

ETHNICITY, INSURGENCY, AND CIVIL WAR*

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Abstract

An influential conventional wisdom holds that civil wars proliferated rapidly with the end of the Cold War and that the root cause of many or most of these has been ethnic nationalism. We show that the current prevalence of internal war is mainly the result of a steady accumulation of protracted conflicts since the 50s and 60s rather than a sudden change associated with a new, post-Cold War international system. We also find that after controlling for per capita incomes and growth rates, more ethnically or religiously diverse countries have been no more likely to experience significant civil war in this period. We argue for understanding civil war in this period in terms of *insurgency* or rural guerrilla warfare, a particular form of military practice that can be harnessed to diverse political agendas, including but not limited to ethnic nationalism. The factors that explain which countries have been at risk for civil war are not their ethnic or religious characteristics but rather the conditions that favor insurgency. These include poverty and slow growth, which favor rebel recruitment and mark financially and bureaucratically weak states, rough terrain, and large populations.

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1 Introduction

Using data to be discussed below, Figure 1 shows the number of countries with ongoing civil wars by year from 1945 to 1999. Since the number of independent states has grown sharply in this period, it also shows the proportion of countries with at least one ongoing war in each year.

The graph indicates that, contrary to popular belief, the prevalence of civil wars in the 1990s is *not* a post-Cold War phenomenon. There is indeed a large increase after 1990 associated in part with the collapse of the Soviet Union, but this is followed by a marked *decline* in the second half of the decade.¹ It should be noted that the basic pattern observed in Figure 1 is not an artifact of the way we have coded “civil war”; it is observed in a broad range of other data sets on violent domestic conflict for this period (e.g., Gantzell 1997; Gleditsch et al. 2001).

Thus, the prevalence of civil wars in the post-Cold War world is not due to effects of the end of the Cold War. Nor is it the result of civil wars breaking out at a steadily increasing rate over the period. The rate of outbreak is 2.3 per year since 1945, highly variable by year but showing no significant trend up or down. The problem is that at the same time, civil wars have ended at a rate of only about 1.7 per year. The result has been a steady, almost linear accumulation of unresolved conflicts since 1945.

Put differently, states in the international system have been subject to a more-or-less constant risk of violent civil conflict over the period, but the conflicts they suffer have been difficult to end. The average duration of the civil wars in progress has increased steadily from 2 years in 1947 to about 15 years in 1999. From a policy perspective this is an important observation. It suggests caution about seeing as a temporary “blip” the sorts of military and political problems Western foreign policy makers have faced recently in Kosovo, Macedonia, Bosnia, Somalia, Haiti, East Timor, Colombia, and elsewhere.

What explains the susceptibility of states in this period to hard-to-end civil wars, and thus the secular trend? A natural place to look for an answer, and an important puzzle in its own right, is the question of why some states have had civil wars in this period while others have not.

An influential conventional wisdom suggests an answer. Journalists, state leaders, and political scientists often interpret the prevalence of civil war in the last decade in terms of *ethnic nationalism*. In this view, the end of the Cold War helped unleash powerful, long-standing nationalist forces that produced a host of bloody civil wars in Eastern Europe,

¹Cf. Gurr (2000a), notes the decline in ethnic war in the 1990s and argues that the trend reflects improved management strategies by states and international organizations.

the former Soviet Union, and subSaharan Africa. The root cause of these civil wars is seen as the nationalism of culturally distinct ethnic minorities who rebel either because they harbor grievances arising from discrimination by ethnic majorities, or because ethnic or “civilizational” cleavages within a state are untenable due to cultural incompatibilities and nationalist aspirations.

This conventional wisdom is an example of a broader approach that tries to explain rebellions by referring to *the motivations of rebels*. The core idea is that rebels are purposive actors seeking some end, such as redressing religious, nationalist, or economic grievances (Gurr 1971; Gurr 2000b), or just “loot” (Collier and Hoeffler 2001). Journalists and academic researchers commonly find rebels explaining their own actions by referring to such motivations (or at least to grievances), often entirely plausibly. A next step for political scientists and sociologists has been to look for measures of the presence of motivations to rebel in order to explain why some countries get civil wars (Gurr 1971; Gurr 2000b; Muller 1985).

Those associated with the “social movements” and “state centric” literatures rejected approaches focused on grievances and rebel motivations as overly instrumentalist (McCarthy and Zald 1977; Tilly 1978; Tarrow 1996; Skocpol 1979).² McCarthy and Zald emphasized “resource mobilization,” the capacities of organizations to produce collective action. Tilly and Tarrow took “political opportunity structure” from the literature on urban riots in the U.S. (Eisinger 1973), a concept that highlights the rules, institutional structure, and elite alliances that make states more or less vulnerable to protest and rebellion.³

We suggest an explanation for civil war in this period that like the earlier social movements perspective stresses opportunities over specific motivations affecting groups of people. However, we attempt to specify the “opportunity structure” more precisely, as it exists for the particular problem of civil war in the period since 1945.

In brief, we argue that the main factors influencing which countries and groups have seen civil war in this period are not cultural differences and ethnic grievances, but rather the conditions that favor *insurgency*. Insurgency is a technology of military conflict characterized by small, lightly armed bands practicing guerrilla warfare from rural base areas. As a form of warfare insurgency can be, and has been, harnessed to diverse political agendas, motivations,

²These citations are the tip of an iceberg. Against this, Collier and Hoeffler’s (2001, 2) claim that the distinction between focusing on grievances as opposed to “constraints” “has fairly precisely divided the large political science literature on the causes of conflict from the much smaller economics literature” should be taken advisedly.

³In McAdam, Tarrow and Tilly (2000), “political opportunity structure” is conjoined with three other variables to account for contentious events: exogenous social change, mobilizing structures, that is, the pre-rebellion solutions to collective action problems by potential rebels, as resource mobilization theory emphasized; framing processes, bringing grievances back in, but now with elites representing these grievances to publics; and “repertoires of contention,” bringing in path dependency from previous protests and rebellions.

and grievances. The concept is most closely associated with communist insurgencies fought in Latin America, Asia, and Africa during the Cold War. But the methods can just as well serve Islamic fundamentalists, ethnic nationalists, or “rebels” who focus mainly on the production and sale of coca or diamonds. We certainly agree that ethnic antagonisms, nationalist sentiments, and grievances can motivate rebel fighters and that these may also help their efforts. But we suspect that these factors are far too common to reliably distinguish the relatively small number of cases where civil war breaks out.

In the next two sections, we develop some empirical implications of thinking about civil war in terms of ethnicity and group grievance versus insurgency. In brief, the ethnic nationalism view implies that more ethnically or religiously diverse countries should be more prone to civil war, and perhaps should have more bloody civil wars when they occur. Likewise, greater democracy and observance of civil liberties should be associated with lower grievances, and thus a lower likelihood of civil war.

By contrast, insurgency – and thus civil war, in our argument – should be favored in countries with financially, organizationally, and politically weak central governments. This condition renders insurgency more feasible and attractive due to weak local policing or inept and corrupt counterinsurgency practices, sometimes including a propensity for brutal and indiscriminate retaliation that helps drive locals into rebel forces. Police and counterinsurgent weakness, we argue, is proxied in a large part by low per capita income, although it can also come about more quickly due to political instability at the center or when an external power withdraws or lessens its support of a regime (e.g., the Soviet Union in Eastern Europe the former Soviet republics, France in its former subSaharan colonies in the 1990s). Poverty should also directly favor insurgency by making the life of a guerrilla relatively more attractive and so aiding in recruitment. Insurgency should be favored in addition by the presence of a rural base area, preferably with rough, inaccessible terrain; rebels with local knowledge of the population superior to the government’s, which aids rebels in hiding from superior government forces; and by foreign financial or logistic support and training.

In the empirical analyses conducted in section 5 below, we find that the onset of civil war is at best weakly related to various measures of ethnic or religious diversity or structure, when per capita income is held constant. By contrast, we estimate that each additional \$1,000 of per capita income is associated with 35% lower odds of civil war onset in any given year. Consistent with the observation that the technology of insurgency need not rely on broad grievances in a population, neither democracy nor the presence of broad civil liberties appears to reduce the incidence of civil war onsets. Again consistent with the insurgency perspective, political instability is a relatively powerful predictor of civil war onset. Mountainous terrain and noncontiguous territorial holdings appear to significantly raise the risk as well. Former French colonies in subSaharan Africa have been somewhat less likely to have civil wars and these have killed *far* fewer when they occurred. We suspect this is the result of French post-colonial military and political support to what would otherwise

be much weaker states.

The most closely related study is that of Collier and Hoeffler (2001), who examine determinants of civil war onset with a smaller list of wars coded for five-year periods beginning in 1960. They find similarly that measures of economic modernization such as income or school enrollment are better predictors of onset than are measures of ethnic fractionalization, though they also find some support for the hypothesis that “ethnic dominance” favors war (we find none). Collier and Hoeffler put considerable weight on their results for a measure of primary commodity exports, which strongly predicts civil war in their “greed model.” They initially interpreted this finding as implying that rebels are motivated mainly by “greed” rather than grievance (Collier and Hoeffler 1998, 1999; Collier 1999). More recently they argue that primary commodity exports and low schooling rates favor rebellion by easing constraints on rebel financing and staffing. By contrast, we are not surprised to find little or no impact for primary commodity exports in our data, given that ports and national distributions systems are required to exploit these effectively, and these are beyond the reach of small insurgencies. We agree with Collier and Hoeffler that low income favors rebel recruiting, but argue that the main reason that poorer countries are more disposed to civil war is that low income proxies for state, military, and police incapacity and incompetence. These in turn favor the technology of insurgency.⁴

Sections 2 and 3 develop in turn the theoretical arguments and hypotheses out of the ethnic nationalist/grievance and insurgency perspectives. Section 4 discusses our coding of civil conflicts, and section 5 presents the empirical analysis.

2 Ethnicity, discrimination and grievances

During the Cold War, political scientists and sociologists often sought to trace rebellion to economic inequality in developing countries (Russett 1964; Muller 1985); to rapid economic growth that was said to destabilize traditional rural social systems (Paige 1975; Scott 1976; Huntington 1968); or to frustrations arising from the failure to gain expected benefits of economic modernization (Gurr 1971). A few scholars had argued that the “real” source of rebellion in many cases was ethnic nationalism (Connor 1994), and a rich literature on the sources nationalist mobilization developed in comparative politics (Deutsch 1953; Gellner 1983; Anderson 1983). But this perspective did not become a dominant frame for interpreting civil war until the collapse of the Soviet Union and Yugoslavia.

