Understanding Ethno-Nationalist Conflict: From Factors to Actors*

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Version 3.1

May 16, 2008

Abstract

The quantitative literature on civil wars has debated whether ethnic diversity increases the likelihood of "ethnic conflict". In contrast, we analyze such conflict as the result of competing ethno-nationalist claims to state power. Rather than estimating the impact of aggregate factors such as ethnic diversity indices, we identify key actors representing different ethnic groups, and analyze how their motivations and mobilizational capacity influence the propensity for armed conflict. In order to support this disaggregated approach, the authors assembled a new dataset on Ethnic Power Relations (EPR) that covers all politically relevant ethnic groups and their access to power around the world from 1946 through 2005. Based on this data, we show that conflict with the government is more likely to erupt (1) the more excluded from state power representatives of an ethnic group are, (2) the higher their mobilizational capacity, and (3) the more they have experienced previous conflict.

Despite its fundamental role in legitimizing the modern state system, nationalism is rarely linked to the outbreak of civil wars in the quantitative literature. Instead, scholars have debated whether "ethnic conflict" is an outcome of high degrees of ethnic heterogeneity. Such a restricted perspective obscures the mechanisms that cause ethno-nationalist violence. In contrast, the sociological and historical literature on nationalism recognizes the central role played by nationalism in modern power politics, but usually remains vague as regards the specific causes of civil wars and other types of violent conflict.

Building on earlier work, we conceive of such conflict as the result of competing ethno-nationalist claims to state power. To better understand the conditions under which such competing claims escalate into armed conflict, we move away from the variable-centered approach that dominates quantitative civil-war research. Rather than merely estimating the impact of different aggregate factors, such as ethnic fractionalization indices, we identify *key political actors, as well as their motivations and mobilization capacity, in their proper political context*.

Such a disaggregated perspective calls for new data. The authors assembled a new dataset on Ethnic Power Relations (EPR) that covers all politically relevant ethnic groups and their access to power around the world from 1946 through 2005. This dataset improves significantly on existing datasets, such as the widely used Minorities at Risk dataset, which focuses on mobilized minorities and thus largely overlooks the ethnopolitical constellation of power at the center.

Improved theory and data allow us to show that, against the expectations of many scholars of civil wars, competing ethno-nationalist claims over the state is the driving force behind many of the civil conflicts in the post-World War II era. We show that conflict with the government is more likely to erupt (1) the more excluded from state power representatives of an ethnic group are, (2) the higher their mobilizational capacity, and (3) the more they have experienced previous conflict. In view of these findings, we

conclude that ethno-nationalist struggles over access to state power are an important part of the dynamics leading to the outbreak of civil wars.

The paper is organized as follows: We first review the literature on ethno-nationalist conflict and then develop three main hypotheses that lie at the core of our theory. The following section introduces the new dataset. The main results are presented in the next section, followed by a brief sensitivity analysis and a concluding section.

Theoretical background

We start by observing that in many civil wars, rebels claim that their own ethnic community has not received its due share of government positions and that it has been discriminated against by dominant, nepotistic rulers. Many schools of thought in the civil war literature dismiss such discourse as mere rhetorical nods to post-Cold-War ideas about identity politics (Laitin 2007), and emphasize other motivating factors, such as the weakness of governments (Fearon and Laitin 2003) or the desire to gain access to lootable resources (Collier and Hoeffler 2004) as the main driving force of rebel activity.

Our proposal is to take nationalism seriously rather than to dismiss its causal significance as a matter of principle. The classical sociological literature on nationalism offers a good starting point in understanding its logic: nationalism can be defined as a political principle that demands that the unit of governance and the nation should be congruent. It replaces the principle of dynastic sovereignty on which agrarian empires were based (Gellner 1983). Whereas the unit of governance is usually understood to be the sovereign state, the nation is imagined as a community of common origin and shared historical destiny (Anderson 1991). Many of these national communities are based, as Anthony Smith insists, on an ethnic core—a particular ethnic community that is re-imagined as a nation once the modern ideas of popular sovereignty and equality are introduced (Smith 1996). Wimmer (2002) focuses on states that lack such a

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state-owning ethnic core to form the basis of a nationalist movement and future statehood. He outlines the conditions under which various ethnic communities will start competing for political control over the state and the prestige of being declared its constituent nation and how such competing demands may spiral up into ethno-nationalist conflict.

Thus, historical sociology has shown how introducing the principles of nationalism—that ethnic likes should rule over ethnic likes—leads to waves of political mobilizations and a subsequent transformation of the state system (Kedourie 1960; Breuilly 1994; Brubaker 1996). They have also shown the variety of forms that these ethno-nationalist struggles can take: conflicts over access to state power for a particular ethnic community, secessions from existing states in order to establish a new state ruled in the name of a particular ethnic community or to join another state controlled by ethnic kin, and competition between new states over mixed territories inhabited by members of both ethnic core groups.

While this literature helps to explain the broad historical and political circumstances under which we expect ethno-nationalist mobilization and conflict, it is much less concerned with identifying the precise mechanisms by which ethno-nationalist mobilization turns violent. Typically, the literature on nationalism has been preoccupied with sweeping debates relating to the historical and social roots of nationalism in the modern world, rather than with processes that produce specific political outcomes. Because of its structural and macro-historical focus, Beissinger's critique of Gellner applies to much of this macro-historical approach: "Like Marxism without a theory of revolution, Gellner's theory provides no coherent vision of how nationalism works its way into the realm of substantive human action" (Beissinger 2002, p. 170). More recently, a number of scholars have studied nationalist mobilization and conflict in greater detail (e.g. Hechter and Levi 1979; Beissinger 2002; Olzak 2006). We draw on this scholarship in order to develop our own model of ethno-nationalist conflict.

In the early post-Cold War period, a new political science scholarship on ethnicity and nationalism emerged that is also highly relevant for this project. In response to the conflicts in the former Yugoslavia and Rwanda, this research transplanted ideas that had been developed to study interstate relations during the Cold War. For example, Posen (1993) analyzed ethnic conflict as a struggle between ethnic groups in the wake of state collapse. Drawing explicitly on neorealist theory, Posen's account stresses fear of exploitation in security dilemmas as a major motivation triggering preemptive violence. These ideas were further developed with the help of rational-choice theory in a series of studies (e.g. Hardin 1995; De Figueiredo and Weingast 1999). As a consequence of the assumption of state breakdown, however, this research tends to overlook the important role played by state actors in generating these conflicts in the first place, as the wars in Yugoslavia, Rwanda, and many other places clearly demonstrate. Moreover, the explanations proposed by this literature are rarely tested empirically beyond the recounting of illustrative case studies.

More recently, statistical methods have been applied to the study of civil wars. Within this literature, an increasing number of "revisionist" scholars have questioned the focus on ethnically defined actors and grievances that had characterized the immediate post-Cold-War scholarship. In their view, ethnic grievances are too widespread to explain the rare onset of conflict. Rather, civil wars break out in states that are too weak to suppress rebellions or where natural resources invite warlords to loot and enrich themselves (Collier and Hoeffler 2004; Fearon and Laitin 2003; for a review, see Kalyvas 2007).

There can be no doubt that this recent literature has greatly improved the analytical precision and systematized the empirical treatment of civil wars. Often inspired by microeconomic models, it has clarified the general conditions that are conducive to civil wars and insurgencies. Yet, for all its advantages, this research tradition has actually made it harder to appreciate crucial aspects of ethno-nationalist conflict. We argue that these difficulties flow directly from the theoretical and methodological

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assumptions commonly made in that literature. The three main weaknesses derive from not correctly

specifying (1) the relevant actor constellations, (2) the actors' motivations, and (3) the processual, path-

dependent nature of conflict.

Factors rather than actors[1]

For reasons of tractability and data availability, the bulk of the quantitative civil war literature still relies

on "country years" as the main unit of analysis together with highly aggregated country-level indicators.

Analysts typically postulate rationalist micro-level mechanisms that allegedly generate observed macro-

level patterns. However, relatively little empirical analysis has so far been conducted to assure that the

country-level factors are consistently related to the postulated micro mechanisms (though see Collier and

Sambanis 2005).

