098) and almost identical to the point estimate of 0.0082 in Model 2. Second, to address the concern of unobserved confounding, Online appendix B.2 utilizes the 'E-value', a conservative bounding factor for sensitivity (bias) analysis (Ding & VanderWeele, 2016; VanderWeele & Ding, 2017). The E-value represents the minimum strength of unobserved confounding to invalidate a treatment–outcome association. The estimates reveal that the effect of unobserved confounding needs to be at least *stronger* than that of the observed covariates to overturn the reported treatment effect. Collectively, these results suggest that the setup serves as a reasonable reference for mediation analysis.

Results II: Mechanisms

Baseline ACDE

The robust total effect estimate provides a relevant basis for further analysis to unpack the underlying mechanisms. In the baseline setting, I recentered *Group size* to its mean value as a point of comparison. Thus the estimate identifies the ACDE of ethnic partition on postcolonial conflict in the counterfactual that group size is set to the mean value for all ethnic groups. Recall that the core theoretical prediction here stresses the role of contemporary group size in generating the effect of ethnic partition. While the total effect is positive, the ACDE is expected to be negligible once accounting for the indirect and causal interaction effects through contemporary demographic size.

Table II reports a series of estimates obtained from different models: two naive LPMs simply conditioning on mediator and/or intermediate confounders as well as pretreatment covariates (Models 1 and 2) and sequential *g*-estimator (Model 3). Table A.VI in the Online appendix reports the summary statistics of the posttreatment variables. Following Acharya, Blackwell & Sen (2016), the uncertainty estimate for the sequential *g*-estimation was obtained by 1,000 bootstrapped replications to account for the increased error due to the two-stage estimation. Note that an underlying assumption of our theoretical account is that ethnic partition systematically influences contemporary demographic size of ethnic groups. To validate the assumption, Model 4 regresses (logged) *Group size* on *Partition*, X, and Z.

The estimates provide initial support for Hypothesis 3. While the overall treatment effect is strongly positive, the direct effect of ethnic partition turns out to be negligible once accounting for the effect of contemporary group size (mediator), indicating the substantial role of group size in the underlying causal pathway. Although the two naive models are likely to suffer from posttreatment bias and intermediate variable bias, Models 1 to 3 yielded qualitatively similar estimates. The sequential

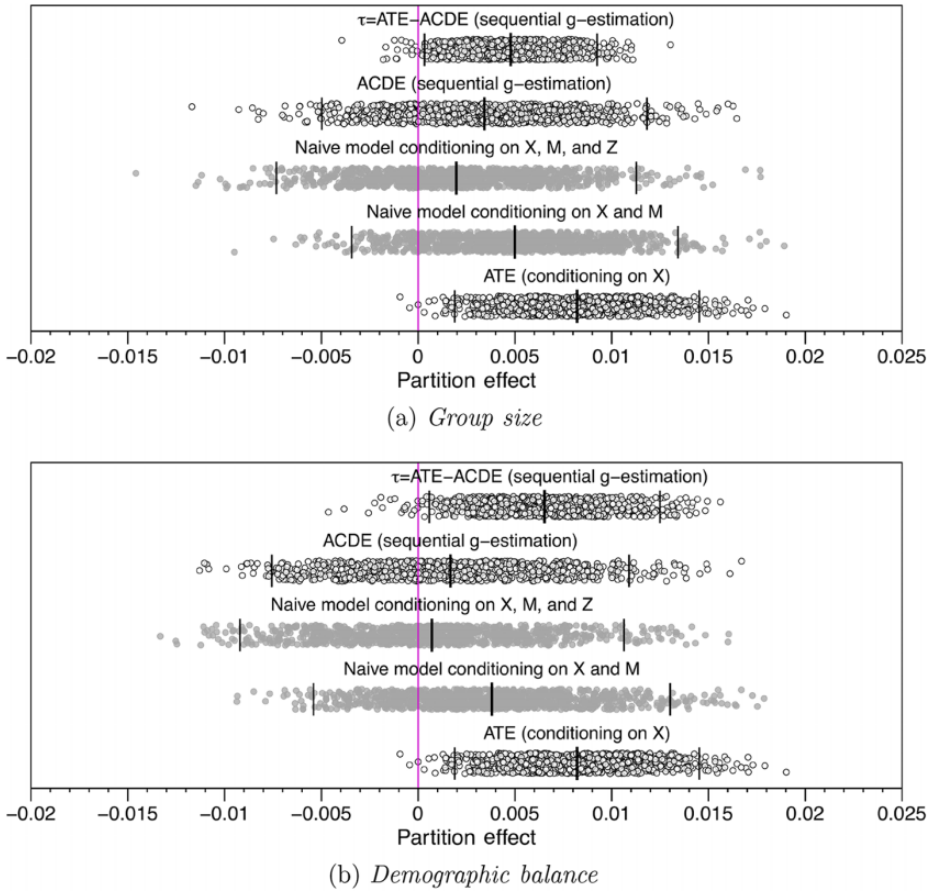

Figure 4. Effect of *Partition* on conflict onset with the mediator recentered to mean

