Ethnic Politics and Armed Conflict: A Configurational Analysis of a New Global Data Set

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Quantitative scholarship on civil wars has long debated whether ethnic diversity breeds armed conflict. We go beyond this debate and show that highly diverse societies are not more conflict prone. Rather, states characterized by certain ethnopolitical configurations of power are more likely to experience violent conflict. First, armed rebellions are more likely to challenge states that exclude large portions of the population on the basis of ethnic background. Second, when a large number of competing elites share power in a segmented state, the risk of violent infighting increases. Third, incohesive states with a short history of direct rule are more likely to experience secessionist conflicts. We test these hypotheses for all independent states since 1945 using the new Ethnic Power Relations (EPR) data set. Cross-national analysis demonstrates that ethnic politics is as powerful and robust in predicting civil wars as is a country's level of economic development. Using multinomial logit regression, we show that rebellion, infighting, and secession result from high degrees of exclusion, segmentation, and incohesion, respectively. More diverse states, on the other hand, are not more likely to suffer from violent conflict.

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Karl Marx predicted that revolutionary class struggles would transform the world during the twentieth century. Instead, it turned out to be the age of ethnonationalist conflicts. Wars fought in the name of national liberation or ethnic autonomy comprise only one fifth of the wars between the Congress of Vienna (1814) and the Treaty of Versailles (1919). From Versailles to 2001, however, the share of ethnonationalist wars rose to 45 percent, and since the Cold War ended it has reached 75 percent.[1] Ethnic demands and grievances play a promi-

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[1] These figures are based on the data set assembled by Wimmer and Min (2006) and concern wars with more than 1,000 battle deaths.

nent role in most conflicts reported in the daily news—from Iraq to Darfur, Kenya to Tibet, Israel and Palestine to Burma. What can the social sciences offer to an understanding of these conflicts? When do lines of conflict follow ethnic divides and what are the causal mechanisms linking ethnicity to conflict?

There is no satisfactory answer to these questions in the burgeoning quantitative literature on civil wars that has emerged over the past decade. The most influential school of thought dismisses ethnicity as an explanatory factor altogether, arguing that ethnic grievances are too widespread to explain the rare event of civil war. In this view, rebels fight wherever governments are militarily weak or lootable resources can feed an insurgent organization (the greed-and-opportunity perspective). Other scholars maintain that ethnicity does matter, and that more ethnically diverse states are more likely to see conflict (the diversity-breeds-conflict tradition). Yet a third group examines the conditions under which discriminated ethnic minorities will rebel (the minority-mobilization school). We argue that all three traditions tend to misconceive the relationship between ethnicity and conflict.

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To get this relationship right, we first need to recognize that the modern state is not an ethnically neutral actor or a mere arena for political competition, but a central object of and participant in ethnopolitical power struggles. Why is this the case? Our answer takes an institutionalist point of departure. Contrary to empires, nation-states are governed in the name of "their peoples," which provides incentives to align political loyalties along ethnic divides. To gain legitimacy, political elites in control of executive-level state power will favor co-ethnics when deciding with whom to ally and to whom to distribute public goods. Politics will then center on the question of which ethnic group controls which share of executive government, and the struggle over state power will pit ethnically defined actors against each other. In this view, ethnic politics is not exclusively a struggle to rectify the grievances of minority groups, as the minority-mobilization school assumes, but it is more generally and fundamentally about the distribution of state power along ethnic lines. The diversity-breeds-conflict school relies on demographic indices of heterogeneity that overlook how ethnicity relates to the state. Rather than high degrees of diversity, it is ethnic exclusion from state power and competition over the spoils of government that breed ethnic conflict.

We propose a configurational model that identifies three constellations in which this struggle over the state is most likely to escalate into armed conflict. First, armed rebellions are more likely when the state excludes large sections of the population from central state power on the basis of their ethnic background. Second, the likelihood of infighting increases when a large number of ethnic elites shares government power and engages in competitive rivalry. Third, both rebellion and infighting will be more likely and take on secessionist forms when segments of the population have a short and troubled history of direct rule by the center. We examine these hypotheses with quantitative analysis of all states since World War II using a new data set on Ethnic Power Relations (EPR). This data set records all politically relevant ethnic groups, minorities and majorities, and their degree of access to executive-level state power—from total control of the government to overt political discrimination and exclusion. The EPR data set overcomes the limitations of existing data sets, especially the widely used Minorities at Risk data set, which focuses exclusively on disadvantaged minorities and is thus unable to capture the dynamics of ethnic politics at the power center. The EPR data set is also an improvement over conventional demographic indices of diversity that are only tangentially related to the ethnopolitical struggle over the state.

Ethnic politics, our findings reveal, helps to explain the dynamics of war and peace, contrary to what the greed-and-opportunity school maintains. Second, our results demonstrate that more diverse states are not more war-prone, in contrast to the expectations of the diversity-breeds-conflict school. Third, disaggregated analysis using multinomial logit regressions shows that different kinds of ethnic conflicts result from different causal processes: rebellions are more likely the higher the share of the excluded population; the chance of infighting increases as the number of power sharing elites augments; and secessions are more frequent in incohesive states that lack a long history of direct rule by the center. We thus follow in the footsteps of other scholars in the quantitative literature who argue that different types of wars have different caus-

es (Buhaug 2006; Sambanis 2001), and we support the recent trend of closely investigating the various mechanisms that lead to armed conflicts (Kalyvas 2007).

ETHNICITY AND CONFLICT: GETTING THE RELATIONSHIP RIGHT

Two major shortcomings characterize the quantitative literature on ethnicity and violence. First, the mechanisms linking ethnicity to conflict are specified in theoretically problematic and empirically unsatisfactory ways. Second, quantitative approaches tend to overaggregate the dependent variable and treat ethnic conflicts as though they have uniform causes. We first discuss the problem of specifying relevant mechanisms, focusing on three prominent schools of quantitative research on the outbreak of civil wars: greed and opportunity, ethnic diversity breeds conflict, and minority mobilization.

The most influential articles argue that ethnicity plays no role in predicting the onset of civil wars. According to authors in this tradition, the increase in ethnic conflicts during the twentieth century does not capture any meaningful trend, but is due to the unfortunate tendency of both scholarly observers and rebels themselves to attribute conflict to primordial ethnic identities—a collective delusion of sorts (Laitin 2007:20–27). More important than ethnic identity or political exclusion along ethnic lines are the material and organizational incentives to stage a rebellion against government. According to Fearon and Laitin's (2003) well-known insurgency model, wars erupt when governments are weak and rebels have ample opportunities to hide from troops while recruiting unemployed young men for whatever cause: national liberation, revolutionary progress, the spread of true religion, or rich bounty. Similarly, Collier and Hoeffler (2004) maintain that civil wars occur where rebellions are most feasible, rather than where actors are motivated by ethnic inequality or social marginalization. More specifically, they argue that lootable economic resources make organizing and sustaining a rebel organization easier (see also Collier, Hoeffler, and Rohner 2006).

A second group of scholars insists that ethnicity *does* matter. They suggest various reasons why ethnically diverse states experience more conflict. Some argue that high degrees of ethnic diversity contradict the assumption of cultural homogeneity on which modern nation-states are based, thus triggering waves of separatist wars and ethnic cleansings (Gellner 1991; Nairn 1993). Vanhanen (1999), the most ardent proponent of the diversity-breeds-conflict argument, relies on van den Berghe's sociobiological theory of ethnic nepotism, according to which humans tend to favor kin and quasi-kin, such as co-ethnics, over others. As a result, more ethnically heterogeneous states will have more conflict. Finally, Sambanis (2001) draws on organizational economy models to argue that more ethnically divided societies face higher risks of ethnic war because shared ethnicity decreases the collective action costs associated with organizing a rebel force. Since the likelihood of ethnic rebellion does not depend on group size, he expects "the relationship between ethnic war and ethnic divisions [to be] linear and positive" (Sambanis 2001:266; see also Easterly and Levine 1997).

These two positions—the greed-and-opportunity school and the diversity-breeds-conflict tradition—rely on the same type of demographic diversity indicators to test their core assumption regarding ethnicity and conflict. Many use a linguistic fractionalization index, calculated as the likelihood that two randomly drawn individuals would speak a different language. This is a poor indicator for capturing the political dynamics associated with ethnic conflict. First, not all ethnic groups matter for politics (Chandra and Wilkinson 2008; Posner 2004). Second, ethnic conflicts are not the outcome of everyday encounters between individuals; they are the result of interactions between the state and ethnopolitical movements that challenge state authority (Cederman and Girardin 2007).