⁴Differences in our civil war lists probably account most for differences in our results (where they differ). Collier and Hoeffler’s is loosely based on the COW list, and suffers, we think, from lack of clarity in the rules for inclusion and/or inconsistent application of them; see section 4 below.

Using a broad brush, we can distinguish between perennialist and modernist (or constructivist) positions on the nature and sources of ethnic nationalism. Given in purest form by nationalist politicians and journalists reporting on nationalist conflicts in progress, perennialist arguments stress long-standing cultural practices said to define and distinguish ethnic groups. Differences between these practices are argued to have made conflict more likely.⁵

By contrast, modernist theories see the thorough-going politicization of cultural difference that ethnic nationalism represents as a development of the last 200 to 500 years.⁶ The core argument is that economic modernization and the development of the modern state make upward social mobility possible, but contingent on sharing the culture of the group that dominates state or society. When state or society pose cultural barriers to upward mobility for minority groups, the minorities develop separatist nationalist movements. All the more so, according to Deutsch (1953), Gellner (1983), and Anderson (1983), the greater the preexisting cultural differences between the minority group and the dominant group. (When the preexisting differences are slight, assimilation is more likely.)

The two approaches thus yield the same hypotheses about the relationship between cultural diversity and civil conflict.

*H*₁: Measures of a country's ethnic or religious diversity should be associated with a higher risk of civil war, as should measures of the extent of cultural distance between major groups.

Seeking to explain anti-colonial nationalist movements in countries barely affected by economic modernization, Gellner (1983, 108n1) argued that even "the advance shadow" of economic modernization was sufficient to start nationalist dynamics in motion. Anderson argued that by the late 19th century nationalism had become a "modular form" that could be easily imitated around the world, even in conditions quite different from its origination. Nonetheless, if one took the modernization approach more literally one might infer that higher levels of modernization should imply higher levels of discrimination and thus more nationalist contention in culturally divided countries. Thus,

*H*₂: The effect of ethnic diversity on the probability of civil war should increase at higher levels of per capita income (a proxy for economic modernization).

⁵Academics rarely make such arguments as baldly as do nationalist leaders. But authors who stress the long-standing, "deep" nature of ethnic differences and suggest that these make domestic peace difficult include Rabushka and Shepsle (1972), Horowitz (1985), Smith (1986), Hastings (1997), Moynihan (1993), Ignatieff (1993), and Huntington (1996). Arguably, the main message of Horowitz's influential book on ethnic conflict is that ethnically plural societies face a host of pathologies that render them especially prone to conflict and, at the extreme, violence.

⁶Depending on which modernist you consult.

Horowitz (1985) and several others argue that the relationship between ethnic diversity and severe ethnic conflict should be nonmonotonic, with less violence for highly homogenous and highly heterogenous countries. The politics of a highly heterogenous country might be strategically similar to those of a highly homogenous one, since small ethnic groups must join coalitions to have political influence, and there are a great many possibilities for cross-cutting and shifts in coalition membership. Along these lines, Horowitz suggests that the most severe ethnic conflicts will arise in countries where a substantial ethnic minority faces an ethnic majority that can, given ethnic voting, win for sure in any national election. Such arguments yield

H_3 : Countries with an ethnic majority and a significant ethnic minority are at greater risk for civil war, and perhaps for bigger civil wars if they occur (since more may be at stake).

In the analysis below we consider several measures for these concepts. For H_1 we use (1) the commonly employed ethnic fractionalization index based on data from a Soviet ethnographic index (Sov 1964), which gives the probability that two randomly drawn individuals in a country are from different ethnolinguistic groups⁷; (2) a measure of the share of population belonging to the largest ethnic group that we constructed from the CIA Factbook and other sources; and (3) the number of distinct languages spoken by groups exceeding 1% of country population, based on Grimes and Grimes (1996); a measure of religious fractionalization (analogous to (1)) that we constructed using data from the CIA Factbook and other sources. For H_2 we interact these measures with per capita income. For H_3 , we use a dummy variable marking the 73 countries whose largest and second largest ethnic groups exceed 49% and 8% of the population, respectively.⁸

The mechanism that gives rise to nationalist contention in modernist arguments is state or societal discrimination along the lines of cultural difference, which is thought to create the grievances that motivate rebellions. Grievances are difficult to measure independently of our knowledge of the actions we are trying to explain (rebellions, civil war), but various measures of average levels of discrimination are feasible. Other things equal, political democracy should be associated with lower levels of discrimination and repression along cultural or other lines, since democracy endows citizens with a political power (the vote) they do not have in dictatorships. Even more directly, measures of state observance of civil rights such as freedom of association, expression, the right to trial, and so on, should be associated with

⁷With this and several other variables we filled in values for missing countries or country years whenever possible based on our own research; often the sources were the CIA Factbook, Encyclopedia Britannica, and the Library of Congress Country Studies, though we used country-specific sources when necessary.

⁸Alternative thresholds for the second largest group, such as 10%, make no difference to the results reported.

less repression and thus lower grievances. Thus,

H_4 : Measures of political democracy and civil liberties should be associated with lower risks of civil war onset.

We consider both the Polity IV and the Przeworski, Alvarez, Cheibub and Limongi (2001) democracy measures, along with the Freedom House indicator of the observance of civil liberties, which seems particularly apt.⁹ We also examine measures of state-instigated human rights abuses constructed from Amnesty International and State Department human rights reports (Poe, Tate and Keith 1999).

3 Insurgency

If many post-1945 civil wars have been “ethnic” or “nationalist” as these terms are usually understood, then even more have been fought as *insurgencies*. Insurgency is a technology of military conflict characterized by small, lightly armed bands practicing guerrilla warfare from rural base areas. We shall argue that to explain why some countries and groups have experienced civil wars in this period, one needs to understand the conditions that favor insurgency, which are to a significant extent independent of cultural differences between groups and even group grievances. These conditions are best summarized by way of a brief statement of the logic of insurgency.¹⁰

The fundamental fact about insurgency is that insurgents are weak relative to the governments they are fighting, at least at the start of operations. We mean “weak” in a specific sense. Call it *absolute weakness*: If government forces knew who the rebels were and how to find them, they would be fairly easily destroyed or captured. This is typically true

⁹Since 1972, Freedom House has coded countries annually on a 7-point scale based on a “checklist” that awards points for government observance of a long list of civil liberties and rule of law, including freedom of the press, religion, and association (including parties and trade unions), an independent judiciary, equal treatment under the law, civilian control of the police, “protection from political terror,” secure property rights, and equality of opportunity. “Freedom from war and insurgency” is one element of their checklist, so we are careful to lag this independent variable. For details on its construction, see <http://www.freedomhouse.org/>. For Polity IV, see <http://www.bsos.umd.edu/cidcm/polity/>. We follow common practice by using the difference between the 11-point democracy and autocracy scales, treating “transition periods” (-88) and “interruptions” (-77) as zeros, and foreign occupation (-66) as missing data.

¹⁰Though our formulations often differ, we have been influenced here by Stathis Kalyvas’ work on the Greek civil war. The literature on guerrilla warfare is extensive; see, for examples, Griffith (1961), Desai and Eckstein (1990), Laqueur (1976).

even in states whose military and police capacities are low.¹¹ The total number of active rebels in many wars in which thousands of civilians have been killed (through the actions of both governments and rebels) is often in the hundreds. Even in relatively “big” cases such as Kashmir or Algeria the number of active fighters rarely exceeds 10,000.

The absolute weakness of the insurgents has several implications. First and most importantly, it means that to survive the rebels must be able to hide from government forces. Several hypotheses follow immediately.

H_5 : The presence of (a) rough terrain, poorly served by roads, at a distance from the centers of state power should favor insurgency and civil war. So should the availability of (b) foreign, cross-border sanctuaries, and (c) a local population that can be induced not to denounce the insurgents to government agents.

The nationalism script and a good deal of scholarly writing on civil wars holds that ethnic or class solidarity and grievances are necessary for (c), the local population’s support of active rebels. In line with some analysts of communist insurgencies (e.g. Leites and Wolf (1970), Thompson (1966), Clutterbuck (1967), Kalyvas (1999)), we argue that while grievances and ethnic solidarity can be helpful in this regard, they are by no means necessary. Instead, the key to inducing the local population not to denounce the active rebels is *local knowledge*, or information about who is doing what at the village level. Local knowledge allows the active rebels to credibly threaten retribution for denunciation and other acts that hurt them.¹² Evidence indicates that ethnic insurgents use this informational advantage to great effect, often threatening and inflicting unimaginably harsh sanctions on “their own” people (Kalyvas 1999). The nationalist script is mistaken, therefore, to infer from the presence of an ethnic insurgency that the members of the ethnic group are of one mind in their determination to fight the state till they realize a nationalist dream. Instead, the attitudes of the nonrebel members of the ethnic minority are likely to reflect a more complex mix of sentiments – fury at discrimination and (often) police and army oppression by the dominant groups, but fear and unvoiced anger at their self-appointed national champions.

An empirical implication of the importance of local knowledge is hypothesis H_{5d} : having a *rural base* should greatly favor insurgency. In the city, anonymous denunciation is generally easier to get away with, giving the government an advantage in its counterinsurgent efforts.¹³

¹¹Contrast this to relations between states, where each knows where to find the other and more-or-less what to expect, but usually lacks the desire or ability to crush the other.

¹²A ‘second order’ mechanism by which ethnicity may favor insurgency is that ethnic minorities are sometimes marked by dense social networks that are isolated from dominant group networks, thus giving an informational advantage to local rebels (Fearon and Laitin 1996). But such an advantage does not require ethnic difference.

¹³Northern Ireland is one of very few urban-based insurgencies in our data. In a way, it is an exception

A second set of implications pertains to what might be called *relative weakness*. Within the basic constraints of absolute weakness – the need to hide and not be denounced by locals – insurgents can be more or less able to wage war against government and other targets. To survive, rebels need a supply of arms and materiel, money to buy them, or smugglable goods to trade for them. Relatedly, they need a supply of recruits to the insurgent way of life, and they may also need know-how, information and instruction in the practical details of running an insurgency.¹⁴

But the most important determinants of the prospects of an insurgency are most likely *the police and military capabilities and competences of the government, and the reach of government institutions into rural areas*. Simply put, insurgents are better able to survive and prosper if the government and military they oppose is relatively weak – badly financed, organizationally inept, corrupt, and poorly informed about goings on at the local level.