Direct effect estimates with (a) *Group size* and (b) *Demographic balance* as the mediator variable, along with the ATE estimate. Each dot indicates a bootstrap estimate, whereas vertical stripes represent the point estimates (mean) and 95% confidence intervals obtained via 1,000 bootstrap repetitions.

g-estimation yielded a statistically insignificant and substantially small ACDE of 0.0034 (95% CI: $-0.0050, 0.0118$). Despite the strongly positive ATE, the direct effect becomes almost attenuated away when accounting for indirect effect and causal interaction with contemporary group size in the counterfactual where all ethnic groups had the average group size. Moreover, *Partition* turns out to be a key predictor of contemporary *Group size* with a strong negative impact, suggesting that split groups are on average smaller than non-split groups. Substantively, the coefficient on *Partition* in Model 4 can (roughly) be interpreted as semi-elasticity such that ethnic partition by modern political boundaries results in a $100 \times \beta = 34.8\%$ reduction in contemporary demographic size, lending further support for Hypothesis 3.

Figure 4(a) visualizes the estimates of Models 1 to 3 in Table II and the ATE model (Model 2 in Table I), with the corresponding $\Delta\tau = \text{ATE} - \text{ACDE}$. As in the top row of the figure $\Delta\tau \sim 0.0048$ (95% CI: $0.0003, 0.0092$) is statistically significant at the 5% level and substantially meaningful. Given the ATE of 0.0082, the estimate indicates that a substantial part of the treatment effect comes from the indirect and causal interaction effects induced by contemporary group size. The substantive interpretation follows that although split groups are more likely to engage in violent interactions with the incumbent, the conflict-escalating effect operates via the proposed group-size channel, lending support for Hypothesis 3. Figure 4(b) replicates the estimation with an alternative group size measure, *Demographic balance*, or logged fraction of group population relative to the host country's total population, which yields qualitatively similar results.

Causal interaction

The results so far suggest that the total effect of ethnic partition is strongly positive, but the conflict-escalating effect turns out to be less visible when controlling for

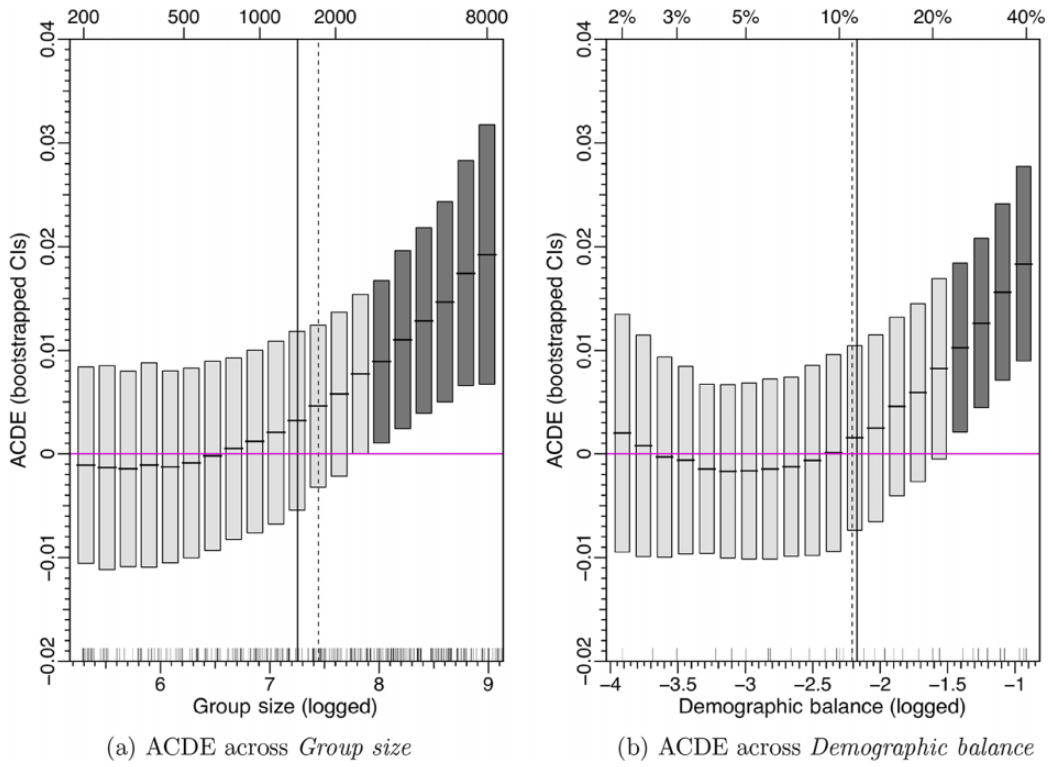

Figure 5. Controlled direct effect of *Partition* across different demographic sizes