Given these conceptual and measurement problems, it is not surprising that empirical studies produce conflicting results when using fractionalization indices. Some find that ethnic fractionalization does not explain high-intensity conflicts (defined as more than 1,000 battle deaths per year) (Collier and Hoeffler 2004; Fearon and Laitin 2003). Others show that ethnic fractionalization is very important if the dependent variable includes low-intensity wars (Hegre and Sambanis 2006) or if one focuses on ethnic wars (Sambanis 2001) or secessionist conflicts only (Buhaug 2006). Some find a par-

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abolic relationship between ethnic fractionalization and the prevalence of civil war (Elbadawi and Sambanis 2000). Still others maintain that polarization between two equally sized ethnic groups, rather than fractionalization, best explains conflict (Montalvo and Reynal-Querol 2005).[2]

We move beyond these demographic indicators of ethnic diversity in the analyses that follow by introducing a new data set that records politically relevant groups and their access to executive state power. This allows for a direct test of how ethnic politics affects war and peace, rather than relying on demographic proxies far removed from how ethnicity works in political practice. Once we account for the political dynamics of ethnic exclusion and competition, diversity in and of itself has no effect on the likelihood of civil conflict.

The third major approach is the minority-mobilization school. These scholars analyze the relationship between ethnicity and conflict at the group level, rather than the state level. Coming from a political mobilization perspective, Gurr (1993a) and others explore the conditions under which ethnic minorities protest or rebel. They find various factors that account for the political behavior of ethnic groups, including, as will be familiar to students of social movements, the strength of communal grievances and the political opportunity structure provided by different political regimes. Gurr and colleagues have also assembled a large, worldwide data set on these "Minorities at Risk" (MAR). The MAR data set has produced a quantum leap in the study of ethnic politics and has provided an invaluable service to researchers in political science (Elkins and Sides 2007; Saideman and Ayres 2000; Toft 2003; Walter 2006) and sociology (Chai 2005; Olzak 2006).

The minority-mobilization perspective comes much closer than the other schools to the empirically observable mechanisms linking ethnicity to conflict. We thus incorporate some of their insights into the model of ethnic politics developed below. Their perspective, however, is limited by its focus on minority groups only. This has two consequences. First, the state appears as ethnically neutral, making it impossible to grasp the dynamics of ethnic politics in the power center. Second, the MAR coding scheme does not fit countries with ruling minorities or complex coalitions of ethnically defined elites (e.g., Nigeria, India, and Chad).[3] In such countries, ethnic conflict will be pursued in the name of excluded majorities (rather than minorities) or ethnic groups that share power (and are thus not at risk). Roughly half the observations in our data set conform to such ethnopolitical constellations and thus escape the logic of the MAR approach. By reducing its focus to the political mobilization of discriminated minorities, the minority-mobilization model overspecifies the conditions under which ethnicity leads to conflict.

All major schools in the quantitative literature fail to specify convincing mechanisms linking ethnicity and conflict. They either rely on a version of the ethnic diversity argument that is unrelated to the logic of ethnic politics, or they define ethnic conflicts too narrowly as a matter of minority mobilization. A second problem in the existing literature is that it conceives ethnic conflict as a unitary phenomenon caused by uniform factors.[4] Qualitative comparative work shows the importance of taking different ethnopolitical constellations into account and of acknowledging the causal heterogeneity of the processes that lead to ethnic conflict. The following four vignettes of well-known ethnic conflicts illustrate this point.

In Ireland, when segments of the educated Catholic middle class, inspired by the U.S. civil rights movement, mobilized against their long-standing exclusion from power, the state appa-

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[2] Ellingsen (2000) finds support for both a linear relationship to fractionalization and a U-shaped relationship to polarization.

[3] The MAR data set tries to address these limitations by including five "advantaged" minorities who benefit from political discrimination and control a state apparatus. MAR also includes a series of "communal contenders" (i.e., groups that share power with others while at the same time mobilizing in protest or rebellion); these are mostly in Africa (Gurr 1993b). Ethnically defined elites that do not mobilize their constituencies in protest are omitted.

[4] The MAR data set comes closest to a more disaggregated perspective by coding different types of ethnic groups. Gurr's (1993b) analysis, however, mostly focuses on the difference between peaceful protest and violent rebellion, irrespective of these group differences.

ratus, controlled entirely by Protestants elites who ruled Northern Ireland as an outpost of the British state, reacted with repression and intimidation. The ensuing escalation reinvigorated the Irish nationalist underground army, which fought to unite Northern Ireland with the rest of the country. This in turn led to the emergence of Protestant militias and terrorist groups opposed to the nationalist project (Bardon 2001).

In Bosnia shortly before independence, the leadership of the Serbian territories withdrew from the provincial government they had shared with Croatian and Bosniak politicians. Mobilization for war proceeded quickly on both sides. Serbian militias, supported by the army of neighboring Yugoslavia, soon attacked Croatian and Bosniak villages that they intended to incorporate into the territory of a future Serbian state (Burg and Shoup 1999).

In January 1994, the now iconic *commandante* Marcos led a group of masked men and women to the main square of San Cristobal de la Casas and announced that the indigenous peoples of Chiapas and Mexico would no longer accept their fate as second-class citizens. He demanded profound constitutional, economic, and political change. Decades of political mobilization preceded his rebellion, including left-wing organizations fighting for land reform and members of the lower clergy inspired by liberation theology. The central government reacted to this provocation by sending the army to occupy indigenous villages that supposedly harbored members of the Zapatista army. After a series of armed encounters, the Zapatistas eventually withdrew into the Lacandon jungle (Collier and Lowery Quaratiello 1994; Wimmer 1995).

Most recently, in Iraq after the fall of Saddam Hussein, former Baathist officers and high level functionaries joined Sunni clerics, tribal leaders from the Sunni triangle, and foreign *jihadists* in a fragile alliance to fight the new power holders from the Shiite south of the country. They struggled against what they perceived as an illegitimate government controlled by Shiite apostates and Kurdish separatists. Opposing any federalization and power sharing on the national level, they dreamt of restoring the ethnocratic regime they once controlled. Meanwhile, factions within the Shiite block jockeyed for power, exploiting the unpopularity of the new government and its dependence on U.S. military power. The Sadr Army harnessed the support of marginalized urban youth to oppose power sharing with Sunni and Kurdish political parties, advocating instead a strong, central state under Shiite command (Bengio 2004; Cole 2003; Wimmer 2003).

The factors affecting these four conflicts and the mechanisms at play are quite different. While Irish Catholics and indigenous *Chiapanecos* represent excluded groups that mobilized against the state, representatives of Bosnian Serbs and Shiite Arabs were partners in coalitional governments. Serbian Bosniak elites and Iraqi ethnoreligious factions faced a disorganized and ethnically fragmented state, while Catholics in Northern Ireland and the Zapatistas in Mexico opposed an entrenched state apparatus. The IRA and the Bosnian Serb nationalists developed separatist agendas aimed at joining established neighboring states, while the Zapatistas and Iraqi groups focused on changing ethnic power relations within existing states. It seems doubtful that any single indicator can accurately grasp these different ethnopolitical dynamics. The power configurations are different, as are the mechanisms and logic relating ethnicity to conflict. In the following discussion, we introduce a configurational approach that links different ethnopolitical constellations with distinct causal pathways leading to specific types of ethnic conflict.

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AN INSTITUTIONALIST, CONFIGURATIONAL THEORY OF ETHNIC POLITICS AND CONFLICT

Our theory of ethnic politics and conflict is based on two pillars. First, we rely on institutionalist theories that show how established structures of political legitimacy provide incentives for actors to pursue certain types of political strategies. Second, our model follows a configurational logic. Depending on the configuration of political power, similar political institutions can produce different consequences, while similar consequences can result from different constellations of power. The institutionalist part of the argument specifies the conditions under which political loyalties will align along ethnic cleavages; the configurational part explains when we expect such ethnic politics to lead to armed violence.

Institutional Incentives for Ethnic Politics

We derive the institutionalist part of the argument from Wimmer's (2002) theory of nation-state formation and ethnic politics. It states that ethnicity matters for politics, not because of a universal, naturally-given tendency to favor (ethnic) kin over non-kin (as sociobiologists argue), nor because of a primordial attachment of individuals to their identities, nor because it provides lower costs for political organization (as the political economy tradition maintains). Rather, ethnicity matters because the nation-state itself relies on ethnonational principles of political legitimacy: the state is ruled in the name of an ethnically defined people and rulers should therefore care for "their own people." As a result, ethnicity and nationhood have much greater political significance in nation-states than they do in other types of polities such as empires or city-states.