Effective counterinsurgency requires government forces to distinguish and separate active rebels from noncombatants without destroying the lives and living conditions of the latter. This is an extremely difficult political, military, and organizational problem even for well-equipped and well-paid modern militaries; witness the U.S. military’s failures in Vietnam (Krepinevich 1986; Avant 1994), early British efforts in Northern Ireland (Kennedy-Pipe 1997), or Soviet efforts in Afghanistan. It goes far beyond problematic for less well-financed and bureaucratically competent states. These either cannot prevent the abuse of local powers by field commanders, or may even permit these abuses as a sort of tax farming to the military – that is, they “pay” the soldiers with the opportunity to loot and pillage, a practice that tends to sustain rather than end insurgencies.¹⁵

This analysis suggests the following hypothesis:

that proves the rule, given the efforts the paramilitaries have invested in policing and punishing informants, and the efforts the British forces have made to cultivate informants.

¹⁴In the case literature on insurgencies one frequently finds that rebels leaders have either spent time at guerrilla training camps in Libya, Afghanistan, Lebanon, Mozambique (in the 1970s), or that they gained guerrilla experience in one insurgency that they apply in pursuing another.

¹⁵ See Keen (1998) for some examples. A more elite-based example comes from Angola. Flush with increased oil revenues, Angola’s leadership has chosen in recent years to invest in sophisticated and expensive aerial bombardment technology rather than in paying soldiers better and improving their ability to administer areas they captured from UNITA. Why? Expensive equipment allows for lucrative kick-backs from Western defense firms, even if it will be less effective in winning the war and peace. Regarding the growing insurgency in Macedonia, a *New York Times* war reporter writes “To take the fight to their foe and regain territory, the Macedonians have used Soviet-era artillery and helicopters. But those strikes have endangered civilians as much as the enemy, and they seem to have primarily succeeded in generating more popular support for the rebels among the ethnic Albanians in Macedonia. ... What the nation lacks is a sufficient number of well-trained infantry troops able to take towns without leveling much of them ... ” Michael R. Gordon, “A Hobbled Army Casts a Cloud in Macedonia,” 6 July 2001, National Ed.

H_6 : Proxies for the relative weakness or strength of the insurgents – that is, their odds of being killed or captured for a given level of counterinsurgent effort by the government – should be associated with the likelihood that a country develops a civil war. In particular, *higher per capita income* should be associated with a lower risk of civil war onset because (a) it is a proxy for a state’s overall financial, administrative, police, and military capabilities, and (b) it will mark more developed countries with terrain more “disciplined” by roads and agriculture and rural society more penetrated by central administration.

Following the arguments given above, there is an additional reason why lower per capita income should favor the technology of insurgency: (c) recruiting young men to the life of a guerrilla will be easier when the economic alternatives are worse. Though we try below, it is difficult to find measures that allow one to distinguish between these three mechanisms associating lower per capita with civil war onset. We believe that the strong results for per capita income reported below are largely due to its acting as a proxy for state military and police strength relative to potential insurgents, which can deter or squash nascent insurgencies (a and b in H_6). But this remains something of a conjecture given the data we have.

Additional factors that would be expected to affect (or proxy) the strength of an insurgent band *relative* to a state follow.

H_7 : The political and military technology of insurgency will be favored, and thus civil war made more likely, when potential rebels face or have available:

- (a) Political instability at the center, which would typically indicate disorganization and weakness and thus an opportunity for a separatist or center-seeking rebellion.
- (b) A large country population, which makes it more difficult for the center to keep close tabs on who is doing what at the local level, and also increases the number of potential recruits to an insurgency for a given level of per capita income.
- (c) A territorial base that is separated from the territorial base of the center by water or distance – for example, East Pakistan (now Bangladesh) from West Pakistan, West Papua from Java, or Angola from Portugal.
- (d) Foreign governments or diasporas willing and able to supply weapons, money, or training and know-how. This might be called “insurgency as international and transnational politics by other means.”
- (e) Land that supports the production of high value, low weight goods such as coca, opium, diamonds, and other contraband, which can be used to finance (and sometimes just motivate) an insurgency.

Partially excepting (d),¹⁶ none of these conditions crucially involves cultural differences, ethnic minority status, or group grievances. We do not claim that these factors provide no help to would-be insurgents in many specific cases. But – to reiterate – we suspect that grievances and ethnic differences are far too common to help distinguish the countries and years that see civil wars, and we argue that in any event the technology of insurgency does not require strong popular grievances or ethnic solidarities to operate effectively.¹⁷ This latter point suggests the following contrast to H_4 above:

H_8 : After controlling for per capita income (or other measures of state strength), neither political democracy nor presence of civil liberties should be expected to associate strongly with lower odds of civil war. Given the right environmental conditions, insurgencies can thrive on the basis of small numbers of rebels without strong, widespread, freely-granted popular support – hence even in democracies.

As to measures, for “rough terrain” we use the proportion of the country that is “mountainous” according to the codings of geographer A.J. Gerard.¹⁸ This does not pick up other sorts of rough terrain that can be favorable to guerrillas such as swamps and jungle, and it takes no account of population distributions or food availability in relation to mountains; but it is the best we have been able to do at present for H_{5a} . For H_6 we use Penn World Tables and World Bank data on per capita income, estimating missing values using data on per capita energy consumption.¹⁹ For H_{7a} – political instability – our primary measure is

¹⁶Foreign governments often fund insurgents who have no ethnic ties to the population of the foreign government. Saideman (2001) argues, however, that ethnic ties best predict third-party support for rebel movements.

¹⁷Another way to state the claim is that there is a difference between having a motivation (e.g., a grievance) and acting on it. We do not doubt that many or most insurgents are motivated at least in part by the grievances they articulate, but we do doubt that the existence of the grievances sharply distinguishes them from others in the same country and other countries who are not rebelling.

¹⁸Gerard produced this measure for Paul Collier’s project on civil wars at the World Bank; many thanks to Collier for letting us use it here. Our sample of countries differs slightly from Collier’s. We estimated values for the 21 missing countries by using the difference between the highest and lowest point of elevation in each country, which proves to be well correlated with the mountains measure (.78 in logs).

¹⁹Estimates of growth rates from the World Development Indicators 2000 were first used to extend the Penn World Tables income estimates (which cover 1950 to 1992) wherever possible. For countries with at least 12 common observations on this estimate and the COW estimate of total energy consumption, we took the predicted values from the regression of $\log(\text{PWT or WB income})$ on a model that included country dummies, the interaction of country dummies and year, and the interaction of country dummies and the log of energy consumption per capita, in effect fitting country-specific growth rates adjusted by country-specific evidence on energy per consumption ($R^2 = .993$). For countries with more than zero but fewer than 12 common observations we took the predicted values from the same regression but used the average rather than country-specific estimated impact of energy consumption (if these cases are included in the first set,

a dummy variable indicating whether the country had a 3-or-greater change on the Polity IV regime index in any of the three years prior to the country-year in question.²⁰ Country population (H_{7b}) is based largely on World Bank figures, and we coded a dummy variable for states with noncontiguous territory ourselves (H_{7c}).²¹

The remaining hypotheses (H_{5b-d} , H_{7d-e}) present more difficult measurement challenges for a cross-national study. Whether availability of a rural base favors insurgency (H_{5d}) is better tested in a research design where ethnic groups are the unit of analysis, so that groups with different geographic concentrations and locations can be compared.²²

Although it is possible to code rebellions *in progress* for whether the rebels receive shelter and financial or logistic support from foreign countries (H_{5b} , H_{7d}) the *potential* availability of these aids to the relative strength of rebels is difficult to observe and measure *ex ante*, prior to the onset of any fighting.²³ Three special cases, however, represent instances where the potential availability of military and logistic support from a foreign power to *governments* is observable in the form of major power foreign policies – in Soviet policy (the “Brezhnev doctrine”) in Eastern Europe, U.S. policy with respect to rightist regimes in Latin America during the Cold War, and French policy with regard to its former colonies in subSaharan Africa (at least until the late 1980s). By our analysis above we would expect such support to increase the relative advantage of government forces against potential rebels and thus associate with lower rates of civil war onset and fewer deaths when they occur. We also consider some even more tenuous possible measures of potential support to rebels, such as the existence and number of civil wars on-going in neighboring countries, which might be thought to associate with more easily available weapons, training, or the presence

bizarrely low estimates for a few Eastern European countries in the 40s and 50s result). For countries with no income data, we use the predicted values based on the regression of log(PWT or WB income) on log(per capita energy consumption).

²⁰ “Transition periods” (-88) and “interruptions” (which indicate a “complete collapse of central authority”) are coded as cases of instability; foreign occupations are treated as missing. In addition, we code the first year of a state’s existence as a political instability year.

²¹ Countries with territory holding at least 10,000 people and separated from the land area containing the capitol city either (1) by land or (2) 100 kilometers of water were coded as “non-contiguous.” Ignoring the colonial empires, 25 of our 161 countries meet this criterion at some time since 1945; with the empires, the figure is 26 (since all but one former empire, Belgium, remained “noncontiguous” by this measure after dissolution).

²² Using the Phase III Minorities at Risk data, Fearon and Laitin (1999) found that groups without a rural base area were far less likely to be engaged in violent conflict with the state, even after controlling for a range of country and group-specific factors (and including measures of relative group deprivation). Toft (1996) was the first to note and examine the strong bivariate relationship in the MAR data.

²³ Collier and Hoeffler (2001) develop a measure of diasporas based on number of members of an ethnic group resident in the U.S.; we do not have access to this measure at present.

of experienced guerrillas looking to “ply their trade.”²⁴

4 Dependent variables

Working from existing civil war lists such as those provided by the Correlates of War project (Singer and Small 1994), the Institute for International and Strategic Studies (IISS 2000), Licklider (1995), Sivard (1996), Doyle and Sambanis (2000), the State Failure Project (Esty et al. 1998) we constructed a list of violent civil conflicts that we at present believe meet the following criteria:

1. They involved fighting between agents of (or claimants to) a state and organized groups who sought either to take control of a government, take power in a region, or use violence to bring about a change in government policies.
2. The conflict killed or has killed at least 1000 over its course.
3. At least 100 of the dead are on the side of the government (including civilians attacked by rebels). This last condition is intended to rule out state-led massacres where there is no real organized or effective rebel opposition.