The way that ethnicity is dealt with in the quantitative literature – to the extent it is treated at all – is no

exception from this problem. Instead of measuring ethno-nationalist mobilization by empirical actors,

existing quantitative studies rely on proxies that are not clearly related to any specific mechanism leading

to ethnic conflict. For example, Fearon and Laitin (2003) argue that the lack of a statistical association

between "ethnic diversity" and civil war allow them to reject major theories of ethnic conflict, including

those derived from Gellner's work.[2] Some scholars propose measures of ethnic polarization that are

loosely related to Horowitz' (1985) theory of ethnic conflict (e.g. Montalvo and Reynal-Querol 2005).

Others seek to operationalize the concept of ethnic domination but end up using a purely demographic

proxy (e.g. Collier and Hoeffler 2004).

We believe that these efforts to grasp the logic of ethnic politics through macro-level indices are

problematic. First, they implicitly assume that those ethnic groups that are listed in the work of

anthropologists and linguists are politically relevant—thus often misrepresenting the actor constellations

in the political arena (Posner 2004). Second, the macro-level indices refer to demographic statistics rather than constellations of power (Cederman and Girardin 2007). Clearly, in ethno-nationalist wars, the state does not constitute a neutral arena but rather an active participant (Brass 1991; Breuilly 1994). In ignoring the state as a crucial collective actor, the recent quantitative attempts to evaluate the role of ethnicity in conflict share a fundamental weakness with the aforementioned wave of studies of ethnic conflict in the early post-Cold War era. Because of these conceptual difficulties, it is unsurprising that the quantitative literature has failed to reach agreement on whether or not ethno-demographic diversity causes internal conflict.

Reacting to these limitations of country-level studies of civil wars, a number of scholars have called for a more disaggregated approach that focuses on concrete actors involved in a conflict, on their recruitment patterns and violence strategies, and so forth (e.g. Kalyvas 2006; Collier and Sambanis 2005; Humphreys and Weinstein 2006; for a review, see Tarrow 2007). This research is based on innovative methodologies including surveys, interviews, field experiments, theoretically guided case studies and other in-depth methods. Our own approach to disaggregation complements these studies at a level of analysis that is less detailed, yet provides global coverage, encompassing many more cases than is possible with small-n studies based on surveys, fieldwork, or archival data.

If factors constitute insufficient explanations of civil war, where should we turn for actor-based accounts? There is a substantial empirical literature that analyzes minority mobilization. Among the quantitative datasets with global coverage, the Minorities At Risk (MAR) project remains the prime data source to evaluate theories of ethnicity and violence quantitatively (Gurr 1993; 2002). Indeed, it has been widely used to test theories of ethno-nationalist conflict (e.g. Saideman and Ayres 2000; Toft 2003; Olzak 2006; Elkins and Sides 2007). The MAR dataset allows for empirical tests of mechanisms linking ethnicity to conflictual politics, a crucial advantage over the macro-level indicators reviewed above. But it also has its

limitations. First, MAR's stress on persecuted and powerless minorities is likely to cause selection bias that undermines inference about conflict onset (Fearon 2003). Second, the MAR dataset focuses exclusively on minorities and thus largely overlooks the dynamics of ethno-political power at the center of state power.

Materialist rationalism rather than nationalist motivations

Theories that fail to get the main actor constellations right are also likely to misrepresent their motivations. Inspired by economic reasoning, the recent literature on civil war tends to downplay the influence of questions of political legitimacy, including those raised by over- and under-representation in national governments. In their much cited model, Collier and Hoeffler (2004) stipulate that "greed" rather than "grievances" determine whether civil wars will break out. Most of the political economy literature agrees with this materialist orientation. Genuinely political motives and perceptions of justice and legality are dismissed as rationalizations covering up the economic motivations of entrepreneurs of violence. Likewise, rebel recruitment is mainly seen as a matter of labor market conditions rewarding potential fighters with lucrative side payments.

While rational choice theory can help articulate causal mechanisms driven by instrumental and strategic behavior based on careful calculation, it ignores, and often explicitly rejects, mechanisms that involve emotional motivations (Petersen 2002) or the quest for political power (rather than economic resources). Indeed, a narrowly rationalist and materialist interpretation of the rational choice paradigm stacks the explanatory cards against ethno-nationalism, which draws its power *both* from the motivation to gain political power *and* from emotionally loaded symbolism. Successful nationalist collective action depends on political self-interest of potential participants, the organizational capacity to co-ordinate their activities and overcome collective action problems, and the emotional/ideological appeal of a discourse of injustice that justifies such action.

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These theoretical biases are further compounded by operational difficulties associated with empirical measurement of ethno-nationalist "grievances." It is easier to measure objective variables such as GDP per capita and natural resource stocks than developing observable indicators of the degree of legitimacy of a political regime. Below, however, we attempt to show that such difficulties should, and can at least partly, be overcome.

Static analysis rather than path dependence

Ethno-nationalist mobilization and contestation are macro-historical processes that operate over both short (Beissinger 2002) and long time spans (Wimmer and Min 2006). Frustrations with perceived humiliation and unfair status hierarchies sometimes take decades to build up before they result in violent collective action. Once violence enters the picture, however, mobilization processes have a tendency to become self-reinforcing. Violence often takes on a logic of its own, making it difficult to achieve political ends by non-violent means (Waldman 2004; Kalyvas 2007).

In contrast, the contemporary literature on internal conflict makes strong assumptions about the relative irrelevance of such historical effects (Sambanis 2004a). In this sense, they fit into the usual pattern of theorizing in mainstream political science that assumes history to be "efficient" (March and Olsen 1998). For example, instrumentalist approaches to ethnic identities that view them as flexible aggregations of individual choices (e.g. Kuran 1998) downplay longer-term historical processes that may lead to the institutionalization of ethnic dividing lines. Relatively unburdened by historical legacies, rational political actors are assumed to respond quickly to institutional and material incentives. Alternative explanations that posit more stable identification patterns are usually lumped together with primordialist theories and summarily dismissed as theoretically crude and empirically flawed. Yet, there are reasons to suspect that this critique targets a straw man (Horowitz 2004; Petersen 2002, pp. 62-63).

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Again, methodological choices stand in the way of a full understanding of the long term nature of ethno-nationalist conflict. Most quantitative studies of civil war expect causal effects to operate within a year or two. Despite efforts to capture temporal dependence, statistical panel studies based on country-years typically resort to methods that are designed to "avoid endogeneity" (e.g. Beck, Katz and Tucker 1998) rather than making the effort to understand historical sequences and dependencies (Pierson 2004; Abbott 2001). Although important exceptions exist in the quantitative literature, including studies of escalation patterns (Sambanis and Zinn 2005), the influence of past conflicts (Walter 2004), and applications of the IR concept of enduring rivalries to internal ethnic conflicts (DeRouen and Bercovic 2008), there are very few systematic attempts to link such effects to a coherent theory of ethno-nationalist violence.

In sum, it can be noted that the qualitative literature on nationalism contains powerful ideas about who the key actors are and what their motivations are, but these insights have rarely given rise to precise and empirically testable hypotheses about mechanisms connecting ethno-nationalist politics with political violence. The following section addresses this task.

Theorizing ethno-nationalist conflict: Actor constellation and collective action

The starting point is Tilly's (1978) polity model, which conceives of a political system comprised of a government and a number of contenders that seek to maximize their access to executive power. Whereas members of the polity enjoy a privileged position, challengers attempt to improve their power status (see Figure 1). Adapting Tilly's model, we assume that the members of the polity represent included groups (IGs) and the challengers represent excluded groups (EGs). Given the principle of ethno-national representativity embodied by the modern nation-state, we postulate that all challengers seek to avoid the rule of ethnic others by gaining access to the polity or leaving it in favor of a new polity or an already existing kin state. In contrast, representatives of ethnic groups in power primarily seek to retain power.

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Power holders want to maximize their control of the state and are unwilling to share it with others—thus opening the possibility of infighting among power sharing partners.

It is important to note that our model deviates from Tilly's group-based scenario in that it features conflict between organizations and movements competing along ethnic lines, rather than directly between social groups. In order to avoid reifying ethnic groups (cf. the critique of Brubaker 2004), conflict processes in our theoretical rendering feature political organizations that draw on support from one or several ethnic groups.