Given this institutional environment, political office holders have incentives to gain legitimacy by favoring co-ethnics or co-nationals over others when distributing public goods and government jobs; judiciary bodies have incentives to apply the principle of equality before the law more for co-ethnics or co-nationals than for others; the police have incentives to provide protection for co-ethnics or co-nationals, but less for others; and so forth. The expectation of ethnic preference and discrimination works the other way too. Voters prefer parties led by co-ethnics or co-nationals, delinquents hope for co-ethnic or co-national judges, and citizens prefer to be policed by co-ethnics or co-nationals.

Not all modern nation-states are characterized by such ethnic and national favoritism, however. As we discuss elsewhere, this favoritism is more likely in poor states that lack the resources for universal inclusion, as well as in states with weak civil society institutions where other, nonethnic channels for aggregating political interests and rewarding political loyalty are scarce (Wimmer 2002). In such states, political leaders and followers orient their strategies toward avoiding dominance by ethnic or national others—they strive for the self-determination and self-rule that are at the core of nationalist ideology. 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 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 or national group. The aggregate consequence of these strategic orientations is a struggle over control of the state between ethnically defined actors—or ethnic politics for short (Esman 1994; Rothschild 1981).

Such ethnic politics may lead to a process of political mobilization, counter-mobilization, and escalation. Political leaders appeal to the ideal of self-rule and fair representation enshrined in the nation-state model to mobilize their followers against the threat of ethnic dominance by others. These demands may stir the fear of ethnic dominance among other political elites and their ethnic constituencies and result in a process of counter-mobilization. The conflicting demands may finally spiral into armed confrontation. Our theory does not explicitly address the logic of this escalation process (see Olzak 2006; Tarrow and Tilly 2006) but seeks to specify the ethnopolitical configurations that make it more likely.

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Ethnopolitical Configurations of Power and Types of Ethnic Conflict

To accomplish this task, we first introduce some conceptual tools to describe different configurations of actors and the power relations between them (see Figure 1). Borrowing from Tilly's (1978) polity model, we distinguish between various social groups that control or have access to the central government (the inner circle in grey), those who are excluded from government but are still citizens of the country (the next circle in white), and finally, the social world beyond the territorial boundaries of the state. Each ethnopolitical constellation of power is thus defined by three types of boundaries: (1) the *territorial boundaries* of a state that define which ethnic communities are considered a legitimate part of a state's citizenry, (2) the *boundary of inclusion* separating those who share government power from those who are not represented at the highest levels of government, and (3) the *division of power* and the number of ethnic cleavages among the included sections of the population.

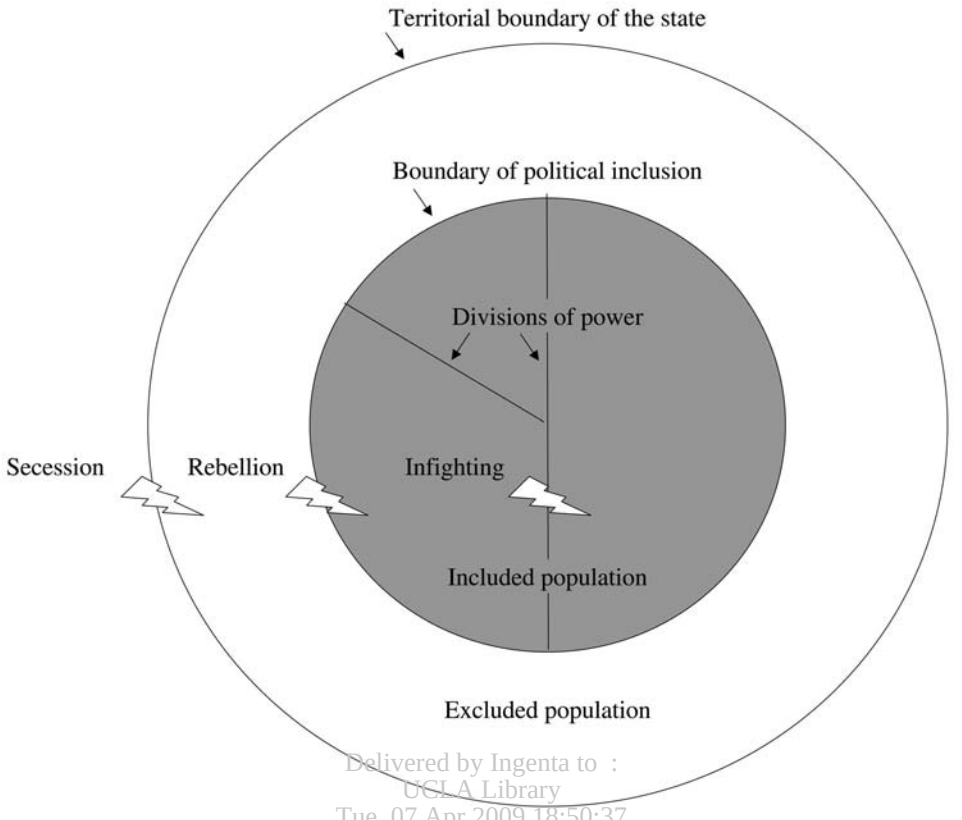

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Figure 1. Ethnopolitical Constellations of Power and Conflict

Each boundary can become the focus of ethnopolitical conflict: who is included or excluded from state power, how power is shared among ethnic elites and their constituencies, and which ethnic communities should be governed by a state. We can thus distinguish between three types of ethnic conflict, depending on which of these boundaries is at stake and which actors are challenging each other over its location. When excluded segments of a population fight to shift the boundaries of inclusion, we call these conflicts *rebellions.* When ethnic elites in power are pitted against each other in a struggle over the spoils of government, we speak of *infighting. Secession* aims at changing the territorial boundaries of a polity and can be pursued by both excluded and included groups.

War-Prone Configurations: Hypotheses

Following the logic of our configurational argument, we propose separate hypotheses for rebellions, infighting, and secession. First, *a high degree of ethnic exclusion will increase the likelihood of rebellion* (Hypothesis 1) because it decreases a state's political legitimacy. This makes it easier for political leaders to mobilize a following among their ethnic constituencies and challenge the government.[5] We expect that the most war-prone configurations are ethnocracies, that is, the rule of an elite with a small ethnic constituency (e.g., the Tutsi in Burundi, white settlers in Rhodesia, and Sunni rule under Saddam Hussein).

Second, we assume that infighting is more likely to occur when many partners share government power, that is, in states characterized by a segmented center. The greater the number of political partners, the more likely alliances will shift, increasing the fear of losing out in the

[5] For additional specifications of the mechanisms leading to successful ethnic mobilization, see Hechter and Levi (1979), Gurr (1993b), and Wimmer (1997).

ongoing struggle over the distribution of government spoils.[6] In such configurations, an elite faction is more likely to mobilize its ethnic followers and challenge its power sharing partners by demanding a bigger share of the government cake. In states with only one ethnically defined elite in power, such ethnic infighting is logically impossible. Thus, *the greater the number of power sharing elites, the greater the likelihood of violent infighting* (Hypothesis 2). We expect countries characterized by a high degree of center segmentation, such as Lebanon and India, to be particularly conflict-prone.

Third, we hypothesize *that states with a long history of indirect rule are more likely to see secessionist conflicts* (Hypothesis 3). In such states, large segments of the population are not accustomed to being governed directly by the political center. These groups can be more easily mobilized for a secessionist project with the argument that only independence will avoid the danger or reality of alien rule (Hechter 2003). An example is Bosnia, which spent the nineteenth and most of the twentieth century under Ottoman, Habsburg, and later Yugoslavian rule. Fourth, we postulate that *secession is more likely in large states* (Hypothesis 4). Large states are less likely to have penetrated the outer reaches of their territory in the past, and thus the population is less accustomed to direct rule. Imperial past and population size are both measurements of *state cohesion,* that is, the degree to which the population takes a state's territorial borders for granted and identifies with a state independent of who controls its government. An earlier literature in political anthropology refers to this aspect of an ethnopolitical configuration as "institutional pluralism."[7] Secessionist groups claiming to represent power sharing partners or excluded populations are more likely to challenge states that lack coherence. Low state cohesion thus reinforces the dynamics of exclusion and segmentation and leads challengers to secessionist paths.