The three criteria for inclusion are very similar to those stated by COW, State Failure, Doyle and Sambanis, and several other researchers. We developed our own list (working from these and other sources) mainly because we wanted data for the whole 1945-99 period, and because of doubts about particular inclusions and exclusions in each list. Also, in the case of the often-used COW data we had a variety of disagreements and doubts about the criteria employed and their application.²⁵ After developing our case list, we obtained a copy of a

²⁴The presence of valuable minerals or the suitability of land for the cultivation of opium, coca, or other valuable contraband is also codeable in principle; but at present we lack such measures (H_{7c}). Nor do we have measures for the comparative disadvantage of governments in access to village-level information (H_{5c}).

²⁵The main difference between our formal criteria and most others lies in condition (3). The parallel condition stated by COW is that 5% of the dead be on the side of government. If it could be implemented reliably, this criterion would imply that a conflict in which 1000 are killed total with 50 on the side of the government qualifies, while one with 1 million dead and 40,000 on the side of the government does not. This seems unintuitive. In practice, it is not clear that COW follows the stated criterion consistently in any event. There is also the problem (noted by others) that it is unclear whether the COW list requires 1000 dead total or 1000 dead per year. Ambiguity on this point may help account for why the COW list omits numerous conflicts that would seem to qualify at 1000 total. Finally, the COW list ends in 1992, which makes it less suitable for our purposes. Collier and Hoeffler (2001) base their list on COW, breaking a number of COW civil wars into multiple wars according to unspecified criteria, and including some colonial wars (coded in non-existent “states,” such as Angola 1961) but not others.

parallel effort by Gleditsch, Strand, Eriksson, Sollenberg and Wallensteen (2001), who list all “armed conflicts” for the same period using similar criteria though with a lower threshold of deaths. After making allowance for the different threshold and requirement (3), we find that our lists are very close.

In one respect our data differ significantly from most other civil war lists: we see no reason in principle to exclude, as many have done, colonial wars, such as the French versus the FLN in Algeria. We count these as occurring within the colonial empire. Thus, the French state/empire looks highly civil war-prone by our list, with 5 colonial wars occurring in the course of the 1950s. But to drop such cases would be like dropping the current conflict in Chechnya as a civil war in Russia if the Chechens were to succeed in gaining independence. Alternatively, it would make even less sense to include them as wars within “states” that did not exist (such as “Algeria” in 1954).

There are both practical and theoretical considerations pointing the other way, however. In practical terms, to include the anticolonial wars in the analysis requires that we form estimates of GDP per capita for whole empires, along with ethnic fractionalization and democracy scores. Further, these estimates must change almost by year, as the French, British, and Portuguese empires steadily diminished in size over the post-1945 period. While we are able to use the country-level data to produce such estimates for ethnic fractionalization, our estimates for per capita income are much more problematic, and the question of how to code the empires on a democracy index is vexed. Regarding theory, the colonial empires differed so radically in so many ways from other independent states, and faced such an inhospitable international environment after the war (with pressure from the U.S. and the new U.N. system), that we need to be very cautious about interpreting any empirical results that depend wholly on these cases. For these reasons, in the analysis below we examine the data both with and without the 13 colonial wars in our list.

Another difficult coding question concerns how to date war starts and war ends; the former is particularly important when the dependent variable is war onset. Naively, we would like to say that a civil war begins when the killing begins and ends when the killing ends. This rule can be applied without much difficulty to the majority of cases. But some insurgencies sputter along at very low levels of intensity before suddenly flaring into a significant conflict, and some wars seem to end only to begin again within a few months or years. If the war started very gradually, we tried to code the first year in which at least 100 were killed and which was followed by fairly continuous fighting. For war ends we made a peace agreement or decisive victory followed by at least two years of peace a sufficient condition.²⁶ Since any coding scheme for civil war starts and ends is likely to be arbitrary “around the edges,”

²⁶If one side in a war is decisively defeated and a new war starts immediately with different parties (e.g., Afghanistan 1991, Somalia 1991 but not Angola 1992), we code a new war. Ideally we would also like to code as war ends two years of peace plus fairly successful disarmament and demobilization, but have not completed this work yet. Precision about war end dates is less important given our purposes here than it is

we also tried breaking the sample into five-year periods for each country. While it can be difficult to identify the specific year in which a given war started, one can say with greater confidence whether it started in a given five-year period.

Some summary statistics before turning to the analysis. We identified 126 conflicts that meet the threshold criteria given above, of which 13 were anticolonial wars. This makes for 126 civil war starts in a sample of 6,610 country-years, or about 2%. Table 1 provides some figures by decade, and shows that civil war onsets have been particularly common (relative to country years) in the late 1940s and in the 1990s, periods immediately following international systemic change. The two most civil war-prone regions in this period were Asia and sub-Saharan Africa, with 27 and 26 percent of all onsets, respectively, followed by North Africa/Middle East (13.5%), Latin America (12.7%), “Europe” and North America (12.7%), and Eastern Europe (8%) (although Europe is significantly lower, at 2.3%, if the anticolonial wars are omitted). The rate of civil war onset follows a similar pattern, with war starts in 3 out of 100 country-years in Asia, 2.1 in 100 in sub-Saharan Africa, and from 1.3 to 1.8 (North Africa/Middle East) in the rest. Indonesia is the most civil war-prone country in this sample with seven onsets, followed by India with five.

5 Empirical analysis

Our central hypotheses concern the relationship between ethnic and religious diversity or structure, on the one hand, and the susceptibility of a country to civil war, on the other. We begin with a simple analysis that makes comparisons across countries, proceeding to a more developed multivariate analysis using country-years.

5.1 Are more diverse countries more prone to civil war?

The reader will encounter a thicket of logits and asymptotic t -statistics further below. But the main story emerging from them is made strikingly clear by the simple cross-tab given in Table 2. Here the 161 countries in our sample are divided into nine categories, according to whether they have a low, medium, or high levels of ethnic fractionalization and per capita income.²⁷ The three categories divide the countries into equal size groups (terciles) on each

in an analysis of the determinants of civil war duration; see Fearon (2001), which has a longer discussion of the problem coding starts and ends.

²⁷To avoid confounding the effect of income on the likelihood of civil war with the effect of civil war on income, for each country we use average income for all years prior to the first onset of civil war, if any occurred. The table looks almost identical if we use average income over the whole period for each country.

variable. Note that the data for Table 2 omit the 13 anticolonial wars, due to the radical changes in income and ethnic fractionalization in the shift from empires to states (more on this below).

Each cell contains the average number of civil war onsets for the countries that category and the total number of countries in the category. Observe that onsets per country drop dramatically as income level increases *for every level of ethnic fractionalization*. By contrast, within income categories, *there is no consistent effect as we move from low to high levels of ethnic fractionalization*. Indeed, for the poorest two thirds of the countries, the number of civil war onsets per country looks essentially flat with respect to ethnic heterogeneity. Only in the *richest* third of countries is there any indication of an increasing relationship between cultural diversity and a propensity for civil war (and this should be qualified by the small number of cases in the high-diversity, high-income cell).²⁸

The pattern holds up even more strongly in a Poisson regression analysis where the dependent variable is a count of the total number of civil war onsets for each country.²⁹ In equations 1 and 2 in Table 3, the independent variables are average per capita income for the years prior to the first civil war onset (if any), the log of country population (averaged for years before any war onset), and two of our measures for ethnic and religious diversity. Notice that the coefficients for ethnic fractionalization and size of largest ethnic group are statistically and substantively indistinguishable from zero. The same goes for the religious diversity measures, which actually have the opposite sign expected by H_1 .

By contrast, lower per capita incomes and higher country populations appear strongly related to a higher propensity for civil war for this period, in statistical terms. Substantively, the estimates imply that a country at the 10th percentile on income (\$580) had about a 72% chance of having at least one civil conflict in this period, and a 36% chance of two or more. This compares to a 44% chance of at least one conflict for a country at the median (\$2,167) and a mere 2.7% for a rich country at the 90th percentile (\$8,650).³⁰ A country at the 90th percentile on population (36 million) and at the median for the other variables had about an estimated 63% chance of suffering at least one conflict; a country at the 10th percentile (1.2 million) had about a 30% chance.

²⁸The same basic pattern is evident if we use our other measures of cultural diversity, such as religious fractionalization or size of plurality ethnic group. Countries that are polarized in the sense of H_3 prove to have a slightly *lower* mean number of civil war onsets than countries that aren't, even without controlling for per capita income (.69 versus .86).

²⁹A Poisson distribution is more appropriate than a negative binomial here – estimating the latter yields a dispersion parameter that is essentially zero (i.e., a Poisson distribution). Ordered logit, logit with ‘one or more wars’ as the dependent variable, and OLS all yield nearly identical results.

³⁰The other variables are held at their medians for this calculation, which was performed by simulation using Clarify (Tomz, Wittenberg and King 1999). The same applies for similar results reported below.

The empirical pattern is thus inconsistent with the “nationalist script” that sees ethnic diversity as a major and direct cause of civil violence. It might be consistent with an interpretation of a modernist perspective on nationalism that sees ethnic antagonisms as “activated” only by high levels of modernization (H_2), although this is not really what the main exponents of this school suggest.³¹ Ethnic diversity could still have an indirect effect as a cause of civil war, if it causes low per capita income (Easterly and Levine 1997) and/or a weak state. But in this case the mechanisms that actually bring about violence would most likely be those of the insurgency perspective, not the nationalist script in either perennialist or modernist form.

Equation 3 in Table 3 introduces two other factors that vary little over time, (the log of) the measure for mountainous terrain and the dummy variable marking countries with noncontiguous territory.³² Both are strongly related to higher numbers of civil war onsets. Substantively, a country that is about 10% “mountainous” by this measure (the 50th percentile) had an estimated 40% chance of at least one war in this period, versus 28% for a country that is relatively flat (0% mountainous, the 10th percentile). A country with noncontiguous territory and at the median for other variables had an estimated 65% chance of at least one war and 30% chance of two or more, against 40% of one or more for territorially compact countries. Note that this substantial difference obtains despite the omission of the war-prone and highly noncontiguous colonial empires in the analysis in Table 3.