[Figure 1 about here]

Civil wars confront incumbent governments with political and military organizations that challenge the governments' claim to sovereign rule. This corresponds to standard definitions of civil war (Sambanis 2004b; Kalyvas 2007), but excludes communal conflicts or pogroms in which the state plays no active role (cf. Horowitz 2002). In conflicts that are fought in the name of excluded groups, rebel movements are composed of mobilized and militarized organizations that challenge the government. In the case of challenges launched in the name of groups that are already represented within government, other actors such as a faction within the army, or newly created political organizations and militias instigate a violent confrontation.

Having identified the main actors and the types of conflicts they may instigate, we now consider under which conditions violence is most likely to erupt. We first study which groups will be more motivated to support a rebellion against ethnic domination by others. Indeed, many aggrieved groups have not produced militant rebel organizations, as Fearon and Laitin (2003) correctly note. We therefore have to identify those groups that perceive the government as particularly illegitimate and are therefore more inclined to support rebellions. In a second step, we consider the organizational capacity to challenge incumbent state power because high levels of motivation alone will not suffice to produce an armed

organization willing to take on the government army. Finally, rather than being historical singularities, political violence leaves long traces that put nationalist politics on a contentious track and we therefore need to specify this path-dependency. In the following, we consider the causal mechanisms associated with each of these main dimensions of nationalism in turn.

Most motivated groups: Excluded, downgraded, and underrepresented
To specify motives and identify the most motivated actors, we rely on an institutionalist approach to nationalism and ethnic politics. It maintains that once nationalism becomes the dominant principle of political legitimacy, rulers are expected to care for "their own" people. Political office holders thus have incentives to gain legitimacy by favoring co-ethnics over others when it comes to the distribution of public goods and government jobs. The expectation of ethnic preference works the other way too: Voters prefer parties led by co-ethnics, delinquents hope for co-ethnic judges, and citizens prefer to be policed by co-ethnics.

Under such conditions of pervasive ethnic favoritism, political leaders and followers are driven by the strategic motive to avoid, or to overturn, dominance by ethnic "others." This motive is at the same time material, political, and symbolic: "adequate" or "just" representation in a central government offers material advantages such as access to government jobs and services, legal advantages such as a the benefits of full citizenship rights, a fair trial and protection from arbitrary violence, and symbolic advantages such as the prestige of belonging to a "state-owning" ethnic group. In brief, this approach conceives of ethnic politics as the struggle over control of the state between various ethnically defined organizations and their constituencies (e.g. Brass 1991).

It follows that groups that lose out in this struggle for state control are much better breeding grounds for organizations that challenge the government. We postulate a direct relationship between the degree of

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state control and the likelihood that an armed rebellion will be instigated in the name of that particular

group. The most excluded groups will thus be most likely to support armed organizations that challenge

the government (ibid.). Given nationalist principles of political legitimacy, feelings of resentment will be

widespread (cf. Petersen 2002) and can be channeled into successful collective action with the aim of

rectifying the situation. We summarize this reasoning in a first hypothesis:

> *H1a. The probability of ethno-nationalist conflict increases the more representatives of an ethnic*
>
> *group are excluded from central executive power.*

The exclusion mechanism also alerts us to the consequences of changes in power hierarchies.

Sociological theories of emotions suggest that arousal of negative emotions is especially likely following

loss of power and prestige (Kemper 1978; see also Turner and Stets 2005). Wherever the subject blames

others for their downgrading, anger and resentment increase the readiness to fight in order to change the

situation (Turner and Stets 2005, p. 217). We postulate a similar mechanism at the level of collective

organizations: Leaders of ethno-nationalist organizations will be most likely to resort to violence if they

have recently experienced a loss of relative power. They can channel the resentment of their

constituencies and mobilize to "reverse a reversal" (Petersen 2002, p 173). This reasoning can be stated

more concisely with another hypothesis:

> *H1b. The probability of ethno-nationalist conflict increases following a fall in power status that*
>
> *decreases access to central executive power by representatives of an ethnic group.*

Finally, the principle of legitimacy of modern nation-states is also violated if groups in power are "under-

represented" compared to other power sharing partners. When smaller groups wield more power than

larger groups, representatives of the larger groups can portray the situation as "unfair" and stir up fear of

ethnic domination among their constituents. Thus,

H1c. The probability of ethno-nationalist conflict increases if the ethnic group represented by a power-sharing partner is larger compared to that of more powerful coalition partners

Mobilizational capacity: group size and organization

Collective action theory tells us that group motivations are insufficient to produce political mobilization and violent contestation (McCarthy and Zald 1977; Tilly 1978; Tarrow and Tilly 2006). The motivational forces described above are thus a necessary but not a sufficient cause for ethnic conflict. Successful mobilization requires *both motivations and organizational capacity*. Larger excluded groups will be more likely to challenge a government because superior numbers increase the possibility to recruit fighters, mobilize economic resources to support political mobilization and armed rebellion, and provide an organizational infrastructure to sustain it (DeNardo 1985). In the case of excluded groups, the organizational advantage of large numbers are combined with increased legitimacy of their political claims. Given the principles of legitimacy of nation-states, the exclusion of large sections of the population from power is much more scandalous compared to the exclusion of smaller groups, and minority ruled states (ethnocracies) are among the least legitimate political regimes in the modern world (Petersen 2002, p. 51).[3] Based on this argument, we postulate that:

H2. The probability of ethno-nationalist conflict increases with the ethnic group's relative demographic size.

History matters: the influence of past conflict

We have argued above that rather than being an instant and ahistorical phenomenon, nationalist mobilization takes place in a context that might be characterized by the effects of previous occurrences of ethno-nationalist violence. In extreme cases of path-dependency, actors are locked into self-sustaining cycles of violence. We postulate that past conflicts influence the likelihood of present conflict through three mechanisms.

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First, ethno-nationalist activists attempt to glorify their groups' history through one-sided narratives that stress their own victories and attribute blame for military losses to appropriate out-groups. This implies that leaders might not update their risk assessments and take up arms again even when the chances of winning have not improved significantly (for similar cognitive mechanisms, see Rydgren 2007). Second, past experiences of traumatic violence may live on as a part of oral traditions, or sometimes be perpetuated in official history textbooks and public rituals, nourishing calls for revenge (Kalyvas 2007). To be sure, this type of path-dependence effect by no means implies that conflict "hardwires" mutually hostile identity configurations to the extent that reconciliation becomes impossible (as assumed by Kaufman 1996). After all, memories are selective and history contains enough episodes to allow for re-interpretations. This second mechanism is thus of a highly probabilistic nature. Third, prior exposure to combat means that violence is no longer unthinkable but constitutes part of the accepted repertoire of action and may help create organizational structures and identities that can be reactivated at later points in history (Waldman 2004; Zürcher 2007, pp. 55, 219).[4] We express these three path-dependency mechanisms in our third main hypothesis:

> H3. *The probability of ethno-nationalist conflict increases with the number of prior conflicts fought in the name of the same ethnic group.*

Before operationalizing and rendering more precise our main hypotheses, we turn to the dataset that underlies this study. Evaluating the theory calls for data that codes access to executive power for representatives of different ethnic groups as well as the conflicts in which they have been involved.

Measuring ethnic power relations

The Ethnic Power Relations (EPR) dataset identifies all politically relevant ethnic groups around the world and measures in how far access to state power differs among them in all years from 1946 to 2005.[5]

In collecting the data, we relied on the expert input of nearly one hundred students of ethnic politics to assess formal and informal degrees of political participation and exclusion along ethnic lines.[6] This dataset improves significantly on previous efforts to code ethnic groups' access to power, such as Cederman and Girardin (2007), who rely on preliminary, static measures of inclusion and limit their sample to Eurasia and North Africa.

Thus, the dataset takes ethnic groups as units of observation, rather than political organizations that claim to speak in their name. The main reason for this methodological decision is that organizations are much more prone to change (such as through processes of fusion, fission, exit and entry) than politically relevant ethnic groups and thus would make a analysis of conflict-prone configurations immensely more complex.