Additional factors may halt the spiral of mobilization, counter-mobilization, contestation, and escalation and instead lead to a path of accommodation and de-escalation. First, rich states' governments can better accommodate protest movements through redistribution policies and by co-opting the movements' leadership into the power elite, such as in the aftermath of the civil rights movement in the United States. The same holds true for dissatisfied members of a power sharing arrangement: new government institutions can be created and staffed with their followers, and new infrastructure projects can be directed toward their ethnic constituency. Both rebellions and infighting, therefore, *should be less likely the greater a state's level of development* (Hypothesis 5). Our model incorporates one of the most robust findings in the civil war literature (Hegre and Sambanis 2006)—that civil wars happen in poor countries—and gives it a new interpretation in line with theories of contestation and violence (see Tarrow and Tilly 2006:145).

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Second, the likelihood that a particular actor will instigate conflict depends on the entire power configuration, not just on that actor's position within that configuration. More specifically, we expect that power sharing partners are less likely to fight each other when there is a high risk of rebellion by the excluded population. We assume that *the likelihood of infighting decreases as the degree of exclusion increases* (Hypothesis 6) *and as states become larger (and thus more incoherent)* (Hypothesis 7). Our configurational theory posits that exclusion and cohesion will have opposite effects on different types of ethnic conflict. Ethnocracies

[6] Horowitz (1985) offers many insights into the mechanisms through which such elite competition escalates into violent conflict, including mutual outbidding of ethnic parties, the holding of a close election that resembles an ethnic census (see also Wilkinson 2004), and the logic of military coups and counter-coups.

[7] Existing typologies are also based on exclusion, elite segmentation, and state cohesion as main aspects of ethnopolitical configurations of power. Hechter and Levi (1979), Horowitz (1985), Lustick (1979), and Wimmer (2002) distinguish highly exclusionary states and those with high levels of elite segmentation. Anthropologists working in "complex societies" have analyzed different degrees of institutional pluralism (Despres 1968; Simpson 1995; Smith 1969), referring to the cohesion dimension. Cohen (1978) combines cohesion and exclusion, while Schermerhorn (1970) combines segmentation and exclusion. Young (1976) and Rothschild (1981) offer the most comprehensive typologies building on all three aspects.

will have more rebellions (Hypothesis 1) but less infighting among the included population (Hypothesis 6); incoherent states will have more secessions (Hypothesis 4) but less infighting (Hypothesis 7). Only a disaggregated research design distinguishing between different types of ethnic conflicts can test these hypotheses.

Relation to Existing Theoretical Traditions and Empirical Findings

Our configurational theory incorporates and reconciles two sets of theoretical propositions that are usually seen as mutually exclusive. First, much debate centers on whether exclusion and segregation (the "internal colonialism" model of Hechter [1975]) or competition and increased contact (Horowitz 1985; Olzak and Nagel 1986) are more conflict-prone. Our theory maintains that both hierarchical exclusion and vertical competition are relevant mechanisms that link ethnic politics to violence, but they affect different types of actors, as defined by actors' positions in the ethnopolitical power configuration. Our theory also specifies what competition and exclusion are about: they are not primarily about individual goods such as housing or jobs (as maintained by competition theory), nor more generally the fruits of modernization (as argued in Horowitz 1985). Rather, competition and exclusion concern control over the state and the public goods and services at its disposal.

Our approach also avoids the popular distinction between "greed" and "grievance" theories of civil war (introduced by Collier and Hoeffler 2004). While the alliteration is certainly seductive, and the dichotomy resonates well with Western traditions of opposing the material to the ideal, it makes little empirical sense. As argued above, ethnic politics simultaneously concerns material interests, such as access to government controlled jobs, services, and contracts; idealist motives, such as the recognition of one's ethnic heritage by the state; and genuine political goals, such as access to state power. Because political domination by ethnic others also affects one's economic, legal, and symbolic standing, it is pointless to try to disentangle these intertwined and mutually reinforcing motives (see Tarrow and Tilly 2006). The crucial question is not whether rebels are coolly calculating materialists or hot-blooded idealists fighting for a cause, but rather what causal dynamics lead actors with complexly intertwined motives down the path toward conflict.

Our institutionalist theory of ethnic configurations and conflict builds on previous empirical research while extending it in new directions. To date, no scholar has proposed or tested hypotheses regarding center segmentation—that is, how the number of power sharing elites influences infighting. In line with our hypothesis that low state cohesion is related to secession, quantitative research based on the MAR data set (Gurr 1993b; Walter 2006) shows that previous political autonomy predicts the likelihood of secession at the group level. Similarly, on the basis of a new data set, Roeder (2007) demonstrates that previous provincial autonomy greatly increases the likelihood of nationalist mobilization.[8] Buhaug (2006) shows that population size affects secessionist conflicts only, but he offers a different explanation for this finding.

Quantitative tests of the exclusion hypothesis (Hypothesis1) produce more conflicting results. Gurr (1993b:179) uses his Minorities at Risk data to demonstrate that political disadvantage increases the likelihood of armed rebellion, while political discrimination decreases it. Using data covering all countries from 1945 to 2001, Fearon and Laitin (2003:85) find that a lack of minority language rights and a constitutional preference for certain religious groups do not increase the likelihood of high-intensity civil war. Wimmer and Min (2006) also use a global data set and aggregate country-level MAR data; they demonstrate that countries with more politically discriminated groups are more likely to have civil wars. Olzak (2006:124) also aggregates MAR data to the country level for a subset of 55 countries from 1965 to 1989. She arrives at the conflicting conclusion that both ethnic discrimination *and* the granting of ethnic group rights are associ-

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[8] Two other factors that we do not incorporate into our theory are known to increase the likelihood of secession: kin groups across the border (Gurr 1993b; Saideman and Ayres 2000; see also Davis and Moore 1997; but see Walter 2006) and geographic concentration and peripheral location (Buhaug, Cederman, and Rød 2008; Saideman and Ayres 2000; Toft 2003; Walter 2006).

ated with higher intensity of ethnic rebellion. Cederman and Girardin (2007) made a first attempt to code ethnic groups' access to state power in the countries of Eurasia and found evidence that exclusion breeds conflicts. Some have contested this finding (Fearon, Kasara, and Laitin 2007),[9] but more recently, Buhaug and colleagues (2008) confirmed the initial results using Eurasian data that include additional geo-coded variables.

Existing tests of the exclusion argument are thus rather inconclusive.[10] We argue that this is because of measurement problems and data limitations. Most researchers define exclusion narrowly, focusing on a small number of minority rights rather than explicitly measuring access to state power. The corresponding data thus do not capture ethnic power relations in a broader, nonlegalistic way and depend too much on the dominant majority versus discriminated minority scheme of the MAR data set. Data sets that use a broader definition of exclusion are limited in geographic scope and purely cross-sectional and therefore do not record changes in ethnic power relations over time. There is thus ample room to improve on the existing research to test the exclusion argument in a more adequate and comprehensive way. This is the aim of the new data set we have assembled.

THE ETHNIC POWER RELATIONS (EPR) DATA SET, 1946 TO 2005

The Ethnic Power Relations (EPR) data set identifies all politically relevant ethnic categories around the world and measures access to executive-level state power for members of these ethnic categories in all years from 1946 to 2005. For the sake of brevity, we introduce only the major aspects of the data set here and refer readers to the Online Supplement on the *ASR* Web site (http://www2.asanet.org/journals/asr/2009/toc068.html) for more details about coding procedures and rules. The data set contains two parts. The first is a country-year data set that codes all politically relevant ethnic groups and their degree of access to central state power.[11] The second is a conflict data set, based on the widely used PRIO/Uppsala Armed Conflict Data Set that includes all armed conflicts with more than 25 battle deaths. We extend the data set with new codings of whether rebels pursued ethnic or nonethnic goals, as well as whether they aimed at secession. We then link conflicts to politically relevant ethnic groups if rebels claimed to fight in the name of a particular ethnic community.

Politically Relevant Ethnic Groups and Access to Power

Following the constructivist, Weberian tradition, we define ethnicity as a subjectively experienced sense of commonality based on a belief in common ancestry and shared culture. This definition includes ethnolinguistic, ethnosomatic (or "racial"), and ethnoreligious groups, but not tribes and clans that conceive of ancestry in genealogical terms, nor regions that do not define commonality on the basis of shared ancestry. Ethnic categories may be hierarchically nested and comprise several levels of differentiation, not all of which are politically relevant at a particular time. (On the notion of ethnicity underlying this project, see Wimmer 2008.)