What about the anticolonial wars? By using the ethnic fractionalization, population, and income data for each colony in its first year of independence, we were able to produce annual estimates for these quantities for each colonial empire in the years before its demise. The income estimates are systematically too high, since we are using (for example) Nigerian GDP per capita in 1962 as an estimate of its GDP per capita as a British colony in 1945. This creates some bias against the income variable. In addition, taking countries as the unit of analysis means that, for instance, Britain and France appear once each in the data as extremely culturally diverse middle-income countries that suffered multiple civil war onsets; the richer postcolonial states of Britain and France that have been civil war-free are missing.

Not surprisingly, then, the cross-tab in Table 4 shows a somewhat stronger tendency for onset rates to increase with higher levels of ethnic fractionalization.³³ However, when

³¹And the relationship suggested by H_2 and column 3 of Table 2 is only weakly discerned in the poisson regression when we interact income with our measures of cultural diversity; estimated coefficients are consistent with H_2 but do not come close to statistical significance.

³²We add 1 to the terrain variable (which is measured from 0 to 100) and log it because it is highly skewed. It also appears that most of the effect is associated with very flat as opposed to somewhat or highly mountainous countries

³³The 1.429 figure in the high diversity, middle income category drops to about 1.0 when the French empire

we use regression analysis to consider the continuous versions of these two variables as in equation 4 of Table 3, the impact of ethnic diversity remains statistically and substantively weak while that of per capita income remains strong.

5.2 A multivariate analysis of civil war onset using country-year data

We coded a variable **onset** as ‘1’ for all country years in which a civil war started and ‘0’ for all others. Equation 1 in Table 5 shows the results of a logit analysis using **onset** as the dependent variable and a fairly full specification of independent variables discussed in sections 2 and 3. **war**_{*t*-1} is a control variable indexing whether the country had a civil war ongoing in the previous year.³⁴

The first thing to note about these results is that per capita income (**gdp**_{*t*-1}, measured in thousands of 1985 U.S. dollars) is strongly significant in both a statistical and substantive sense. \$1,000 less in per capita income is associated with 35% greater odds of civil war onset, on average, after controlling for a variety of other factors. Compare two countries, both at peace and with the median values on all other variables in equation 1 except for per capita income. A country at the median level of income (about \$2,000) is estimated to have a 1.2% chance of civil war breaking out in the next year, whereas a country at the 10th percentile on income (\$576) faces a 1.8%, or about 54% greater risk. A similar country at the 90th percentile on income (\$9,466) has a mere .15% chance, not even 10% that of the 10th percentile country. It should also be noted that the per capita income variable is not just serving as a proxy for the rich Western countries, which might have low rates of civil war onset for reasons of culture, history, or international relations that have little to do with income. The estimated coefficient changes from -.29 to -.24 when a dummy for “the West” is included, and remains strongly significant despite the high correlation between the two variables.

The second thing to note is that the measures for political democracy (**democ**_{*t*-1}), ethnic and religious fractionalization (**ethfrac** and **religfrac**) are all statistically insignificant, contrary to the implications of hypotheses 1 and 4 that follow from the ethnic nationalist/grievance perspective. **democ**_{*t*-1} is the lagged value of the (often used) difference between the Polity 4 democracy and autocracy measures; it ranges from -10 to 10. Its es-

is omitted (due to its six colonial wars that make our threshold).

³⁴When **onset** is taken as the dependent variable, the data is structured as grouped duration data and we need to consider the possibility of temporal dependence between observations within countries. We followed Beck, Katz and Tucker (1998) by including (1) dummies for each successive “peace year” prior to an onset, and (2) fitting natural cubic splines with knots at years 1,4, and 7, and then at 1, 2, and 5. In all instances the coefficients and standard errors estimated for the other variables remained virtually unchanged, with the spline or dummy coefficients jointly insignificant.

estimated coefficient is in the “wrong” direction – that is, more democracy, more civil war – but in statistical terms it is indistinguishable from zero. Though available for fewer years (1972-99), the Freedom House 7-point scale measuring observance of civil liberties should be a better indicator for discrimination, oppression, and hence grievances. As shown in equation 2, however, using lagged civil liberties in place of the Polity measure yields similar results; the sign of the coefficient is correct for H_4 (higher values of the scale mark worse civil liberties), but its estimated standard error is as large as the estimated coefficient.³⁵ These negative results for political democracy and civil liberties do not depend on the inclusion of the other variables in equation 1, apart from per capita income. More democratic regimes appear marginally less likely to have civil strife in a bivariate logit, but the relationship turns around and becomes statistically insignificant when we control for per capita income. The same pattern holds for civil liberties.

Alternative measures of ethnic and religious diversity and structure are similarly unsuccessful in predicting civil war onset rates. Equation 3 replaces the ethnic and religious fractionalization measures with variables for the population proportion of the largest ethnic and religious groups in the country (**ethplural** and **religplur**). Neither is statistically significant at the conventional 5% level, although **ethplural** comes somewhat close in this specification ($p = .095$). The coefficient for **ethplural** implies a moderate effect – a 43% decrease in risk going from a country with 30% versus a 75% ethnic plurality (10th to the 50th percentile). But both the coefficient and even more its standard error are highly dependent on the particular specification we choose. Statistical significance drops precipitously, for example, in a logit with just per capita income and **ethplural**, if we include a dummy variable for the Western countries, if we add a measure of percent Muslim (see below), or if we drop any of **war** _{$t-1$} or the variables for mountains or non-contiguous territory, singly or in combination.³⁶ When we use our third measure of ethnic diversity – the number of languages spoken by at least 1% of the country population – we obtain results that are “in between” those for **ethfrac** and **ethplural**. So if there is support here for H_1 , it is weak and uncertain.

Nor are countries that are ethnically or religiously polarized in the sense of H_3 significantly more likely to experience major civil violence. Equation 4 uses dummy variables (**ethpolar** and **relpolar**) for countries that have an ethnic or religious majority and a minority of at least 8% of country population in the 1990s. Religious polarization has the “wrong” sign and neither comes close to statistical significance. This finding does not depend in the

³⁵We also considered the Przeworski et al. (2001) dichotomous democracy measure, which is available for a much smaller number of country years than is the Polity measure. It is likewise unrelated to civil war onset, and when it is used in place of **democ** _{$t-1$} the other coefficients in equation 1 change very little.

³⁶**ethplural** is coded with 1990s data, so that in contrast to **ethfrac** it may encode more ex post assessments of ethnic differences. For example, the Soviet geographers saw Somalia as ethnically homogenous, whereas our measure **ethplural** codes Somalia as quite diverse on the basis of the largest clan grouping.

least on what other variables are included in the model.³⁷

The third set of points to make about these main results concerns the four remaining variables suggested by our arguments about the political and military technology of insurgency. Political instability (**instab**) in the three years prior to the year in question is *powerfully* associated with an elevated risk of civil war onset – the odds increase by an (estimated) factor of 2.7.³⁸ What is more, this effect is almost surely underestimated, since we do not code political instability in the current year in order not to confound the effect of political instability on civil war onset with the reverse. Looking case-by-case at the onsets suggests that quite often political instability immediately precedes or occasions the fighting, as with a number of coup attempts that lead into larger civil wars (e.g., El Salvador 1979).³⁹

The measure for noncontiguous territory is likewise strongly associated with a higher risk of onset. The estimate implies odds are twice as great in any given year for a country with geographically separated territory, other things equal. More mountainous countries are estimated to have a higher probability of civil war onsets, about double the risk in moving from a country that has quite flat terrain to one that is 10% mountainous by this measure (the 50th percentile). Finally, countries with larger populations appear more likely to have civil wars by these measures, although in substantive terms the marginal effect is not great. Other things equal, doubling country population is associated with only a 20% increase in the annual odds of civil war onset. Still, population can double many times in ranging from, say, Swaziland, to China, so that the latter’s annual odds of conflict are estimated to be about 2.6 times greater than that of the former (the estimated probabilities are .0066 and .0171). This is consistent with H_{7b} of the insurgency perspective, although it is also consistent with a simpler “troublemaker” theory (more people, more potential rebels).

5.3 Other variables and robustness checks

1. *Anticolonial wars.* The preceding analyses omitted the 13 anticolonial wars in five colonial empires (Britain, France, Belgium, Portugal, and the Netherlands). We estimated annual per capita income and ethnic fractionalization for the empires using data on the former

³⁷Collier and Hoeffler (2001) find that “social fractionalization,” the interaction of ethnic and religious fractionalization, to be negatively related to onset. Others have argued that **ethfrac** has a non-monotonic relation to conflict. Neither relationship appears in these data.

³⁸The implied 95% confidence interval for this effect ranges from 1.75 to 4 times the odds if there is no political instability.

³⁹Snyder (2000) has argued that democratization puts states at greater risk for civil war, ethnic in particular. Coding variables for 3-or-greater moves toward and away from democracy on the Polity index, we find in these data that moves *away* from democracy are much more strongly associated with civil war onset in the next year than moves toward it, though both forms of instability elevate the risk.

colonies in their first year of independence.⁴⁰ Equation 1 in Table 6 reports a logit analysis using these data with the independent variables income, population, ethnic fractionalization, and ongoing war. Given that the empires were all in the top 5% by this measure of ethnic diversity, and that they were quite civil war-prone in this period, it is not surprising that equation 1 displays a more substantively and statistically significant effect for ethnic diversity.

However, this effect depends almost entirely on the inclusion of the anticolonial wars and the French empire in particular, which had 6 of the 13 imperial wars by our codings. Because the empires differ markedly from the other states in the sample in many respects other than ethnic diversity, we should ask whether the effect is properly attributed to this cause. Equation 2 in the same table includes the variable for noncontiguous territory, which is seen to “pick up” a substantial part of the effect of ethnic fractionalization and display a powerful estimated impact itself. States with noncontiguous territory are here estimated to have more than three times the annual odds of civil war onset, whereas moving from the 10th to the 50th percentile on ethnic fractionalization is now estimated to raise the odds by only (a highly uncertain) 14%. Equation 3 in Table 7 includes two additional geographic variables – the mountains measure and total land area, both logged and adjusted for the empires using data on their former colonies – that arguably cause relative weakness of the state vs. potential rebels. Total land area in particular is moderately correlated with ethnic fractionalization, and the logit has a hard time “deciding” to which to attribute the effect. Finally, equation 4 drops France from the analysis; the results indicate that the statistical significance of ethnic fractionalization depends very much on how we interpret the evidence from this one empire/country.