Politically relevant ethnic groups

Following the Weberian tradition of ethnicity studies, we define ethnicity as any subjectively experienced sense of commonality based on the belief in common ancestry and shared culture (Weber 1978: 385-398). Different markers may be used to indicate such shared ancestry and culture: common language, similar phenotypical features, adherence to the same faith, and so on. We thus include ethnolinguistic, ethnosomatic (or "racial") and ethnoreligious groups in our definition of ethnicity, but exclude tribes and clans which define community in genealogical terms, as well as regions which do not define commonality on the basis of shared ancestry. Ethnic groups might be hierarchically nested, i.e. comprise several levels of differentiation, not all of which might be politically relevant.

An ethnic category is politically relevant if at least one significant political actor claims to represent the interest of an ethnic group in the national political arena, or if members of an ethnic category are systematically discriminated against in the domain of public politics. By "significant" political actor we

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mean a political organization (not necessarily a party) that is active in the national political arena.[7]

Discrimination is defined as political exclusion directly targeted at an ethnic community—thus

disregarding indirect discrimination based for example on educational disadvantage or discrimination in

the labour or credit markets. The coding rules allow for the identification of countries or specific periods

in which political objectives, alliances, or disputes were never framed in ethnic terms.[8]

Because the politically relevant groups and access to political power may change over time, coders were

asked to divide the 1946 to 2005 period and provide separate codings for each sub-period. This was also

necessary if the list of politically relevant categories changed from one year to the next. The next

important step was to code the degree of access to power that the political leaders of these various groups

enjoyed.

Coding access to power

We focus on executive power only, i.e. representation in the presidency, the cabinet and senior posts in

the administration, including the army. The weight given to these different institutions depended on the de

facto power constellations of the country in question. Experts were encouraged to capture the most

relevant dimension (e.g. in a military dictatorship, power over the army, and in presidential systems, the

presidency etc.). We were primarily interested in major power shifts rather than day-to-day

reorganizations of cabinets or the promotion of officers in the army. In all cases, coders were to

investigate absolute access to power irrespective of the question of under- or over-representation relative

to the demographic size of an ethnic category.

All politically relevant ethnic groups were categorized according to (1) whether those who claimed to

represent its interest held full control of the executive branch with no meaningful participation by

members of any other group, (2) whether they divided power with members of other groups in a power-

sharing regime, or (3) whether they were excluded altogether from decision-making authority within the

halls of state power. Within each of these categories, coders were asked to differentiate between further

subtypes:

(1) Absolute power: In this case, the political elites that claim to represent an ethnic group do not

significantly share power with other political leaders. There are two possibilities:

- *Monopoly*: Elite members hold monopoly power in the executive at the exclusion of members

  other ethnic groups.

- *Dominance*: Elite members of the group hold dominant power in the executive but there is some

  limited inclusion of "token" members of other groups.

(2) Power sharing regimes: By power sharing, we mean any formal or informal arrangement that divides

executive power among leaders who claim to represent particular ethnic groups. Depending on the

relative importance of the positions controlled by the group members, there are two possibilities:[9]

- *Senior Partner:* Representatives participate as senior in a formal or informal power-sharing

  arrangement.

- *Junior Partner:* Representatives participate as junior partners in government.

(3) Exclusion from central power: Finally, if political leaders who claim to represent a particular ethnic

category are excluded from participation in central government, we distinguish between three

possibilities:

- *Regional Autonomy:* Elite members of the group have no central power but some influence at the

  sub-state level, i.e. one level below the central government. This may be the sub-state, the

  provincial or the district (though not local) level, depending on the vertical organization of the

  state.

- *Separatist Autonomy:* A related case is when local governments controlled by representatives of

  an ethnic category have declared their territory to be independent from central government. This

  category differs fundamentally from "regional autonomy" in that the group excludes itself.

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- *Powerlessness*: Elite representatives hold no political power either at the national or the regional level without being explicitly discriminated against.

- *Discrimination*: Group members are subjected to active, intentional and targeted discrimination, with the intent of excluding them from both regional and national power. Such active discrimination can be either formal or informal. [10]

Conflict coding

Our coding of conflict is based upon the UCDP/PRIO Armed Conflicts Dataset (Gleditsch et al. 2002). Armed conflict is defined as any armed and organized confrontation between government troops and rebel organizations or between army factions that reaches an annual battle death threshold of 25. Massacres and genocides are not counted as wars because the victims are not organized and armed, and communal riots and pogroms are excluded because the government is not directly involved. [11]

For each conflict, we coded whether actors pursued ethno-nationalist aims as well as if they pursued secessionist objectives: *Ethnic/non-ethnic conflicts* were distinguished by the aims of the armed organization on the one hand and their recruitment and alliance structures on the other hand. Conflicts over ethno-national self-determination, over the ethnic balance of power in government, over ethno-regional autonomy, against (alleged or real) ethnic/racial etc. discrimination, for language and other cultural rights are typical for ethnic wars. All other war aims were defined as non-ethnic. Regarding recruitment and alliance structures, we defined ethnic wars as fought by armed organizations who predominantly recruit fighters among their own ethnic group and who forge alliances on the basis of ethnic affiliation. For a conflict to be classified as ethnic, armed organizations have to both explicitly pursue ethno-nationalist aims *and* recruit fighters and forge alliances on the basis of ethnic affiliations.

All ethnic conflicts were then linked to the politically relevant ethnic category in the name of which an armed organization instigated the conflict. We looked at the aims and recruitment patterns of each armed organization separately. In some complex cases (as in Afghanistan, Burma, Chad, Uganda, Angola, and Zaire), we disaggregated a conflict into several war fronts when different ethnic claims were made on the non-governmental side. This was also necessary when the constellation of rebel organizations and the ethnic communities they represented changed dramatically over time.

Our dataset includes armed conflicts fought between 1946-2005, 110 of which were coded as ethnic conflicts and thus directly relevant to this study. Given that some of these ethnic conflicts were fought by more than one group, we identify 147 group-level instances of ethnic conflict onset. Among these 147 onsets, 27 were fought by groups in power, 88 by powerless or discriminated groups, and 32 by autonomous groups. One-half of these conflicts reached the standard threshold of civil war (i.e. more than one thousand battle-deaths in a year). In addition, the dataset includes a distinction between secessionist and non-secessionist conflict, to which we will return in our analysis of elite infighting.

Variable definitions and data sources

Drawing on the EPR dataset, we introduce new ethnic politics variables testing all three hypotheses by measuring power access (H1a,b,c), mobilizational capacity (H2) and prior conflict (H3) for each politically relevant ethnic group in each year from 1946-2005.

The *Excluded* variable is a dichotomous indicator that indicates whether representatives of an ethnic category have regional or separatist autonomy, are powerless or discriminated (H1a). To present a more nuanced picture, we will also use dummy variables referring to specific EPR categories, i.e. *Junior partner*, *Autonomy* for regional autonomy, *Separatist* for separatist autonomy, *Powerless*, and *Discriminated.* A *Downgraded* dummy variable identifies whether representatives of a group have

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experienced a decrease in power status during the previous two years (H1b), based on the EPR power status categories. In our analysis of elite infighting, we complement this measure with the *Under-represented* variable that indicates whether a junior coalition partner represents a larger ethnic group than a senior partner (H1c).

Furthermore, we evaluate group size with the logged variable *Group size*, which uses the EPR estimate of demographic group size as a proportion of the countries total population (H2). In order to test the influence of prior conflict, we use a variable *Past conflict* that counts the number of conflicts that have already been fought in the name of the same ethnic group since the beginning of the sample period (H3). Finally, *Peace years* counts the number of peaceful years since the last conflict or the beginning of the sample period. As recommended by Beck, Katz, and Tucker (1998) this indicator is tested together with three cubic splines.

In order to control for country-level characteristics, we use three control variables throughout the paper. Based on extensive robustness analysis, Hegre and Sambanis (2006) conclude that variables measuring wealth and population size have a powerful effect on civil-war onset. Therefore we include measures of logged *GDP per capita* and logged country *Population* size, drawn from the Penn World Tables and World Bank sources. In addition, we control for regime type by including a lagged indicator for *Anocracy*, i.e. countries that are neither fully democratic nor autocratic. Our regime variable is based upon Polity IV data, using their recommended cutoffs of +6 and -6.