An ethnic category is politically relevant if at least one significant political actor claims to represent the interests of that group in the national political arena, or if members of an ethnic category are systematically and intentionally discriminated against in the domain of public politics. We do not distinguish between degrees of representativity of political actors who claim to speak for an ethnic group, nor do we code the heterogeneity of political positions voiced by leaders claiming to represent the same community (Brubaker 2004). The coding scheme allows us to identify countries or specific peri-

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[9] Fearon and colleagues (2007) propose an alternative measurement strategy that records the ethnic background of each country's head of state. This does not capture broader, institutionalized structures of inequality, however, and necessitates ad hoc changes in the data to avoid misleading codings (e.g., Georgian dominance of the Soviet Union under Stalin or Quebecois hegemony in Canada under Trudeau).

[10] Others have tested an exclusion argument for secessionist minority rebellion only, using the MAR data set, and arrived at contradicting results as well (Saideman and Ayres 2000; Walter 2006).

[11] The data set includes all 155 sovereign states with a population of at least 1 million and a surface area of at least 500,000 square kilometers as of 2005.

ods in which political objectives, alliances, or disputes were never framed in ethnic terms, thus avoiding using an ethnic lens for countries not characterized by ethnic politics, such as Tanzania and Korea.

Because politically relevant categories and access to political power may change over time, coders divided the 1946 to 2005 period and provided separate codings for each subperiod. This was also necessary when the list of politically relevant categories changed from one year to the next (either because certain categories ceased to be or became relevant for the first time, or because higher or lower levels of ethnic differentiation became salient). Next, we coded the degree of access to power enjoyed by political leaders who claimed to represent various groups.

We focus only on executive-level power, that is, representation in the presidency, cabinet, and senior posts in the administration, including the army. The weight given to these institutions depends on their de facto power in a given country. In all cases, coders focused on absolute access to power irrespective of the question of under- or overrepresentation relative to the demographic size of an ethnic category.

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We categorized all politically relevant ethnic groups according to the degree of access to central state power by those who claimed to represent them. Some held full control of the executive branch with no meaningful participation by members of any other group, some shared power with members of other groups, and some were excluded altogether from decision-making authority. Within each of these three categories, coders differentiated between further subtypes, choosing from monopoly power, dominance, senior or junior partner in a power sharing arrangement, regional autonomy, powerless, and discriminated (see the *ASR* Online Supplement for details of the coding scheme). For the present analysis, we distinguish only between power-holding groups (whatever their share of power) and the excluded population (for a disaggregated analysis on the group level, using the full array of power categories, see Cederman, Wimmer, and Min [2009]).

War Coding

The conflict data set created for this project is based on the widely used Uppsala/PRIO Armed Conflicts Data Set (ACD) (Gleditsch et al. 2002). ACD defines armed conflict 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 people. Massacres and genocides are not included because the victims are neither organized nor armed; communal riots and pogroms are excluded because the government is not directly involved.

To date, the ACD has been of limited use for ethnic conflict analysis because it does not contain information on whether a conflict should be classified as ethnic. To overcome this limitation, we conducted new research and coded each conflict for whether rebel organizations pursued ethnonationalist aims and recruited along ethnic lines. We also coded whether rebels aimed at establishing a new independent state.

We distinguish between ethnic and nonethnic conflicts using the aims of the armed organization and their recruitment and alliance structures (this is in line with other ongoing coding projects, e.g., Sambanis 2009). We identify as "ethnic" the aims of achieving ethnonational self-determination, a more favorable ethnic balance-of-power in government, ethnoregional autonomy, the end of ethnic and racial discrimination, language and other cultural rights, and so forth. In ethnic wars, armed organizations also recruit fighters predominantly among their leaders' ethnic group and forge alliances on the basis of ethnic similarity.

We looked at the aims and recruitment patterns of each armed organization involved in a conflict separately. In some complex cases (e.g., Afghanistan, Burma, Chad, Uganda, Angola, and Zaire), we disaggregated a conflict into subconflicts when the nongovernmental side made different ethnic claims and rebel organizations acted independent from each other. Our data set thus contains a higher number of conflicts than the original ACD data (see the *ASR* Online Supplement for details).

We then linked all ethnic conflicts to the politically relevant ethnic category in the EPR data set. To avoid endogeneity problems, we made sure that the coding of ethnic power relations reflects the power constellation *before* the outbreak of conflict in cases where political changes occurred in the same year as a conflict. To test our configurational theory of ethnic conflict, we then divided ethnic conflicts into

Table 1. The Conflict Data Set

	Ethnic Conflicts			
	Infighting	Rebellions	Nonethnic Conflicts	Total
Secessionist	9	48	3	60
Nonsecessionist	11	42	102	155
Total Infighting/Rebellions	20	90		
Total	110		105	215

those fought in the name of ethnic groups excluded from central government power (rebellions) and those fought in the name of power holders (infighting). We further subdivided rebellions and infighting depending on whether they aimed to establish a separate, independent state or join another existing state. This produced a fourfold typology with separatist rebellions, nonseparatist rebellions, separatist infightings, and nonseparatist infightings

Our data set includes 215 armed conflicts fought between 1946 and 2005, 110 of which were ethnic conflicts. Of the 215 conflicts, 60 had secessionist aims, the vast majority of which were also ethnic in character. Among the 110 ethnic conflicts, 20 were fought by groups in power and 90 by excluded groups (see Table 1). One half of the conflicts reached the standard threshold of civil war (more than 1,000 battle deaths in a year).

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VARIABLES AND DATA SOURCES

Exclusion, Center Segmentation, State Cohesion

To test Hypothesis 1, we compute the share of the excluded population in the total population that is ethnopolitically relevant. We call this the *share of the excluded population* for short. We assume that increases in the share of the excluded population have a greater effect on the likelihood of conflict at lower levels of exclusion than at higher levels, and we therefore use a logged transformation of this variable.[12] We measure the degree of center segmentation (which according to Hypothesis 2 is associated with higher conflict probability) by counting the number of power sharing groups represented by ethnic elites. The *number of power sharing partners* ranges from 1 to 14 (in India). Following Hypothesis 3, the cohesion of a state decreases the longer the pre-independence history of indirect rule in an empire and the larger the size of the population. We rely on a measure of a state's *past imperial history* that calculates the percentage of years spent under imperial rule between 1816 and independence (Wimmer and Min 2006). We count as imperial rule all years during which a territory was a colonial or imperial dependency (including of the Soviet Union and other communist empires) or the heartland of a landbased empire (e.g., Turkey under the Ottomans or Austria under the Habsburgs, but not the "mother country" of an empire with seaborne colonies, like Portugal).

Other Variables

We control for other robustly significant variables in civil war research, especially those identified in Hegre and Sambanis's (2006) meta-analysis. We include *linguistic fractionalization* (as found in Fearon and Laitin's data set) to show its limited significance once ethnic politics variables are included. *GDP per capita*[13] and a state's *population size* also play impor-

[12] We hypothesize that the initial break with the ethnonational principles of legitimacy of modern nation-states carries more political risk than does the shift to an even more exclusionary ethnocracy.

[13] Our GDP per capita data are in constant 2000 US Dollars. Data for 5,737 observations (79 percent) come from Penn World Table 6.2. Using growth rates from the World Bank's World Development Indicators provided 229 more observations (3 percent). Using Fearon and Laitin's data, we calculated annual growth rates and extended our values back to 1946. Total data coverage is 7,105 observations (99.6 percent).

tant roles in our theory of ethnic politics (according to Hypotheses 4 and 5).

Democratic civil peace theory states that democracies are better able than other political regimes to solve internal disputes. Autocracies, on the other hand, can suppress rebellions by using force or threatening mass violence. Civil wars should therefore be less likely in strongly democratic and strongly autocratic societies (Ellingsen 2000; Hegre et al. 2001; Mansfield and Snyder 2005; Müller and Weede 1990).[14] We use Polity IV data and the widely adopted cutoffs of +6 and –6 to identify democracies, autocracies, and anocracies (states that are neither democracies nor autocracies).

Fearon and Laitin's (2003) insurgency model maintains that wars break out when government forces are weak and when mountainous terrain allows rebels to hide and retreat. We include measures of *mountainous terrain* and *previous regime change* (which should weaken the government vis-à-vis the rebels) to evaluate their main argument. We adopt the mountainous terrain data from their data set; we define regime change as any change in the Polity score of 3 points or more over the prior three years.