ACDE across the 10th to 90th percentile values of (a) *Group size* (logged group population in 1,000) and (b) *Balance* (logged fraction of group population relative to host country's total population). Vertical rugs at the bottom indicate the distribution of mediator (randomly sampled 100 observations for visibility). Vertical segments spanning the panels represent mean (solid) and median (dashed) values. Horizontal stripes indicate the point estimates (mean), and rectangle edges represent the corresponding 95% bootstrap CIs obtained via 1,000 bootstrap repetitions. The estimates statistically significant at the 5% level are plotted in darker colors.

contemporary group size. Recall that $\Delta\tau$ measures the sum of the indirect effect through a mediator and causal interaction how the direct effect of the treatment depends on the mediator. Therefore, a conservative interpretation follows that there is either a negative indirect effect of *Partition* on conflict onset through *Group size* or the treatment–mediator causal interaction such that the conflict-escalating effect of *Partition* is stronger for larger groups but weaker for smaller groups. In either case, contemporary group size plays a critical role in producing the long-run treatment effect.

To further investigate the causal interaction between ethnic partition and group size, I replicate the sequential *g*-estimation by subsequently recentering *Group size* to different values. As the estimator generally identifies the ACDE with the mediator set at value m for all observations, this approach enables us to compute the counterfactual treatment (direct) effect conditional on distinct mediator values (group sizes). The two panels in Figure 5 present the results of the ACDE estimates with *Group size* and *Demographic balance* varied from the 10th to 90th percentile values of the sample distributions. The horizontal stripes and rectangle edges indicate the point estimates and corresponding 95% CIs obtained from bootstrap replications. The two panels yield qualitatively similar results, and the overall pattern remains robust regardless of the group size measures.

The overall pattern in Figure 5(a) provides further support for Hypothesis 3 by revealing a causal interaction such that the conflict-escalating effect of ethnic partition is stronger for large-sized groups. For example, in the counterfactual with *Group size* held at its 10th percentile value (5.317), the corresponding ACDE remains smaller in magnitude and statistically indistinguishable from zero (ACDE $= -0.0011$, 95% CI: -0.0106, 0.0084). Yet the ACDE becomes visible with *Group size* fixed at larger values. Indeed, with *Group size* at 8.0213 (roughly the 70th percentile value, or a group size of 3 million), the ACDE reaches 0.0089 (95% CI: 0.0011, 0.0167) with statistical significance at the 5% level.

One may wonder if the results simply reflect measurement error such that the coding of ethnic partitioning is

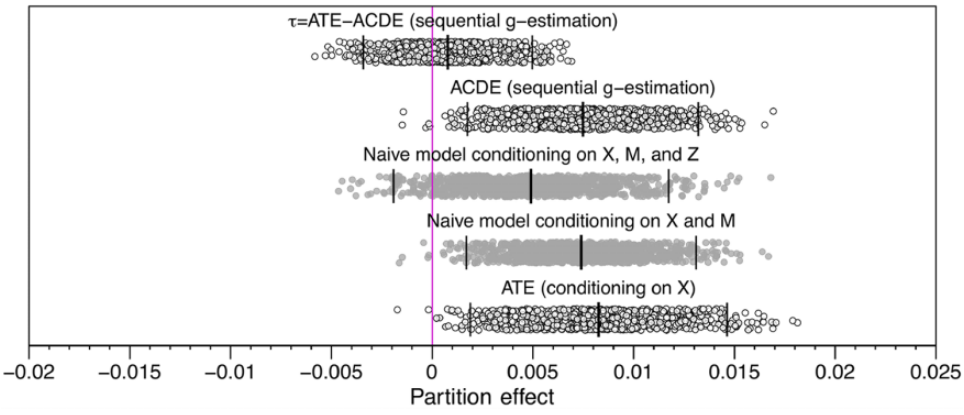

Figure 6. Effect of *Partition* on conflict onset net *Discrimination*
See notes in Figure 4.

'noisier' for geographically (and demographically) smaller groups. Online appendix D addresses this concern by replicating the analysis with subsamples dropping geographically small-sized groups, illuminating the robustness of the findings. Collectively, the empirical illustrations lend strong support for our theoretical expectation that contemporary group size plays a key role in conditioning the association between ethnic partition and risks of civil conflict.