While several interpretations have been offered of the effects of income, our theory of ethnic politics expects that richer countries should have less conflict because they can afford to solve political conflicts through redistribution (cf. Sambanis 2004b). Although it usually has robust and strong effects in country-level studies of civil war onset, it is less clear what to expect regarding the population indicator,

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especially because this may be an aggregation effect reflecting the fact that, on average, more conflicts tend to occur in larger countries. Finally, anocratic states are associated with more ethno-nationalist conflict in keeping with theoretical arguments in the "domestic democratic peace" literature (Hegre et al. 2001) according to which democratic systems are generally better at handling grievances peacefully and autocratic governments are able to suppress violence effectively at the expense of political participation.

Data analysis

We are now ready for a first evaluation of our three hypotheses relating to power access, mobilization capacity and past conflict. The current section introduces statistical models that concern groups in all power status categories, as well as more specific analysis of insurgencies in the name of excluded groups and infighting among power sharing partners. There are good reasons to believe that the behavioral mechanisms depend on the actor configurations within which they are embedded.

Descriptive analysis

Our dataset is structured in group-country-year format, with 32,149 unique observations in all years in which an ethnic group is politically relevant. Table 1 offers a first empirical assessment of the power-access hypothesis H1. As expected, the frequency of conflict increases roughly with the degree of exclusion. Excluded groups are much more likely to experience a rebellion in their name (0.59%) compared to groups in power (0.23%). A χ^2-test confirms that this relationship is significant at p = 0.0001. With the exception of the category Separatist Autonomy which exhibits relatively less conflict than the power-sharing categories, the table also reveals that the conflict frequency falls steadily with increasing access from executive power. Because Separatist Autonomy differs from the other excluded categories in that the group has chosen to "exclude itself" from central state power we list these at the end of the exclusion category.[12]

[Table 1 about here]

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Going beyond descriptive statistics, we proceed with regression analysis to test the main hypotheses. Our standard framework consists of logit models with ethnic conflict onset as a dichotomous dependent variable. Because the causal logic during wars can be expected to be different from peacetime, we drop observations associated with ongoing wars after each onset. To account for the non-independence of observations within countries, we present robust standard errors, clustered on the country unit. Using group-years as the unit of analysis, the sample includes all politically relevant groups, except those that enjoyed Monopoly or Dominance because these categories could not be associated with conflict given the coding of the dependent variable.

Analysis of all groups

Our main results are presented in Table 2. The first two columns present the findings of models that include groups in all power status categories. In order to operationalize the basic power access hypothesis H1a, Model 1 uses the *Excluded* dummy variable to identify those groups that have no access to central power. Based on this dichotomization it is clear that rebellions in the name of excluded groups are much more likely than conflict in the name of included groups, thus vindicating H1a. The *Downgraded* measure, which tests hypothesis H1b, is also strongly confirmed by this analysis. In this case, the corresponding binary variable has an even stronger effect than the static exclusion measure. Turning to the test of mobilization capacity, H2, we also find solid evidence that, *ceteris paribus*, larger groups are more likely experience a rebellion fought in their name. The analysis also powerfully confirms our path-dependency hypothesis H3. As expected, the impact of past conflict, as measured by *Past conflict*, is both large and significant. We also discover that the hazard rate declines during peaceful periods.

The control variables behave as anticipated. Whereas the GDP variable has a strong, negative influence on conflict onset, the result pertaining to the *Anocracy* variable indicates that groups in anocratic states are indeed more prone to experience conflict, although the effect is somewhat weaker than those of our

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ethnopolitical variables.[13] However, the population variable, which usually performs well in country-level analyses, fails to exhibit any significant or substantive effect, which is likely to be a function of the aggregation problem alluded to above.

As a way to compare the marginal effect of our key variables, we plot the difference that our main variables have on predicted conflict probabilities, holding other variables at their mean or modal values. Using the included groups as the base category, the graph in Figure 2 displays the conflict propensities associated with groups that experienced exclusion, downgraded power status and past conflict as a function of GDP per capita. For all group categories we detect an effect of GDP per capita, but this influence becomes especially strong as we consider excluded and downgraded groups and those with past conflict histories, in accordance with hypotheses H1a,b and H3. For groups in poor countries, the effect of being excluded or having experienced past conflict more than doubles the probability of conflict. Downgraded power status has an even stronger effect, leading to yet another doubling of the conflict probability.

[Figure 2 about here]

In a further effort to investigate H1a, Model 2 introduces the more fine-grained power-access categories. It shows that the conflict-inducing impact of power status represents more of a continuum than a dichotomous distinction between inclusion and exclusion. According to our theory, groups' conflict proneness should decrease gradually as a reflection of their power status. Indeed, this is what we find. The coefficients range from a low and insignificant 0.47 for junior partners up to a highly significant 1.79 for discriminated groups and 3.48 for separatists. The latter category is especially conflict-prone, not merely because of high motivation, but also because separatism challenges the state to the highest possible degree, given that modern states are built on the principle of territorial sovereignty. Again, hypotheses H1b, H2 and H3 are powerfully confirmed and the effect of the control variables remains virtually unchanged.

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[Table 2 about here]

So far, we have considered all groups' relationship with the government, but there are reasons to believe that group behavior differs depending on whether or not they are already represented in central government.. Therefore, we now consider excluded and included groups separately to identify the relation-specific effects that are partially concealed in the general analysis.

Analysis of insurgencies

Once we restrict our analysis to rebellions in the name of excluded groups, we are able to specify a clear-cut actor constellation based on a center-periphery logic. Model 3 in Table 2 presents the empirical results of this analysis. Given the focus on excluded groups, we find smaller differences among the power-status categories. However, both *Discrimination* and *Separatist Autonomy* can be statistically distinguished from the baseline category *Regional Autonomy*, thus supporting hypothesis H1a. A recent *Downgraded* power status among these excluded groups increases conflict propensity significantly, in line with H1b. Compared to the general analysis, we find even more unambiguous evidence that large groups are more likely to experience conflict (H2). The influence of prior conflict remains significant (H3). Finally, the control variables behave similarly to the results in Models 1 and 2.

Figure 3 offers a graphic depiction of how mobilizational capacity influences the risk of conflict for a simplified model that does not distinguish among the categories of excluded groups or different levels of GDP per capita. The lower baseline curve shows that the propensity of conflict in the name of excluded groups that have neither experienced a fall in power status or prior conflict increases slightly as group size increases (H2). In comparison, the predicted increase is steeper for groups that experienced a past conflict (H3) and dramatic in the presence of a downgraded power status (H1b).

[Figure 3 about here]

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Analysis of elite infighting

Finally, we shift the attention from excluded to included groups. Here we consider only cases of power sharing, i.e. those that involve senior and junior partners of governing coalitions, because monopoly and dominant groups are directly identified with the government, and thus by definition cannot challenge it. Thus, our goal is to understand why some power-sharing partners get involved in conflict with the government that they have so far been included in. Following our hypothesis H1b and H1c, we expect that underrepresented power sharing partners as well as those who have experienced a downgrading in their power position will be more likely to take up arms against other members of the governing coalition.

Before discussing results, a cautionary remark is in order. As indicated by Table 2, there are very few cases of conflict instigated in the name of groups in power, only 27 onsets in 8079 group years. This means that the results of the statistical evaluation are likely to be sensitive to the coding of this small number of conflict events. With this caveat in mind, we turn to Model 4 in Table 2. The results indicate that conflicts in the name of junior groups are not more frequent than those fought in the name of senior ones (H1a). However, the regression analysis suggests that a downgraded power status has an impact on conflict behavior, thus confirming H1b. We also get strong results for hypothesis H1c that relates to resentment caused by under-representation within governing coalitions. However, group size has no influence on the dependent variable in contrast to the effect for excluded groups in Model 3, thus disconfirming H2 for power sharing partners. It seems that the mobilizational capacity argument does not hold for groups that already have access to organizational apparatus of government. Furthermore, the path dependency effect (H3) cannot be confirmed for the current sub-sample. Finally, we fail to find any significant effect for any of the control variables.