Ross (2003) developed a theory of how the availability of natural resources affects different types of conflict. He expects that when rebels can obstruct the extraction of natural resources, as with oil, the likelihood of secessionist movements increases (see also Collier and Hoeffler 2004). Buhaug (2006), on the other hand, argues that oil matters in conflicts over an existing state because oil resources are usually controlled by the central government. This increases the incentives to capture a state, rather than to secede from it. To measure the impact of oil, we generate an *oil production per capita* variable based on data from Wimmer and Min (2006).

MODELS AND FINDINGS

Our data set includes 7,155 country-year observations covering 155 sovereign states in all years after independence from 1946 to 2005. We use the standard modeling approach in the literature on civil war, regressing a range of independent variables on a binary dependent variable coded as 1 in the first year of an armed conflict and 0 otherwise. We create a civil conflict onset variable that includes both ethnic and nonethnic onsets, as well as a more narrow ethnic conflict onset variable. For the ethnic conflict onset variable, we disaggregate further to distinguish between the political status of the groups instigating the conflict (excluded or power sharers) and the aims of these parties (secession or other aims).

We test our models against two versions of these dependent variables, both common in the literature. The first version includes all observations, including those in which another war was already ongoing, and adds a dummy control for such ongoing war. The second version drops ongoing war years by coding them as missing, thereby omitting additional wars that begin while a first conflict is ongoing. This coding of the dependent variable results in approximately 15 percent fewer observations. In this article, we present results using the first version (for models based on the second version, see the supplement on the first author's homepage: http://www.sscnet.ucla.edu/soc/faculty/wimmer/AppendixEthnicPolitics.pdf). The results of the two models are almost identical.

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We control for possible time trends by including the number of peace years since the outbreak of a war, as well as a cubic spline function on peace years following Beck, Katz, and Tucker (1998). We also add a calendar year variable to capture possible changes in the geopolitical climate over time. For the sake of space, we do not show the time control variables in the following tables (see supplement on first author's homepage). As a robustness check, we tested our models with regional controls and without time controls and found no large differences in our main findings (again, see supplement on first author's homepage). Throughout, we specify robust standard errors clustered by country to account for the nonindependence of observations from the same state. Because armed conflict is a rare event, we also ran our models using the "rare events" logit estimator and found no substantive differences to our main findings (see supplement on first author's homepage).

Our analysis proceeds in four steps, each leading to a more fine-grained, disaggregated analysis of conflict onset. First, we determine

[14] Sambanis (2001) and Reynal-Querol (2002) confirm this hypothesis for ethnic wars only.

whether ethnic politics matters at all in predicting the onset of armed civil conflicts. Second, we focus only on ethnic conflicts while maintaining our global purview and keeping all country-years in the analysis. Third, we evaluate whether exclusion and segmentation predict rebellions and infighting, respectively. Finally, we disaggregate further to determine how state cohesion affects both rebellion and infighting and drives them toward secessionist goals.

Explaining Armed Conflict

We first test whether ethnic politics matters for understanding conflict and peace (Hypotheses 1, 2, and 3). To ensure that our results do not depend on our coding of civil conflicts, we also run our model on high-intensity wars only, as well as against war codings from the well-known civil war data sets assembled by Fearon and Laitin (2003) and Sambanis (2004).

Table 2 shows that ethnic politics is an important part of the puzzle in explaining civil wars. The results challenge greed-and-opportunity theories of civil war, according to which ethnicity is unrelated to conflict. The table also demonstrates that once ethnic politics is measured directly, the ethnic diversity index loses significance—contrary to what the diversity-breeds-conflict school assumes. Rather than diversity as such, it is political exclusion along ethnic lines that breeds ethnic conflict.

The share of the excluded population, the central variable in our configurational model of ethnic conflict, is significant for all model specifications: when using Fearon and Laitin's or Sambanis's coding of dependent variables (i.e., excluding low-intensity wars); when dropping all ongoing war years from the sample or leaving them in; and with or without additional control variables. Ethnic exclusion is as consistently related to conflict as is GDP per capita, one of the most robust explanatory factors in the study of civil wars (Hegre and Sambanis 2006).

In contrast, the number of power sharing partners (Hypothesis 2) does not have a robust impact on civil war onset. This is not surprising, given that only 20 of the 200 conflicts in this analysis were initiated by actors representing ethnic groups in power. Moreover, high degrees of exclusion should have a mitigating effect on the likelihood of infighting (Hypothesis 6), so we expect to see the effects of center segmentation only when disaggregating the dependent variable. The imperial past variable is positive but insignificant (Hypothesis 3). We will demonstrate further that the lack of state coherence substantially increases the likelihood of ethnic secessionist conflicts.[15]

How do other theories of civil war fare in our test? Regime change and mountainous terrain play a key role in the insurgency model but receive rather limited support here, although the mountainous variable helps explain one coding of high-intensity civil wars (Model 4) and one version of the ACD conflict coding (Model 2).[16] Oil production per capita is associated with resource competition theories and receives mixed support (Models 2 and 5). Meanwhile, the findings for democratic civil-peace theory are more robust: anocracy increases the risk of conflict in all models except those run on the high-intensity ACD wars.

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Explaining Ethnic Conflict

This is the first time that the ethnic exclusion argument has been statistically confirmed based on a global data set that measures degrees of exclusion directly and at the polity level, rather than the group level. The robustness of this finding is remarkable, given that we regress on all civil conflicts in the data set. Our model of ethnic politics makes no claims to explain nonethnic wars, such as the civil war in Korea

[15] Among a large number of robustness checks (available in the supplement on the first author's homepage), we controlled for endogeneity (the possibility that past conflict determines future conflict) by running models that include a variable for the number of past conflicts. This did not affect our results (see Table 4d in the supplement on the first author's homepage).

[16] Sambanis (2004) and Collier and Hoeffler (2004) also find no support for the mountainous variable—but it appears in Hegre and Sambanis's (2006) list of the "25 most robust variables," as does political instability. We also experimented with Fearon and Laitin's "new state" variable (results not shown), but we found it extremely sensitive to alternative codings (e.g., three instead of two years of independence).

Table 2. Ethnic Politics and Armed Conflict (logistic regression)

	DV: Onset of Conflicts					DV: Onset of Ethnic Conflict			
Model No.:	1	2	3	4	5	6	7	8	9
Conflict Dataset:	ACD	ACD	ACD High Intensity	Fearon and Laitin	Sambanis	ACD	ACD	ACD High Intensity	Fearon and Laitin
Ethnic Politics Variables									
Excluded population	.1887**	.1291*	.2859**	.2564**	.2792**	.4192**	.3191**	.5347**	.3667**
	(.0513)	(.0558)	(.0834)	(.0779)	(.0808)	(.0862)	(.0875)	(.1351)	(.1214)
Center segmentation	.0862**	.0587	.0562	.0771	.0177	.1554**	.1120**	.1272*	.0969
	(.0295)	(.0389)	(.0455)	(.0586)	(.0491)	(.0312)	(.037)	(.053)	(.0747)
Imperial past	.2075	.4579	.7285	.7899*	.5932	.6401	.9301*	1.1793	1.5761**
	(.2614)	(.2886)	(.4441)	(.3568)	(.3307)	(.4477)	(.4426)	(.6304)	(.4244)
Other Variables									
Linguistic fractionalization		.6298	.1244	–.0283	.0261		1.2800**	.2563	.599
		(.3227)	(.4597)	(.4274)	(.3989)		(.3997)	(.4974)	(.6156)
GDP per capita	–.1239**	–.1093**	–.1902**	–.1267**	–.1750**	–.1446**	–.1256**	–.1921**	–.1554**
	(.0271)	(.0276)	(.0546)	(.0374)	(.0472)	(.0415)	(.0448)	(.0746)	(.0585)
Population size	.1556**	.1397**	.0865	.2354**	.2135**	.2171**	.2102**	.1884*	.3609**
	(.0559)	(.0532)	(.0636)	(.0672)	(.0616)	(.0714)	(.0656)	(.0757)	(.0894)
Mountainous terrain		.1241*	.1901	.1581*	.1320		.1749	.3258*	.0701
		(.0601)	(.1117)	(.0794)	(.0765)		(.0984)	(.1483)	(.109)
Political instability		.3454	.4555	.2693	.2655		.1544	.2979	–.0441
		(.1764)	(.2852)	(.2754)	(.2412)		(.2726)	(.3958)	(.3549)
Anocracy		.4292**	.4014	.7218**	.6478**		.4469*	.5681	.9738**
		(.1625)	(.2511)	(.2369)	(.1863)		(.2263)	(.2929)	(.2614)
Oil production per capita		.0171**	.0051	.0056	.0176*		.0180*	.0277**	.0064
		(.0063)	(.0162)	(.0165)	(.0078)		(.0091)	(.0083)	(.0284)
Ongoing war	–.9832**	–.9678**	–1.2732**	–2.1655**	–1.4045**	.0359	–.0697	–.7636	–2.2861**
	(.3620)	(.3733)	(.4690)	(.4277)	(.4435)	(.6123)	(.6166)	(.6271)	(.5551)
Constant	–16.1544	–14.2810	–9.0225	–49.4749**	–46.1852**	–37.1296*	–32.6393*	–26.7855	–80.7809**
	(10.2646)	(10.5160)	(13.5435)	(14.6798)	(14.8129)	(14.6291)	(15.1798)	(17.9573)	(17.4279)
N Observations	6,938	6,865	6,865	6,034	5,818	6,938	6,865	6,865	6,034
N Conflict Onsets	200	197	82	97	121	103	102	50	66