We should step back from the statistical analysis, however, observing that these five imperial states managed their empires for many years before World War II with fairly little active rebellion despite the same levels of ethnic fractionalization, noncontiguity, mountainousness, and total land area. Nor did discrimination and oppression suddenly increase in the empires after the war, causing anticolonial movements as a response. Rather, the war greatly weakened the main imperial states both materially and in regard to their dependence on a superpower and a new international legal order (the U.N. system) that were fundamentally hostile to colonial rule.

2. *Foreign support.* We argued above that one determinant of the prospects for insurgency is the availability of third-party financial or logistic support to either the rebels or the government of the state in question. “Availability” is difficult to observe *ex ante*, however.

One possible proxy is the number of civil wars ongoing in neighboring countries, although potentially other mechanisms beside increased availability of arms, support, and

⁴⁰Recall that this means that income in the empires is overestimated, which would tend to bias the results in favor of ethnic fractionalization.

seasoned guerrillas might be invoked here.⁴¹ We created a variable counting the number of ongoing wars in contiguous countries by year. Added to equation 1 in Table 5 its sign is as predicted, but its estimated standard error is almost as large. Civil-war-next-door is too common, occurring in almost 45% of the country-years in the sample (as compared to 15% within countries).

As discussed above regarding hypothesis H_{7c} , another possible proxy is foreign support to *governments* against domestic challengers, such as that provided by the Soviet Union to “the Soviet bloc,” France to its former sub-Saharan colonies, and the U.S. to some rightist regimes in Latin America. We cannot include a dummy variable for “Soviet bloc” in the logit analysis because it perfectly predicts civil peace – there were no civil wars recorded in our data for these countries prior to the break up of the Soviet Union. But by itself this observation is consistent with our argument.

In the late 1980s, Mitterand’s government departed from long-standing French foreign policy by supporting, to a limited extent, democratization in some of its former sub-Saharan colonies. This involved encouraging “national conferences,” elections, and some political opposition, which all suggested that the prior policy of immediate military support for French-speaking dictators might have changed. Table 7 compares civil war onset rates by country and decade for the former French versus other sub-Saharan states. It shows that, until the 1990s, the former French sub-Saharan countries were remarkably free of the civil violence that beset former British, Belgian, and Portuguese colonies in Africa, the only exceptions being Chad (1965) and Mali (1989, a case that that really belongs with those of the 1990s). If a variable marking former French colonies in sub-Saharan Africa prior to 1990 is included in the model of equation 1, Table 5, its estimated coefficient is -1.36, which in substantive terms implies a four-fold reduction in the annual odds of civil war onset ($p = .066$). (The other estimates remain stable.) A dummy variable for *all* former French colonies is unrelated to civil war onset, which suggests that the observed effect in sub-Saharan Africa is due to the mechanism behind H_{7c} rather than the effects of French colonial administration or law.

[We are still coding the left right variable for Latin American regimes during the Cold War... we do not expect strong results ...]

3. *Other variables, other hypotheses.* In this subsection we consider a variety of other independent variables that have either been suggested as causes of civil war or are just plausible controls.

(a) *Regional Effects.* As conventionally delimited, the different regions of the world share a variety of historical, cultural, and economic traits. It is reasonable to wonder if any of the variables considered in the multivariate analysis are actually just proxies for

⁴¹See Lake and Rothchild (1998) for a discussion.

these factors. Conversely, if the inclusion of a regional dummy variable does not add to the explanatory power of the model, then we have managed to account for the influence of regional characteristics with our more general independent variables. When we include regional dummies either all together or one-by-one in equation 1 (thus comparing the average onset rate for countries of the region with the average for all other countries), we find that the coefficients and significance levels for our other variables are scarcely affected. “Western” countries are estimated to have odds of civil war outbreak about three times lower than the rest ($p = .06$), while North African and Middle Eastern countries have estimated odds twice as great, other things equal ($p = .03$); the rest of the regional dummies are insignificant whether included singly or all together, and the significance of the former two regions depends on the base category when all regions are included. A likelihood-ratio test of the model in equation 1 against the same model with all regional dummies (but one) does not quite manage statistical significance at the .05 level.

(b) *Oil and the Dutch Disease*. Oil producers tend to have weaker state apparatuses than one would expect given their level of income because the rulers have less need of a socially intrusive and elaborate bureaucratic system to raise revenues – the so-called “Dutch disease” (Chaudhry 1997; Karl 1997; Wantchekon 2000). At the same time, oil revenues raise the value of the “prize” of controlling state power. Consistent with our argument above about the importance of state capabilities for counterinsurgency, this would imply that a measure of oil production should be positively associated with civil war onset, after controlling for per capita income.

In accord with this hypothesis, when added to equation 1 the coefficient on a variable marking oil producers has a positive sign and comes close to statistical significance at the 5% level ($p = .054$), while the rest of the coefficients remain stable. The implied effect is a 75 percent increase in the odds of civil war onset for oil producers. However, if we also add a dummy variable for the North Africa/Middle East region, collinearity renders the estimates for both somewhat imprecise.⁴² So it could be that oil proxies for relative state weakness in accord with the counterinsurgency hypothesis, or perhaps oil is just standing in for some other causal factor common in these countries. Such as ...

(c) *Islam*. Huntington (1996) has argued that “Islam has bloody borders” because Islamic societies have cultural and demographic features that make them violence-prone. When we add a variable measuring the percentage of Muslims in each country to our main specification,⁴³ it gains a positive and statistically significant coefficient ($p = .026$), and a substantive effect corresponding to an 80 percent increase in risk for a 100% versus 0% Muslim society. (The other coefficients remain stable.) Once again, however, including a

⁴²Using **ethplural** in place of **ethfrac** renders the oil variable even less significant, while **ethplural**’s apparent impact is significantly reduced when **oil** is included.

⁴³Coded from the CIA Factbook and a variety of country-specific sources.

regional dummy sharply lowers the estimated effect of percentage Muslim and its statistical significance, so again we should be cautious about inferring that Islam is doing the work. (Dropping Indonesia, with seven onsets, also sharply diminishes the estimated effect for the Muslim variable.)

(d) *Young Males*. Huntington also argues that societies with a surfeit of young males will be particularly prone to civil violence; noting their large numbers in many predominantly Muslim countries, he suggests this as a part of the explanation for “Islam’s bloody borders.” Given that young males have physical and perhaps psychological characteristics that make them particularly apt as guerrillas, our arguments above on insurgency point in the same direction. If we include a lagged measure of the proportion of males aged 15 to 29 in the population⁴⁴ in equation 1, Table 1, it has the expected positive sign, but the standard error is large enough that the estimate is highly uncertain ($p = .19$). Young males appear not to account for the higher rates of civil war onset in the North Africa/Middle East region, since dummy variables for this region (or percentage Muslim) remain significant when the young-male variable is included.⁴⁵

(e) *Primary commodity exports*. Collier and Hoeffler (2001) propose that, up to a point, countries with higher levels of primary commodity exports provide better opportunities for rebels to finance themselves through “looting.” Beyond some point, however, they argue that governments’ revenue gains from such resources are large enough to make civil war less likely. They find support for such a parabolic relationship in their data, and have built a substantial part of their case for “greed vs. grievance” on this finding.

We find little support for it in these data. Equation 1 in Table 8 uses our model specification but follows Collier and Hoeffler’s procedure of grouping the data by five-year periods beginning in 1960.⁴⁶ It also includes the share of primary commodity exports in GDP along with its square, as in Collier and Hoeffler. The estimated coefficients for the commodity variables imply a *decreasing* relationship over the whole range of exports, but in any event they are statistically indistinguishable from zero. Except for country population, the estimated effects and significance levels for the other independent variables discussed

⁴⁴Data from the World Bank’s WDI2000, linearly interpolated for missing years within countries.

⁴⁵Most likely this is because North Africa/Middle East is not even the most young-male heavy region. That distinction belongs to sub-Saharan Africa, and in any event all of the relatively poor regions have very high percentages of young males. Although young-male percentage is highly correlated with per capita income, the latter remains substantively and statistically significant when the former is included. This is consistent with our argument that income matters mainly as a proxy for state capabilities rather than as a measure of the opportunity cost of being a rebel.

⁴⁶Whence begins the data series on primary commodity exports, available only at five-year intervals. Thanks to Paul Collier for giving us access to the data.

above remain quite stable despite the change in formulation.⁴⁷

We agree with the argument behind Collier and Hoeffler’s hypothesis. In our terms, insurgency is more attractive and feasible if sources of financing are readily available. We doubt, however, that primary commodity exports are a good measure of financing potential for rebels. This measure seems to combine mainly cash crop and oil exports, both of which are hard to exploit effectively without control of a national distribution system and ports. A better measure – which admittedly we do not have – would focus on the potential for mineral or contraband production that can reward control of a small “enclave” with huge profits (Leonard and Strauss 2001). We are also skeptical of the theoretical argument for an inverted-U relationship, which seems to us ad hoc. If government and rebels take equal shares of the “loot,” why should government necessarily be favored at both low and high total levels?

(f) “Anocracy.” More insight can be gained into the non-results on the relationship between democracy and civil war onset by dividing regime types into three sets along the Polity index: “autocracies” ($\mathbf{democ}_{t-1} < -5$), “democracies” ($\mathbf{democ}_{t-1} > 5$), and “anocracies,” those regimes in between. The average annual rates of onset for autocracies and democracies are 1.3% and .8%, respectively, as compared to a much greater 3.7% for anocracies. This is prima facie consistent with the views of scholars who have argued that protest and rebellion will be maximized at intermediate levels of state repressiveness and political openness. The idea is that authoritarian regimes can suppress dissent, while grievances can be handled politically in full democracies. This leaves partially democratic regimes most susceptible to political violence (Esty et al. 1998; Hegre et al. 2001; Muller 1985; Weede 1981).