Given the rareness of these conflict events, it is appropriate to examine individual cases. In particular, it should be noted that the events influencing the coefficients on downgraded power status (H1b) and under-

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representation (H1c) refer to cases of non-secessionist conflict only. The 10 secessionist conflicts are not well predicted by our model. Typically, these secessionist conflicts involved radicalized splinter groups that were not part of a power sharing arrangement, such as the United Liberation Front of Assam (Baruah 1994), the Sikh Sangat organization (Chima 1994), or the Albanian UCK/KLA rebels in Macedonia (Lund 2005), were dissatisfied with the power-sharing deal offered by the government and therefore resorted to violence. These cases illustrate the limitations in our research design. Clearly, in these cases, different organizations claim to represent the interests of an ethnic group, and if one organization participates in government, this does not prevent others from taking an oppositional stand to mount a rebellion in the name of "true" group interests. From a theoretical perspective, this outcome is not surprising because it is, after all, parties and their representatives that are in government rather than entire groups. In other cases, the fear of future domination by coalition partners triggered secessionist bids that turned violent, as in former Yugoslavia and Bosnia and Herzegovina (Kalyvas and Sambanis 2005). In principle, the fear of a future fall from power is consistent with our theory, although it cannot be captured with existing indicators without addressing severe endogeneity problems.

The 17 non-secessionist instances of elite infighting are somewhat more numerous than their secessionist counter parts, but the numbers are sufficiently small to warrant case-by-case analysis. How supportive is the evidence in favor of the main hypotheses relating to a fall in power status (H1b)? We find only two instances of conflicts within two years of a downgraded power status. After a long period of dominance by President Eyadéma, who favored his own Kabré group in Togo's army and administration, he came under pressure to democratize and had to agree to power-sharing in 1991. Despite their status as senior partner, he and his entourage in the army quickly became unhappy with the power-sharing arrangement and launched a coup later that same year. Meanwhile, the 1984 coup in Cameroon can be seen as a reaction to the shift of power from the Fulani-dominated regime of President Ahidjo to the government of Biya, a Beti politician (Mehler 1993).[14]

As regards conflicts driven by under-representation within the governing coalition (H1c), the dataset point us to three potential cases, namely the Sara 1991 rebellion in Chad, the Hutu 1991 uprising in Burundi, and Shiite participation in the Lebanese civil war in 1975. Rather than being directly motivated by under-representation, the first of these three conflict appears to be more related to long-term frustrations felt by the Sara, who had dominated the state's early history from independence in 1960 through 1978 (Decalo 1980), thus in line with the downgrading hypothesis (H1b).[15] In the other two events, however, the conflict is indeed related to the under-representation mechanism. In 1991, rebels who claimed to represent the Hutu majority launched an armed struggle against the Tutsi-dominated power sharing arrangement (Lemarchand 1994). In Lebanon, the Shiite political elite were increasingly resentful that the Maronites were overrepresented in the traditional consociational arrangement guaranteeing them the presidency, especially because the Shiite population had increased over the past decades (Makdisi and Sadaka 2005, p. 61).

According to our data, the influence of past civil conflict plays a role in some cases of elite infighting. There are obvious instances, such as the rebellions in Chad that happened against the backdrop of a long-lasting civil war. Nor can there be any doubt that that the Hutu defection from the Tutsi-dominated governing coalition in 1991 was influenced by memories of past massacres. A similar argument can be made with respect to the failure of the power sharing arrangement in Lebanon in 1975, because Shiite and Sunni leaders had already mobilized their constituencies in the conflict against the Maronite elite in 1958. In all three cases, the past conflict variable thus picks up meaningful historical processes.

To sum up, the historical evidence in support of Model 4 is strong, although the results are driven by non-secessionist conflicts. The hypotheses relating to the downgrading and underrepresentation mechanisms

(H1b,c) are tentatively supported by a case-by-case inspection of the non-secessionist cases of elite infighting.

Sensitivity analysis

Table 3 presents robustness checks of our findings based on Models 1 and 3 of Table 2. We start by considering the robustness of Model 1 in the presence of dummy variables that divide countries into six world regions, using the Western World as the reference category (see Model 5). The only regional variable that approaches a significantly positive effect is Asia ($p = 0.094$). More importantly, we do not detect any major differences in the effect of our main explanatory variables: exclusion, downgraded, group size and conflict history remain powerful even after world regions have been controlled for (see Hypotheses 1-3). In contrast, the GDP variable loses some of its impact and is now only significant at the $p = 0.086$ level.

[Table 3 about here]

To further control for local variations in Model 1, we introduce a model with country fixed effects (see Model 6), allowing the constant to vary independently for each state. This research design captures country-specific factors that are not captured by our independent variables. The result of this exercise confirms the robustness of our main hypotheses, except H3. Whereas the status variables and size indicator influence the dependent variable, the impact of past conflict cannot be statistically confirmed due to the low variance among the country-observations that experienced conflict. Moreover, fixed-effects estimation forces us to throw out all countries that never experience conflict, which is a major weakness (Beck and Katz 2001).

In order to test whether our models are sensitive to our inclusion of low and high intensity conflicts, we replace the coding of the dependent variable based on the 25 battle deaths threshold with one that conforms to the standard 1000 civil war threshold, while otherwise keeping Model 1 unchanged. Model 7

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shows that our results hold up for this smaller set of high-intensity conflicts. The variables associated with the main hypotheses retain their effect, which is also the case for the GDP per capita variable. Our indicator for regime type, however, loses much of its explanatory power.

Finally, by using Model 3 as the point of departure, Model 8 introduces a more precise way of measuring the power balance between excluded groups and the government. Instead of using logged demographic group sizes only, the logged *Power balance* variable measures the excluded group's size divided by the number of troops controlled by the government, as recorded in the National Material Capabilities dataset of the COW project. This indicator measures relational properties explicitly but there are no data past 2001, resulting in fewer observations. The power balance variable is highly significant suggesting that excluded groups that are larger relative to the size of the government military are more likely to initiate conflict (H2). However, the past conflict variable is now no longer statistically significant.

Our results are robust to various changes in the operationalization of our key variables (not shown here). For example, varying the two-year window used to calculate the *Downgraded* variable to one, three, four, or five year windows makes no substantive difference to our results. As discussed in the context of elite infighting, the coding of our dependent variable allows us to check whether our findings differ for secessionist and non-secessionist conflicts. Whereas this is the case for elite infighting, all the main hypotheses hold robustly for secessionist and non-secessionist conflicts fought in the name of excluded groups.

Conclusion

In this paper, we have investigated the influence of ethnic power structures on civil war and found that ethnic exclusion and competition are strongly associated with internal conflict. We hope that these findings will take the debate about the role of ethnicity in armed conflict one step further. Rather than

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continuing to debate whether or not ethnic diversity breeds conflict, quantitative research should try to identify those ethnic constellations of power that are particularly war-prone. Once ethnic politics is properly conceptualized and measured, it is indeed possible to identify the structural conditions under which ethnic civil wars are likely to break out. Our analysis shows that large ethnic groups that are excluded from state power or under-represented in government are much more likely to challenge the regime's insiders through violent means. A loss of power in recent history or previous conflict further increases the likelihood of armed conflict.

Readers who are familiar with the qualitative scholarship on ethnic conflict may not be surprised that ethnic groups whose representatives have less access to power are more likely to challenge the government. However, this finding is far from trivial or obvious. First, the most widely cited articles in the civil war literature have maintained that neither high degrees of ethnic diversity nor political exclusion along ethnic lines increases the likelihood of conflict (Fearon and Laitin 2003; Collier and Hoeffler 2004). Our findings challenge this view by showing which ethno-political configurations of power and which dynamics of contention and mobilization are most likely to trigger conflict. Second, our results are inherently falsifiable. If our theory were wrong, we would have found no significant relationship between power status and conflict at all or one pointing in the opposite direction.