Discrimination mechanism

Although the results above underscore the substantial role of contemporary group size, one may wonder how alternative mechanisms participate in the causal pathways. One of the alternative stories is political discrimination such that ethnic partition increases the probability of conflict by leading to discrimination by central government (Hypothesis 2). If this alternative explanation is consistent with empirical records, we would see a negligible ACDE and a non-zero $\Delta\tau$ once we demediate the indirect effect and causal interaction with political discrimination.

To evaluate this alternative causal channel, Figure 6 presents the ACDE estimates with *Discrimination* as the mediator. The corresponding ACDE measures the average direct effect of *Partition* with *Discrimination* set to 0 (without political discrimination). Figure 6 indicates somewhat contrasting results to the baseline estimates with group size as the mediator, which illuminates how posttreatment bias can plague our estimates. Interestingly, the coefficient on *Partition* turns out to be substantially and statistically insignificant when we simply condition on *Discrimination* and intermediate confounders with a naive LPM (middle row). The ACDE, or direct effect net *Discrimination*, however, remains strongly positive, and the corresponding $\Delta\tau$ is statistically indistinguishable from zero at the 5% level. In other words, we would observe a sizeable conflict-escalating effect of ethnic partition even in the counterfactual where all ethnic groups are not politically discriminated, lending little support for the discrimination mechanism. Online appendix E reports further regression estimates focusing on ethnic discrimination and broadly confirms that ethnic discrimination is an important determinant of civil conflict. The conflict-escalating effect of ethnic discrimination, however, remains weakly related to the effect of ethnic partition.

Conclusion

The key to unpacking the persistent impacts of historical events lies in the role of posttreatment factors. Theoretically, this article argued that ethnic partition increases the risks of civil conflicts while the effect is less visible for relatively small-sized groups, or when the corresponding ethnic cleavages do not serve as an effective basis of political mobilization. Large-sized, easily observable ethnic cleavages lead to inefficient fighting with central government by generating greater information and commitment problems. Empirically, the econometric analysis confirmed the theoretical expectation by revealing the conflict-escalating effect of ethnic partition and the substantial conditioning role of contemporary group size. Ethnic partition increases the likelihood of armed conflicts between the central government and politically excluded ethnic groups, but the major part of the treatment effect is attributable to indirect and causal interaction effects induced by demographic size of split groups.

The role of ethnic discrimination in generating the persistent treatment effect remains limited.

These findings have implications for broader literature. First, the findings underscore the persistent role of historical events in determining contemporary conflicts. Historical shocks such as ethnic partition constitute likely omitted variables because, as the analysis revealed, they affect both risks of civil conflicts *and* contemporary determinants of domestic conflicts such as group size. This insight calls for closer attention to historical factors even when our primary focus lies on the impacts of contemporary factors; researchers can suffer from omitted variable bias if they omit relevant historical contexts. Although historical factors are often fluid and change over time, the path-dependent nature of historical forces shapes contemporary politics.

Second, the empirical analysis underscores the role of contemporary, posttreatment factors in shaping the persistent effects of historical events. Contemporary armed conflicts indeed have deep historical roots, but contemporary factors, which are affected by historical events, also play a critical role in generating the long-run effects. A related, important implication for civil war literature is that cultural (ethnic partition) and opportunity factors (group size) jointly shape the risks of civil conflict. While not all civil conflicts are fought along cultural cleavages, specific cleavages matter for conflict when coupled with meaningful demographic forces by influencing rebel mobilization and underlying bargaining problems.

Finally, this article also carries implications for future research and the continued debate over partition as a solution to domestic conflicts (Kaufmann, 1998). This article found that ethnic cleavages with historical contexts can exacerbate civil conflicts, but the effect remains negligible if the corresponding demographic size is not large enough to facilitate political mobilization. The flip side of this insight is that the likely effects of border designs and demographic engineering on armed conflicts depend on the resultant cultural geography. A possible implication follows that we would see increased instances of armed uprising against the incumbent if demographic engineering along particular cleavages generates other, demographically significant cleavages while seeing fewer conflicts when engineering attempts do not create such cleavages. Yet the empirical examination of this pathway, as well as the possible effects through the suffocation channel rather than dismemberment, requires a focused analysis. The effects of ethnic partition, or historical and cultural factors more broadly, on other forms of violence such as communal violence (Wig & Kromrey, 2018) under different political and demographic contexts (Griffiths & Wasser, 2019) are also open to future research.

Replication data

The replication data and R scripts for the empirical analysis in this article, along with the Online appendix, can be found at http://www.prio.org/jpr/datasets.

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ORCID iD

Gaku Ito https://orcid.org/0000-0002-5362-6918

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GAKU ITO, b. 1988, PhD in Advanced Social and International Studies (University of Tokyo, 2018); Assistant Professor (2018–) and NERPS Research Fellow (2019–), Hiroshima University; research interests: armed conflict, long-run effects of historical events.