An alternative hypothesis is that “anocracy” is not really a regime type at all, but rather a category that collects together weak states in the midst of political transitions or with oligopolistic in-fighting that has resulted in an unstable mix of democratic and non-democratic institutions. In fact, at 22% of all country-years “anocracy” is less common in the data than autocracy or democracy, and much shorter lived as well. On average, 10.1% of anocracies transitioned to another category *each year*, as compared to 4.3% for autocracies and 2.2% for democracies. More than one-third of all anocratic country years

⁴⁷Collier and Hoeffler drop periods in which a civil war is ongoing but did not begin. Since this systematically excludes war onsets in countries that already have a war ongoing (there are 24 in our data), we prefer to retain such years or five-year periods and include a control variable (\mathbf{war}_{t-1}) for whether a different civil war was already in progress. The non-result regarding natural resource exports remains just as strong if we follow Collier and Hoeffler’s practice. We also tried interpolating the commodity data in order to allow estimation in the country-year framework. Added to our main specification, resource exports and their square actually approach statistical significance at the five percent level, but the result is extremely fragile; it evaporates erratically with the inclusion or exclusion of other controls in equation 1 and disappears entirely if fuel exports are also included. When the variable is broken into terciles or quartiles, no intermediate level associates with a significantly higher risk of civil war onsets when included in our main specification.

suffered instability in a sense of our variable **instab**, versus about 13% for both autocracies and democracies. A dummy variable for anocracy is strongly significant if **instab** is dropped from our main specification along with **democ**_{*t*-1}, but its coefficient drops from .68 to .43 when **instab** is reintroduced ($p = .065$). The political instability measure is little affected.

Nonetheless, though it is somewhat uncertain statistically the estimate for anocracy remains substantively large; .43 would imply that anocracies have 50% greater annual odds of civil war onset than autocracies. One possibility is that “anocracy” is picking up aspects of state weakness missed by our instability variable.

(g) *Human Rights Abuses*. Poe, Tate and Keith (1999) examine cross-national determinants of government human rights abuses, using a five-point scale they developed using Amnesty International and State Department reports on 142 countries between 1976 and 1994. They find that civil war is powerfully associated with higher levels of human rights abuses. This is to be expected, given that civil wars in this period are mainly fought as insurgencies, and insurgency involves governments using violence against civilians to get to rebels and rebels using violence against civilians to protect themselves from government forces. Here, we turn Poe and Tate’s question around, asking if governments that badly abuse human rights in one year are more likely to see a civil war begin in the next.

Equation 3 in Table 8 shows that at least for the period 1976 to 1994, government human rights performance in the prior year is a relatively powerful predictor of the odds of subsequent civil war. The coefficient estimate implies that each step on the five point scale (**hrabuse**_{*t*-1}) is associated with a *doubling* of the odds of a civil war breaking out (higher values mean worse human rights abuses). Thus, a country rated at the top on human rights performance but at peace and at the mean for all other variables has an estimated annual chance of outbreak of .37%; the same country with the worst human rights performance has a risk of 5.3%.⁴⁸

Some of this effect is probably *not* causal. In a number of cases, such as El Salvador 1979, Nicaragua 1978, or Burundi 1993, the measure may merely pick up prior low-level insurgency before it reaches our coding threshold. But certainly some regimes are more brutal than others, and it is plausible that the relatives and neighbors of those brutalized could be more inclined to take up the gun themselves. This factor merits further investigation with, if possible, more data.

(h) *Inequality*. Economic inequality has often been seen as a source of grievances that could motivate rebellion (Muller 1985). Faced with very spotty data, we computed the average Gini coefficient for each of the 108 countries that have at least one estimate given in Deininger and Squire (1996). This variable is statistically insignificant with a coefficient close

⁴⁸We show the result with the Amnesty International based measure from Poe, Tate and Keith (1999); results are nearly identical for the State Department-based measure.

to zero when added to our main specification, and remains so in less full (or in a bivariate) specification. The poor quality of the data does not allow us to go beyond the claim that there appears to be no powerful cross-national relationship between inequality and civil war onset, which is consistent with the insurgency perspective we have advanced.

(i) *Trade Openness*. Using their measure of “state failure,” which includes both civil war and “disruptive regime transitions,” Esty et al. (1998) found that higher levels of openness to international trade strongly predicted civil peace. Using the Penn World Tables measure of trade as a share of GDP, we find no such relationship in our data. Trade is significantly related to civil peace in a bivariate logit, but this is because bigger countries have less trade and more civil war. When population is included, the estimated coefficient for trade approaches zero.

4. *Alternative Definitions and Coding for “Civil War”* Our coding rules admit civil conflicts in which at least 1000 have been killed. This threshold is (inevitably) somewhat arbitrary, and some would argue that it is too low to distinguish properly between “civil war” and, say, terrorism. Equation 3 in Table 8 shows the results for our main specification when the 35 conflicts in our sample estimated to have killed fewer than 5000 are recoded as zeros. The results are, if anything, stronger; the model fits at least as well if not better.⁴⁹ Note that the coefficient estimate for ethnic fractionalization falls further, an effect we observe even more strongly for **ethplural**.

The final equation in Table 8 uses the data on “armed conflicts” provided by Gleditsch et al. (2001) to provide check on how dependent our results are on our specific civil war list.⁵⁰ With one exception, the mountains variable, the results are remarkably consistent.

6 Conclusion

The prevalence of internal war in the 1990s is for the most part the result of a steady accumulation of protracted conflicts since the 1950s rather than a sudden change associated with a new, post-Cold War international system. Decolonization from the 40s through the 70s gave birth to a large number of financially, bureaucratically, and militarily weak states. These states have been at risk for civil violence for the whole period, almost entirely in the form of insurgency, or rural guerrilla warfare. Insurgency is a mode of military practice

⁴⁹By the criterion of area under the ROC-curve, a measure of postdictive ability (.79 vs. .76). We coded total deaths ourselves, working from and revising the estimates in Sivard (1996), Brogan (1998), and IISS (2000) using country-specific sources. This data on civil war magnitude is the subject of a paper in progress.

⁵⁰We coded only those cases from Wallenstein and Sollenberg that they judged to have reached the “intermediate” level of violence (1000 killed, total).

that can be harnessed to various political agendas, be it communism in Southeast Asia and Latin America, Islamic fundamentalism in Afghanistan, Algeria, or Kashmir, right-wing “reaction” in Nicaragua, or ethnic nationalism in Sri Lanka, Turkey, Rwanda, Burundi, Sudan, Ethiopia, Indonesia (E. Timor, Aceh), India (the northeast, the Sikh insurgency) and more. The conditions that favor insurgency – and in particular poverty, which favors rebel recruitment and marks weak states – appear to be better predictors of which countries are at risk for civil war than are indicators of ethnic and religious diversity, or measures of grievances such as lack of democracy or civil liberties.

How could democracy and cultural or religious homogeneity fail to associate with civil peace across countries? Viewing “ethnic wars” as a species of insurgency may help explain this seemingly paradoxical result. If, under the right environmental conditions, just 500 to 2000 active guerrillas can make for a long-running and highly destructive internal war, then the average level of grievance in a group may not matter that much. What matters is whether active rebels can hide from government forces and whether economic opportunities are so poor that the life of a rebel is attractive to 500 or 2000 young men. Grievance may favor rebellion by leading nonactive rebels to help in hiding the active rebels. But we have argued that all the guerrillas really need is superior local knowledge to the government’s, which enables them to threaten reprisal for denunciation.

Another reason that discrimination may be unrelated to violence is that governments will choose to discriminate most against the groups that are least able to sustain a rebellion, such as Roma, many urban-based minorities, or Kosovan Albanians in Yugoslavia before the KLA obtained weapons from the 1997 disorder in Albania. Collier and Hoeffler (1999) usefully observe that a grievance-based rebellion would face significant collective action problems, in contrast to a “greed-based” rebellion. We would add, however, that solving this collective action problem would not necessarily imply war. Instead, it could yield a common minority front for bargaining with center and the in-group policing of extremists. Indeed, insurgency is often better understood not as a success of collective action but as a failure by the nonrebels to police their own extremists (Fearon and Laitin 1999).

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Table 1: Civil war onsets by decade

	45-9	50s	60s	70s	80s	90s	Total
# Civil war onsets	14	19	20	24	15	34	126
% of total	11.11	15.08	15.87	19.05	11.90	26.98	100.00
Onsets/100 country-years	4.01	2.30	1.68	1.79	1.09	2.22	1.91

Table 2:
Avg. # of civil war onsets per country

Ethnic Fractionalization	Per capita income			
	Low	Medium	High	Total
Low	1.333	0.765	0.000	0.537
	12	17	25	54
Medium	1.111	0.609	0.18	0.519
	9	23	22	54
High	1.294	0.769	0.333	1.057
	34	13	6	53
Total	1.273	0.698	0.113	0.702
	55	53	53	161

Table 3
Determinants of civil war onset in 156 countries
(Poisson regression)

Dep. Var. =	Equation #			
Total Onsets	(1)	(2)	(3)	(4)
GDP/cap	-0.508 (0.089)**	-0.514 (0.090)**	-0.507 (0.089)**	-0.511 (0.091)**
Log(Pop)	0.258 (0.061)**	0.255 (0.062)**	0.166 (0.067)*	0.286 (0.057)**
ethfrac	0.079 (0.343)		0.107 (0.355)	0.308 (0.320)
religfrac	-0.364 (0.472)		-0.125 (0.469)	-0.739 (0.442)
ethplural		-0.043 (0.436)		
religplur		0.005 (0.006)		
Log(%Mountains)			0.199 (0.081)*	
Noncontiguous territory			0.758 (0.237)**	
Constant	-1.615 (0.640)*	-2.011 (0.693)**	-1.517 (0.684)*	-1.820 (0.587)**
N	156	156	156	156

Standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 4

Avg. # of civil war onsets per country/empire

Ethnic Fractionalization	Per capita income			
	Low	Medium	High	Total
Low	1.333 12	0.765 17	0.000 25	0.537 54
Medium	.818 11	0.636 22	0.286 21	0.537 54
High	1.375 32	1.429 14	0.571 7	1.283 53
Total	1.255 55	0.887 53	0.189 53	0.783 161