How do our findings relate to existing research on ethnic conflict? Past quantitative tests of the exclusion hypothesis have produced conflicting results. Whereas some researchers find evidence in favor of this claim (e.g. Gurr 1993), others obtain mixed results (e.g. Olzak 2006), or fail to find any support at all (e.g. Fearon and Laitin 2003). Our research suggests that the literature's inconclusiveness is due to measurement problems and data limitations. Most researchers rely on Minority At Risk data, and define exclusion narrowly, focusing on specific types of minority rights rather than explicitly measuring access to state power. We believe that our new dataset on ethnic power relations (EPR) represents significant

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progress by enabling more precise coding of groups' broader political representation in state governments, including that of dominant majorities or power-sharing partners. Since the EPR dataset is time sensitive, it also allows to trace changes in power access over time and to determine whether such changes have an effect on the conflict dynamic. And indeed, we were able to show that a previous loss of power increases the likelihood of armed rebellion quite substantially.

While improving on previous studies of ethnic conflict, our research leaves open at least two issues that we hope future research will be able to address. First, our focus has been mostly on structural conditions rather than on dynamic processes of contention and mobilization involving governmental and oppositional actors. Second, we have not tried to explain changes in power structures and group boundaries, but have treated these as exogenously given. This study is written as an invitation to other researchers to overcome these limitations by collecting more precise data on ethnic organizations, their mobilizational capacities and power resources, their interactions with each other as well as with non-ethnic organizations and governmental actors, inside or across state borders. Ideally, such data would make it possible to develop a fully processual theory as a way to analyze ethnic conflicts as one element among others in the broader struggle over state power.
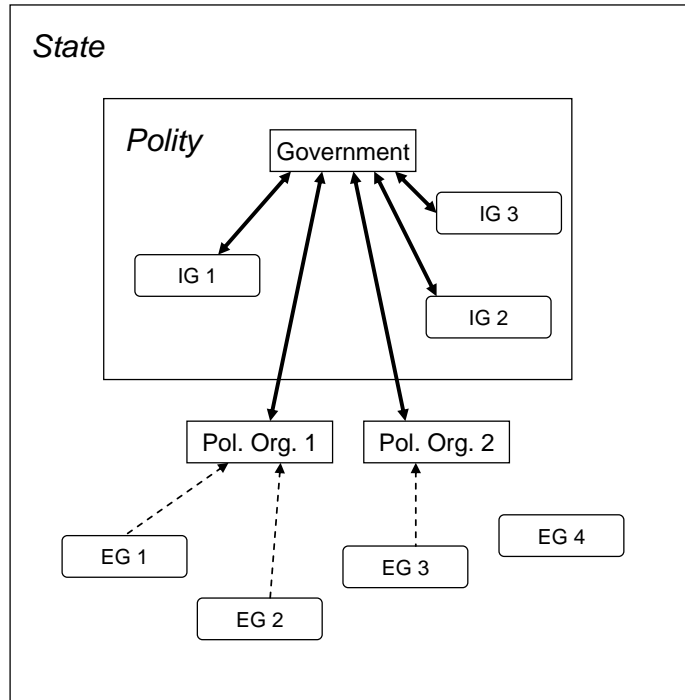
Figure 1. The polity model with included and excluded ethnic groups (IG and EG respectively).
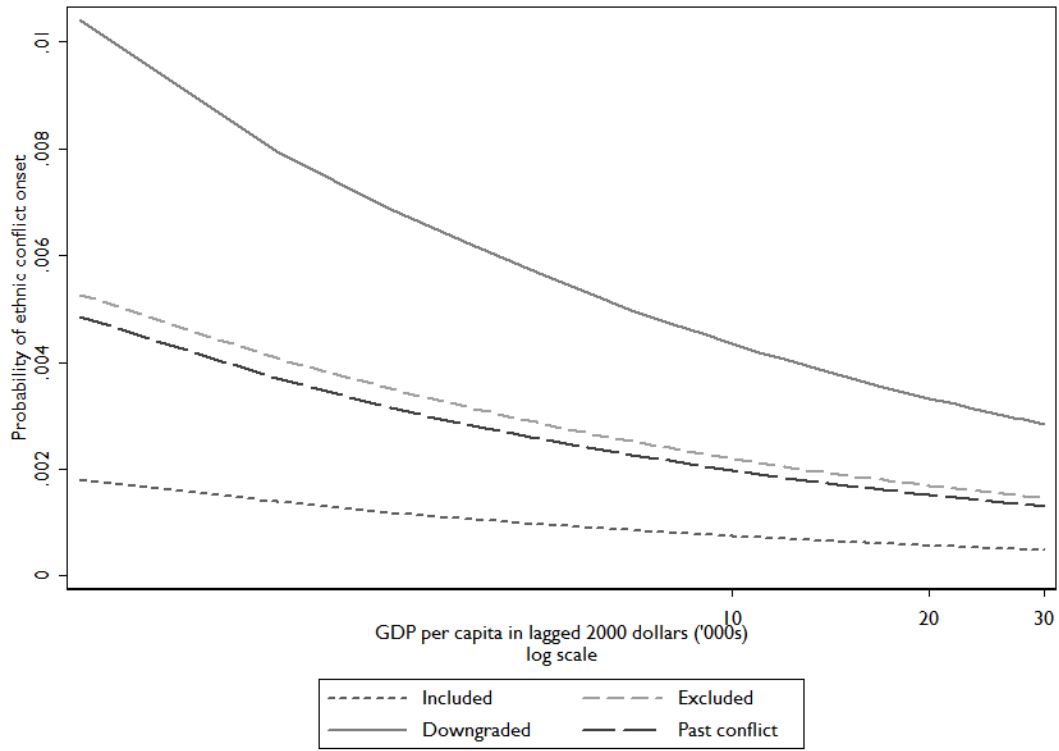
Figure 2. The effect of GDP per capita on ethnic groups' conflict propensity
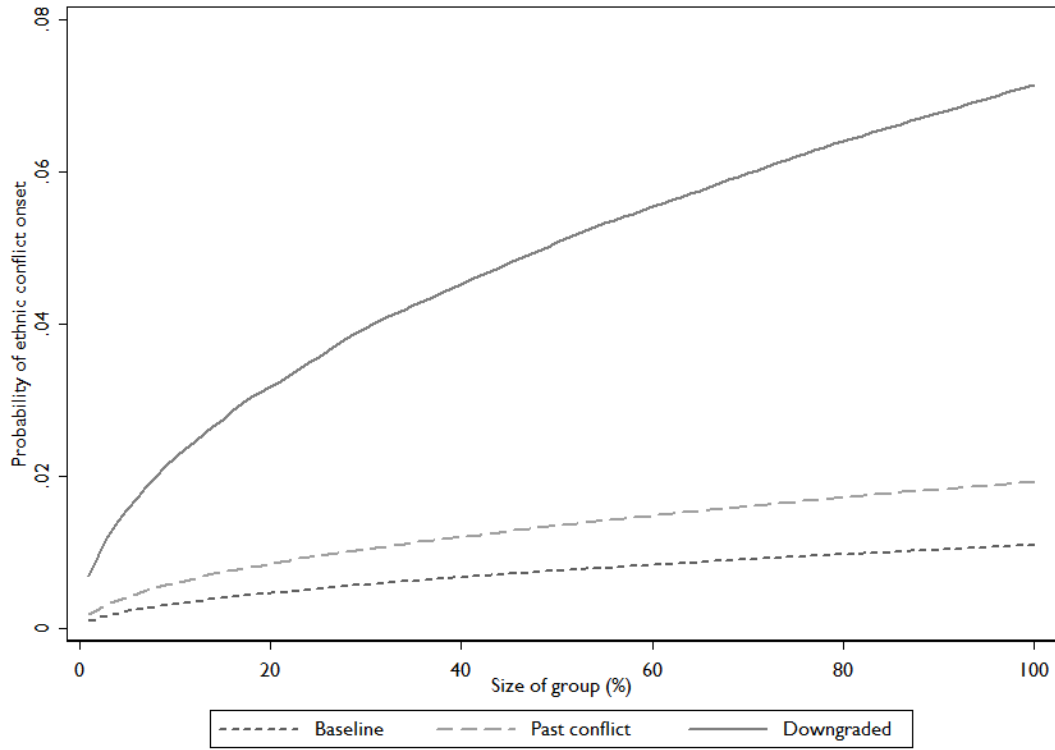
Figure 3. The effect of excluded ethnic groups' size on their conflict propensity