Note: Time controls not shown; robust standard errors in parentheses.
* $p < .05$; ** $p < .01$.

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or army coups in Brazil. Because half of the conflicts in our data set are not fought in the name of ethnic groups, a more focused investigation needs to exclude nonethnic conflicts, as we do in Models 6 to 9 in Table 2. We thus follow Sambanis (2001) who shows that, because different factors cause ethnic and nonethnic civil wars, they should be analyzed separately (but see Fearon and Laitin 2003).

Once we focus on ethnic conflicts only, the other two ethnic politics variables become statistically significant. The number of power sharing groups is significant in all models except in regressions on Fearon and Laitin's coding of high-intensity wars. The imperial past variable, which measures the degree of state cohesion and should predict secessionist conflicts only, reaches significance in some models (we revisit this result further below).

Exclusion, segmentation, and incohesion are also substantively important for the dynamics of war and peace. Increasing the share of the excluded population from 6 to 32 percent (an increase of one standard deviation from the mean) results in a 25 percent increase in the probability of ethnic conflict (calculated on the basis of Model 7). A one standard deviation increase in center segmentation leads to a 9 percent increased risk of conflict, while a similar increase in years under imperial rule increases the chance of armed conflict by 13 percent. A one standard deviation increase in GDP per capita and population size, the two most robust variables in the civil war literature, influence the probability of war by 22 and 13 percent, respectively.[17]

The strength and robustness[18] of the exclusion, segmentation, and cohesion variables are remarkable because the dependent variable here does not distinguish between different types of ethnic conflict. Our theory assumes, however, that infighting, rebellion, and secession are caused by different ethnopolitical configurations and that the same variable could therefore have *opposite* effects on the likelihood of different types of conflict (see Hypotheses 1, 4, 6, and 7). To test this, we disaggregate the dependent variable further and use multinomial logit regressions to predict the onset of different types of ethnic conflicts.

Explaining Rebellion and Infighting

We first distinguish between ethnic conflicts fought in the name of excluded groups (rebellions) and those begun by power sharing partners (infighting). We expect that the two principal aspects of ethnic politics affect rebellions and infighting differently. As the number of power sharing elites increases and their alliances therefore become more unstable, their likelihood of fighting wars against each other should also increase (Hypothesis 2). Center segmentation should have no effect, however, on rebellions by leaders who claim to represent excluded groups. The size of the excluded population should have opposite effects on included and excluded groups: it should increase the likelihood of rebellion (Hypothesis 1) and therefore provide a disincentive for infighting (Hypothesis 6).

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Table 3 shows that the greater the number of groups that share power, the greater the likelihood that they will fight each other on the battlefield. Infighting is also influenced, and again negatively, by population size (Hypothesis 7). The larger (and thus more incoherent) a state's population, the less likely elites can afford to fight each other to increase their share of power. Contrary to our expectations, infighting is not significantly less likely when large segments of the population are excluded from power nor in richer countries (inconsistent with Hypotheses 6 and 5, respectively), although the signs of the coefficients point in the expected direction.

The size of the excluded population does influence rebellions by excluded groups (Hypothesis 1). Rebellions are less likely in rich countries (Hypothesis 5) where governments can afford to redistribute state resources or co-opt the leaders of protest movements. Hypothesis 5 therefore receives mixed support. More populous and linguistically heterogeneous states are more likely to see rebellions (a finding that is mostly driven by secessionist wars, as we will see in the next section). State coherence (measured through the imperial past variable) does not consistently predict rebellions

[17] See the table of first difference, Table S3, in the *ASR* Online Supplement.

[18] For a series of robustness checks, see Tables 5 in the supplement on the first author's homepage.

Table 3. Ethnic Conflicts by Actor Type (multinomial logistic regression)

	Model 1		Model 2	
Type of ethnic conflict:	Infighting by Power Holders	Rebellion by Excluded	Infighting by Power Holders	Rebellion by Excluded
Ethnic Politics Variables				
Excluded population	–.0379	.5212**	–.3146	.5146**
	(.1659)	(.0808)	(.1802)	(.0848)
Center segmentation	.3583**	.0468	.3285**	.0648
	(.0568)	(.0387)	(.0684)	(.0433)
Imperial past	2.8363	.4000	3.7934*	.4520
	(1.5424)	(.4405)	(1.8819)	(.4836)
Other Variables				
Linguistic fractionalization	–.8215	1.5463**	1.1132	1.4589**
	(1.1411)	(.4868)	(1.1328)	(.4450)
GDP per capita	–.2628	–.0921*	–.2148	–.0967*
	(.1493)	(.0391)	(.1248)	(.0437)
Population size	–.2531*	.3832**	–.4172**	.3818**
	(.1184)	(.0765)	(.1517)	(.0826)
Mountainous terrain			.6026**	.0767
			(.2179)	(.1189)
Political instability			.1255	.1751
			(.6731)	(.3150)
Anocracy			.4277	.4566
			(.4815)	(.2374)
Oil production per capita			.0198	.0196
			(.0113)	(.0116)
Ongoing war	.5618	.0881	.2301	–.1307
	(1.2172)	(.6913)	(1.1056)	(.6827)
Constant	–93.2683*	–23.6030	–88.1487*	–26.0182
	(37.7776)	(15.9535)	(42.1082)	(16.3912)
N Observations	6,935	6,935	6,865	6,865
N Conflict Onsets	20	83	19	83

Note: Time controls not shown; robust standard errors in parentheses.
* $p < .05$; ** $p < .01$.

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or infighting (a result that we also revisit below).[19]

Among the control variables introduced in Model 2, anocracy is no longer significant. Mountainous terrain seems to matter when groups in power fight each other, but not in countries where rebels try to overthrow the government (as the insurgency model would predict). Oil resources do not seem to entice either included or excluded groups to fight.

Explaining Secessionist and Nonsecessionist Conflicts by Rebels and Infighters

We now further differentiate between secessionist and nonsecessionist wars. Combining actor types with war aims generates four kinds of ethnic conflict: secessionist wars fought in the name of excluded groups (secessionist rebellions for short), nonsecessionist rebellions, secessionist conflict started by power sharing groups (secessionist infighting for short), and nonsecessionist infighting. We run multinomial logit regressions using these four types of ethnic conflict as possible outcomes.

The results in Table 4 support our expectations. Exclusion and center segmentation have the same effects on the likelihood of rebellions and infighting as before, and they also predict

[19] Dropping the time controls, including the variable for the number of past conflicts, or running the models with additional region controls produce almost no changes to these results (see Tables 6 in the supplement on the first author's homepage).