Tables

Table 1. Ethnic groups' conflict propensity by category of power access

	Group Years	Years of ethnic-conflict onset	
Included groups	11,667	27	0.23%
Monopoly*	1,554	0	0%
Dominant*	1,708	0	0%
Senior Partner	2,983	9	0.30%
Junior Partner	5,422	18	0.33%
Excluded groups	20,368	120	0.59%
Regional Autonomy	5,877	13	0.22%
Powerless	8.869	45	0.50%
Discriminated	5,170	43	0.82%
Separatist Autonomy	452	19	4.03%
Total	32,035	147	0.46%

*) No conflict by definition

Table 2. Explaining group-level ethno-nationalist conflict

	(1) All groups	(2) All groups	(3) Excluded groups only	(4) Included groups only
Excluded	1.1195**			
	(0.2855)			
Junior partner		0.4760		-0.1177
		(0.4283)		(0.3977)
Regional autonomy		0.9174		
		(0.5534)		
Powerless		1.2234**	0.3385	
		(0.4726)	(0.3808)	
Discriminated		1.7876**	0.8941*	
		(0.4675)	(0.4307)	
Separatist autonomy		3.4866**	2.6888**	
		(0.7096)	(0.8051)	
Downgraded	1.7115**	1.8123**	1.8702**	1.8390*
	(0.3898)	(0.3908)	(0.4376)	(0.8655)
log (Group size)	0.3703**	0.4095**	0.5085**	-0.2293
	(0.0924)	(0.0934)	(0.1068)	(0.3725)
Under-represented				0.1242*
				(0.0563)
Past conflict	0.9753**	0.7048**	0.6798*	0.6505
	(0.2043)	(0.2664)	(0.3044)	(1.0664)
log (GDP per capita), lagged	-0.4049**	-0.3772**	-0.3894**	-0.2124
	(0.1204)	(0.1049)	(0.1320)	(0.1863)
log (population), lagged	0.0077	0.0350	0.0928	-0.1085
	(0.1078)	(0.1084)	(0.1259)	(0.1417)
Anocracy, lagged	0.5644*	0.5845*	0.6249*	0.3939
	(0.2531)	(0.2390)	(0.2517)	(0.6718)
Years since last conflict onset	-0.1400*	-0.1249	-0.1328	-0.0507
	(0.0638)	(0.0653)	(0.0729)	(0.1913)
Peace years spline1	-0.0007	-0.0006	-0.0006	-0.0003
	(0.0005)	(0.0005)	(0.0005)	(0.0011)
Peace years spline2	0.0002	0.0002	0.0002	0.0000
	(0.0003)	(0.0003)	(0.0004)	(0.0008)
Peace years spline3	0.0001	0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0003)
Constant	-3.6608*	-4.6679**	-4.4770*	-2.7416
	(1.4723)	(1.5601)	(1.8713)	(2.8066)
Observations	26828	26828	18961	7811

Robust standard errors clustered on country in parentheses,
** $p < 0.01$, * $p < 0.05$

Table 3. Sensitivity analysis

	(5) Region controls	(6) Country fixed effects	(7) High intensity onsets only	(8) Testing power balance (excluded groups only)
Excluded	1.2298**	1.4005**	1.3389**	
	(0.2819)	(0.2982)	(0.4177)	
Powerless				0.2173
				(0.3870)
Discriminated				0.6664
				(0.4478)
Separatist				2.5239**
				(0.8153)
Downgraded	1.5835**	1.5588**	1.9433**	1.4964**
	(0.4009)	(0.3205)	(0.4909)	(0.4023)
log (Group size)	0.3091**	0.2902**	0.4685**	
	(0.1142)	(0.1052)	(0.1199)	
log (Power balance)				0.2929**
				(0.0582)
Past conflict	0.9603**	0.1992	0.8053*	0.6026
	(0.2170)	(0.2480)	(0.3271)	(0.4362)
log (GDP per capita), lagged	-0.2145	0.0835	-0.5633**	-0.3403*
	(0.1251)	(0.2606)	(0.1448)	(0.1503)
log (population), lagged	-0.0844	0.2818	-0.0471	0.0643
	(0.1116)	(0.3873)	(0.1011)	(0.1383)
Anocracy, lagged	0.6011*	0.6435**	0.4488	0.4310
	(0.2461)	(0.2413)	(0.3593)	(0.2693)
Years since last conflict onset	-0.1436*	-0.1230*	-0.1755*	-0.1582*
	(0.0652)	(0.0590)	(0.0858)	(0.0735)
Peace years splines omitted				
North Africa and Middle East	0.9549			
	(0.6574)			
Latin America	-0.4252			
	(0.7471)			
Sub-Saharan Africa	1.0429			
	(0.6791)			
Eastern Europe	0.7352			
	(0.6505)			
Asia	1.2071			
	(0.7207)			
Constant	-5.0037**	-25.1063**	-2.6344	-2.0274
	(1.8011)	(4.6588)	(1.7291)	(1.9515)
Observations	26828	16057	26828	17179

Robust standard errors clustered on country in parentheses,
** $p<0.01$, * $p<0.05$

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Endnotes

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[1] This is the title of Macy and Willer's (2002) article that advocates a similar position based on a computational way to disaggregate social systems. See also Abbott (2002) for a sophisticated argument that theoretical explanations need to feature entities and their relations rather than merely variables.

[2] Using different model specifications, other quantitative studies report a positive impact of ethnic fractionalization on civil war onset (cf. Hegre and Sambanis 2006; Blimes 2006).

[3] Of course the capacity to mobilize nationalist campaigns depends on more than sheer numbers. In particular, mobilization hinges crucially on the group's internal structure and its political organization (Tilly 1978; Gurr 2000).

[4] It is also possible that conflict experiences involving other groups increase the likelihood of ethno-nationalist violence through diffusion mechanisms (see Lake and Rothchild 1998).

[5] The dataset includes politically relevant ethnic groups in all 156 sovereign states with a population of at least 1 million and a surface area of at least 500,000 square kilometers as of 2005.

[6] The process of contacting and interacting with country experts took almost two years. Once sufficient coding responses were available workshops were held to decide on the final coding. We discussed each coding in the light of comments by the experts present, as well as additional data sources and the accumulating comparative knowledge of the project team itself. In many cases, it proved necessary to get back to the initial coders or to invite additional experts to help us synthesize the data.

[7] It should be noted that political relevance does not equal violent conflict. Endogeneity is not a problem because only a tiny fraction of years with mobilized or discriminated groups were characterized by conflict (0.45%).

[8] We did not distinguish between degrees of representativity of political actors who claim to speak in the name of an ethnic group, nor did we code the heterogeneity of political positions voiced by leaders claiming to represent the same community (cf. Brubaker 2004; Zürcher 2007, pp. 39-40).

[9] The choice between "senior" and "junior" depends on the number and relative importance of the positions controlled by group members. For example, in ethnic party systems such as that of Malaysia, the Malay governing party plays the role of a senior partner, whereas the Chinese party is a junior partner.

[10] We did not include in this category a) groups who are suffering from indirect discrimination because they are disadvantaged in the economic sphere or the educational sector and thus unlikely to be able to successfully compete in the political arena; b) general social discrimination (on the labor and marriage markets etc.) c) the exclusion of non-citizens from power, as long as these non-citizens hold passports of other states and are effectively able to return to their country of origin.

[11] In keeping with the disaggregated focus on this study, we drew primarily on version 3-2005b of the ACD dataset which provides two levels of conflict identification, a more general war ID number and a disaggregated sub-ID that identifies whenever the constellation of rebel organizations changes completely or when more than ten years elapse between episodes of violence.

[12] Because all separatist groups get involved in violence at some point, the coding of regional versus separatist autonomy was verified case by case. Endogeneity may still be a residual problem, but removing the separatist cases from the dataset makes no difference to our other findings.

[13] Adding a dummy variable for stable democracies fails to make any notable difference to the results.

[14] The rebellion by the Jamaat al-Muslimee in Trinidad in 1990 was a reaction to the fact that the dominant Creole/black community had to start sharing power with the descendents of Indian plantation laborers—though this fact is not captured by the downgraded variable when defined by a two years window.

[15] The *downgraded* variable does not pick up this case because it is based on a two-year window.