Table 4. Ethnic Conflict, by Actor and Aim (multinomial logistic regression)

War Type:	Secession by Power Holders	Secession by Excluded	Infighting by Power Holders	Rebellion by Excluded
Ethnic Politics Variables				
Excluded population	–.2032	.2554*	–.4504	.7501**
	(.3306)	(.1109)	(.3156)	(.1277)
Center segmentation	.4956**	.0008	.3176**	.0689
	(.1164)	(.0417)	(.0960)	(.1001)
Imperial past	14.6269**	1.9524*	1.1870	–.8041
	(2.8503)	(.8152)	(1.6311)	(.7777)
Other Variables				
Linguistic fractionalization	1.4433	1.9997**	.9991	.9796
	(1.2707)	(.6431)	(1.6116)	(.8709)
GDP per capita	–.6017	–.0226	–.1914	–.1833*
	(.3302)	(.0584)	(.1750)	(.0814)
Population size	–.1882	.4835**	–.7321**	.2498
	(.1925)	(.1256)	(.1841)	(.1329)
Mountainous terrain	.6948	.3943	.5656*	–.0913
	(.3751)	(.2211)	(.2815)	(.1608)
Political instability	–35.2497**	.3655	1.0312	.0291
	(.6728)	(.5128)	(.7487)	(.4485)
Anocracy	1.4050	.2931	.0115	.6333
	(.9854)	(.3892)	(.7129)	(.3639)
Oil production per capita	–.3692	.0016	.0126	.0296**
	(.4031)	(.0452)	(.0088)	(.0085)
Ongoing war	2.6879	–.1664	–.5972	–.0502
	(2.9776)	(1.0923)	(1.7814)	(.9068)
Constant	–290.3441**	–15.6566	12.0956	–45.2199
	(41.4419)	(22.4369)	(68.2112)	(23.1803)
N Observations	6,865	6,865	6,865	6,865
N Conflict Onsets	9	41	10	42

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Note: Time controls not shown; robust standard errors in parentheses.
* $p < .05$; ** $p < .01$.

the onset of secessionist wars.[20] How does state cohesion affect conflict? Conforming to Hypothesis 3, having spent more years in imperial polities over the past two centuries increases the likelihood of secessionist conflict instigated by both power sharers and the leaders of excluded groups. It has no effect, again confirming our expectations, on nonsecessionist ethnic conflicts. The size of a state's population is also linked with secessions (Hypothesis 4). Both a long imperial past and a large population size suggest the presence of population segments accustomed to self-rule who are likely to resent the shift to direct rule brought about by a modern nation-state. As expected, population size is significant and positive for excluded populations only, and the sign of the coefficient is negative for power sharing partners (Hypothesis 7).[21]

[20] This result depends on using a logged version of the share of the excluded population. A nonlogged version, although it does not change any results of previous tables, fails to come close to standard significance levels in Models 2 and 6 in Table 4 (results not shown here).

[21] That population size is totally insignificant in regressions on the onset of nonethnic wars (results not shown) supports our interpretation of population size as a proxy for state coherence. This is contrary to the interpretation of Fearon and Laitin, who hypothesize that large populations are logistically and militarily more difficult for governments to control. Dropping the time controls or running the models with additional region controls produces some small changes to these results (for details, see Tables 7 in the supplement on the first author's homepage).

Our expectations regarding the effects of levels of economic development, however, are again not fully confirmed. Richer states' governments are able to avoid nonsecessionist rebellions because they can afford to co-opt the leadership of ethnic protest movements, but they do not experience less nonsecessionist infighting. That said, the frequency of violent infighting is rare (9 for secessionist and 10 for nonsecessionist cases). These results should therefore be interpreted with some caution.

Table 4 again includes linguistic fractionalization as a control variable. With a disaggregated measure of ethnic conflict as the dependent variable, we find that linguistic diversity is significant only in predicting secessionist rebellions (and only in models that include ongoing war years). We therefore suggest that linguistic fractionalization captures—in an indirect and rough way—an aspect of state coherence. It expresses the extent to which the central state has linguistically assimilated its population in past centuries; this provides an indicator of a state's capacity to extend its reach over a territory across a prolonged timeframe. Linguistic fractionalization should thus be linked with the consequences of low state cohesion, such as higher risk of secessionist conflict. Table 4 shows that once ethnic politics is measured in more adequate and direct ways, and we have reached the appropriate level of disaggregation, the effects of linguistic fractionalization are indeed very limited.

Among other control variables, anocracy and regime change again have no significant effects on any of the four types of conflict, while mountainous terrain is associated with infighting but not rebellion. Oil resources increase the likelihood of nonsecessionist wars fought by excluded groups. This is consistent with Buhaug's hypothesis that oil resources provide incentives to capture the state but not to secede from it.

Overall, the results of these tables demonstrate that a configurational approach to the study of civil wars yields important insights about the different mechanisms that generate violence and war. Measures of ethnic politics have heterogeneous effects on different types of ethnic conflict, as do other key variables such as population size and oil. Our configurational approach allows us to better understand why ethnic conflicts and wars might erupt in such different ethnopolitical constellations as seen in Bosnia, Northern Ireland, and Mexico.

Bosnian Serbs were part of a segmented power sharing arrangement within which elite competition for control over the newly founded state quickly escalated to incompatible positions and demands. The weak coherence of the former Yugoslav state, and the high degree of disidentification among all but the Bosniak segments of the population, further increased the likelihood of conflict and gave it a secessionist form. In Northern Ireland, however, the conflict erupted as a struggle over the political exclusion of the large Catholic population. Ireland was long ruled as an internal colony of Great Britain; the Northern parts of the island disidentified with the British state, increasing the likelihood that rebels would pursue secessionist aims. In 1994 in Mexico, *commandante* Marcos led a group of former peasant activists in a rebellion against the exclusion that the indigenous populations of Chiapas had suffered for centuries. In contrast to Northern Ireland and Bosnia, the Mexican state had time over the past two centuries to project its symbolic and political power over the population, who thus learned to see their membership in the state as self-evident and legitimate. The rebellion did not develop into a separatist endeavor, even though ample opportunities existed to unite with neighboring Guatemaltecan Mayas and their rebel organizations.

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CONCLUSIONS

This article identifies the conditions under which struggles over state power may lead to ethnic conflict. The likelihood of armed confrontation increases as the center of power becomes more ethnically segmented and as greater proportions of a state's population are excluded from power because of their ethnic background. These conflicts are even more likely, and more likely to take secessionist form, in incoherent states where the population is not accustomed to direct rule by the political center.

These results represent a major challenge to the greed-and-opportunity school, which discounts ethnicity as a relevant factor in explaining civil war. To be sure, our argument is not that ethnic identity or grievances, as opposed to interests and greed, motivate people to found and join armed organizations. Rather, ethnici-

ty may channel the pursuit of power and prestige along certain pathways such that the factions that struggle over state control will align along ethnic cleavages. Ethnicity is not an aim in itself, but the organizational means through which individuals struggle to gain access to state power. Our approach specifies the incentive structures under which this political logic of ethnic solidarity comes into play, as well as the conditions under which it leads to armed conflict.

Contrary to the assumptions of the diversity-breeds-conflict school, we show that ethnic conflicts are not any more likely in diverse countries: ethnodemographic diversity indices rarely achieve significance and do so only for a circumscribed subset of conflicts. Ethnodemographic indices, and many theories of conflict and peace that rely on them, bracket the crucial fact that the state is neither a neutral actor nor a passive arena within which ethnic actors operate. Rather, it is both the prize over which contending political actors struggle and a power instrument for those who control it.

These insights have important repercussions for the study of ethnic diversity in general. Recently, economists and political scientists have discovered the unwelcome consequences of "ethnic diversity" for a range of outcomes, including economic development, public goods provision, and levels of social capital and generalized trust. Our study shows that ethnic diversity indices lose much of their significance if we include variables that measure ethnic exclusion and competition. It is worth asking whether one would obtain similar results if our measurements of ethnic exclusion, center segmentation, and state coherence were used to study economic development, public goods provision, and social capital. In a new study of economic development, we show that this is indeed the case (Min, Cederman, and Wimmer 2009). This points to the possible conclusion that economic development, public goods provision, and conflict are endogenous to the ethnic power configurations analyzed in this article. These ethnopolitical configurations at the center of state power may shape the different trajectories of economic and political development in a much more profound way than hitherto acknowledged.

Our study also goes beyond the minority-mobilization model by showing that ethnic mobilization and conflict not only involve discriminated minorities fighting for their rights. Ethnic conflict often concerns the entire configuration of power, most importantly the question of who has access to state power and who controls which share of it. Our results lend themselves to a broader perspective that is not focused exclusively on demographic minorities at risk, but on the dynamics of ethnic politics at the center of the state. Contrary to the minority-mobilization model, challengers are most likely to find an armed following among excluded majorities, not minorities. In addition, groups in power instigate an important number of conflicts. The policy implications are obvious: when minorities rule, or many groups share power, granting rights to minorities will not prevent violence. Rather, nothing less than a fundamental rearrangement of the ethnopolitical configurations of power will secure durable peace.

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