Table 5
Logit analysis of determinants of
civil war onset, 1945-99

Dep. Var. =	Equation #			
Onset	(1)	(2)	(3)	(4)
war _{t-1}	-0.809 (0.294)**	-1.216 (0.393)**	-0.816 (0.294)**	-0.769 (0.296)**
gdp _{t-1}	-0.300 (0.070)**	-0.288 (0.093)**	-0.291 (0.070)**	-0.326 (0.069)**
log(pop _{t-1})	0.218 (0.075)**	0.271 (0.094)**	0.230 (0.074)**	0.246 (0.074)**
log(%mountains)	0.231 (0.084)**	0.274 (0.107)*	0.224 (0.083)**	0.208 (0.086)*
noncontiguous	0.670 (0.271)*	0.963 (0.429)*	0.730 (0.272)**	0.723 (0.276)**
instab	1.001 (0.209)**	1.237 (0.277)**	1.008 (0.209)**	1.025 (0.212)**
democ _{t-1}	0.007 (0.017)		0.005 (0.017)	0.007 (0.017)
ethfrac	0.423 (0.373)	0.804 (0.528)		
relfrac	-0.015 (0.503)	-0.556 (0.689)		
civlib _{t-1}		0.093 (0.105)		
ethplural			-0.778 (0.466)	
religplur			0.003 (0.006)	
ethpolar				0.100 (0.217)
relpolar				-0.318 (0.738)
Constant	-6.451 (0.741)**	-7.402 (1.114)**	-6.137 (0.786)**	-6.196 (0.984)**
N	6321	3644	6321	6248

Standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 6

Logit analysis of determinants of civil war
onset, 1945-99, anti-colonial wars included

Dep. Var. =	Equation #			
	(1)	(2)	(3)	(4)
Onset				
war _{t-1}	-0.356 (0.248)	-0.539 (0.256)*	-0.547 (0.252)*	-0.509 (0.268)
gdp _{t-1}	-0.280 (0.060)**	-0.339 (0.064)**	-0.327 (0.065)**	-0.294 (0.063)**
log(pop _{t-1})	0.356 (0.059)**	0.255 (0.066)**	0.142 (0.087)	0.337 (0.061)**
ethfrac	0.707 (0.323)*	0.469 (0.330)	0.527 (0.356)	0.536 (0.327)
noncontig		0.976 (0.241)**	0.985 (0.240)**	
			0.225 (0.083)**	
log(totland)			0.113 (0.085)	
Constant	-6.922 (0.601)**	-5.931 (0.641)**	-6.158 (0.657)**	-6.643 (0.613)**
N	6376	6376	6354	6321

Standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 7

Civil war onsets in French vs. non-French former
colonies in sub-Saharan Africa

	45-9	50s	60s	70s	80s	90s	Total
Former French colonies							
# civil wars		0	1	0	1	5	7
# countries	0	1	15	16	16	16	16
onsets/country	–	0	.07	0	.06	.31	.44
Others							
# civil wars	0	0	4	6	6	10	26
# countries	3	5	22	25	25	25	25
onsets/country	0	0	.18	.24	.24	.40	1.04

Table 8
 Primary commodities, human rights abuses,
 and robustness checks

Dep. Var. = Onset	Equation #			
	(1)	(2)	(3)	(4)
war _{t-1}	-0.217 (0.252)	-2.225 (0.612)**	-0.780 (0.339)*	-0.686 (0.321)*
gdp _{t-1}	-0.338 (0.088)**	-0.416 (0.137)**	-0.358 (0.093)**	-0.230 (0.066)**
log(pop _{t-1})	0.139 (0.104)	0.191 (0.125)	0.199 (0.089)*	0.245 (0.078)**
log(%mountain)	0.235 (0.105)*	0.312 (0.148)*	0.274 (0.102)**	0.156 (0.088)
noncontig	0.872 (0.369)*	1.325 (0.646)*	0.847 (0.313)**	0.440 (0.293)
instab	0.702 (0.273)**	0.680 (0.390)	1.063 (0.246)**	0.763 (0.230)**
democ _{t-1}	0.008 (0.021)		0.005 (0.020)	0.001 (0.018)
ethfrac	0.576 (0.500)	0.191 (0.645)	0.364 (0.441)	0.609 (0.402)
religfrac	0.182 (0.631)	-0.863 (0.914)	0.112 (0.599)	0.248 (0.537)
pri.commod. exports _t	-0.355 (3.482)			
pri.commod. exports _t ²	-1.448 (7.167)			
hrabuse _{t-1}		0.680 (0.207)**		
Constant	-4.034 (1.112)**	-7.487 (1.323)**	-6.754 (0.893)**	-6.865 (0.772)**
N	952	2324	6321	6321

Standard errors in parentheses
 * significant at 5%; ** significant at 1%

Case list (at 8/20/01)

AFGHANISTAN	1978-92	Mujahideen	INDONESIA	1965-	OPM (West Papua)
AFGHANISTAN	1992-	v. Taliban	INDONESIA	1975-99	E. Timor
ALBANIA	1997-97	Pyramid, rebels in South	INDONESIA	1991-	GAM (Aceh)
ALGERIA	1962-63	Kabylie	INDONESIA	1997-	Kalimantan
ALGERIA	1992-	FIS	IRAN	1978-79	Khomeini
ANGOLA	1975-	UNITA	IRAN	1979-93	KDPI (Kurds)
ANGOLA	1997-	FLEC (Cabinda)	IRAQ	1959-59	Shammar
ARGENTINA	1955-55	Mil. coup	IRAQ	1961-74	KDP, PUK (Kurds)
AZERBAIJAN	1992-94	Nagorno-Karabagh	JORDAN	1970-70	Fedeyeen/Syria v. govt
BANGLADESH	1976-97	Chittagong Hills	LAOS	1960-73	Pathet Lao
BELGIUM	1956-61	Rwandan revolution	LEBANON	1958-58	Nasserites v. Chamoun
BOLIVIA	1952-52	MNR?	LEBANON	1975-90	various militias
BOSNIA	1992-95	Rep. Srpska/Croats	LIBERIA	1989-96	NPFL, INPFL
BURMA	1948-	CPB, Karens, etc.	MALI	1989-94	Tuaregs
BURUNDI	1972-72	Hutu uprising	MOLDOVA	1992-97	Dniestr Rep.
BURUNDI	1988-88	Org. massacres on both sides	MOROCCO	1975-88	Polisario
BURUNDI	1993-	Hutu groups v. govt	MOZAMBIQUE	1976-95	RENAMO
CAMBODIA	1970-75	FUNK	NEPAL	1997-	CPN-M/UPF (Maoists)
CAMBODIA	1978-92	Khmer Rouge, FUNCINPEC,	NETHERLANDS	1945-46	IPA
C. AFR. REP.	1996-97	Factional fighting	NICARAGUA	1978-79	FSLN
CHAD	1965-	FROLINAT, various ...	NICARAGUA	1981-88	Contras
CHAD	1994-98	south	NIGERIA	1967-70	Biafra
CHINA	1946-50	PLA	PAKISTAN	1971-71	Bangladesh
CHINA	1950-51	Tibet	PAKISTAN	1973-77	Baluchistan
CHINA	1956-59	Tibet	PAKISTAN	1990-	MQM:Sindh v. Mohajirs
CHINA	1991-	Xinjiang	PAPUA N.G.	1988-98	BRA (Bougainville)
COLOMBIA	1948-48	Conservatives v. Gov	PARAGUAY	1947-47	Febreristas, Libs, Comms
COLOMBIA	1949-62	La Violencia	PERU	1981-95	Sendero Luminoso
COLOMBIA	1963-	FARC, ELN, etc	PHILIPPINES	1946-52	Huks
CONGO	1998-99	Factional fighting	PHILIPPINES	1968-97	MNLF, MILF?
COSTARICA	1948-48	NLA	PHILIPPINES	1972-94	NPA
CROATIA	1992-95	Krajina	PORTUGAL	1961-75	Angola
CUBA	1958-59	Castro	PORTUGAL	1962-74	Guinea-Bissau
CYPRUS	1974-74	Cypriots, Turkey	PORTUGAL	1965-69	Mozambique
DEM. REP. CONGO	1960-65	Katanga, Kasai, CNL	RUSSIA	1994-96	Chechnya
DEM. REP. CONGO	1996-97	AFDL (Kabila)	RUSSIA	1999-	
DEM. REP. CONGO	1998-	RCD, etc v. govt	RWANDA	1962-65	Post-rev strife
DJIBOUTI	1993-94	FRUD	RWANDA	1990-99	RPF, genocide
DOMINICAN REP.	1965-65	Mil. coup	SENEGAL	1998-	MFDC (Casamance)
EL SALVADOR	1979-92	FMLN	SIERRA LEONE	1991-	RUF, AFRC, etc.
ETHIOPIA	1974-92	Eritrea, Tigray, etc.	SOMALIA	1981-91	SSDF, SNM (Isaaqs)
ETHIOPIA	1976-83	Ogaden, (WSLF)	SOMALIA	1991-	post-Barre war
ETHIOPIA	1997-	ALF, ARDUF (Afaris)	SOUTH AFRICA	1983-94	ANC, PAC, Azapo
FRANCE	1945-54	Vietnam	SPAIN	1968-	ETA
FRANCE	1947-48	Madagascar	SRI LANKA	1971-71	JVP
FRANCE	1952-54	Tunisia	SRI LANKA	1983-	LTTE, etc.
FRANCE	1953-56	Morocco	SUDAN	1963-72	Anya Nya
FRANCE	1954-61	Algeria	SUDAN	1983-	SPLA, etc.
FRANCE	1955-60	Cameroon	TAJIKISTAN	1992-97	UTO
GEORGIA	1992-94	Abkhazia	TURKEY	1977-80	Militia-ized party politics
GREECE	1945-49	DSE	TURKEY	1984-99	PKK
GUATEMALA	1954-54	v. Arbenz	UGANDA	1981-87	NRA, etc.
GUATEMALA	1968-96	URNG, various	UGANDA	1993-	LRA, West Nile, etc.
GUINEA BISSAU	1998-	Mil. faction	UK	1950-56	CPM (Emergency)
HAITI	1991-95	Mil. coup	UK	1952-56	Mau Mau
INDIA	1947-49	Kashmir/Pakistan	UK	1969-99	IRA
INDIA	1948-48	Hyderabad	VIETNAM, S.	1960-65	NLF
INDIA	1952-	N.East rebels	YEMEN	1994-94	South Yemen
INDIA	1982-93	Sikhs	YEMEN A. R.	1948-48	Opp. coalition
INDIA	1989-	Kashmir	YEMEN A. R.	1962-69	Royalists
INDONESIA	1950-50	Rep. S. Moluccas	YEMEN P. R.	1986-87	Faction of Socialist Party
INDONESIA	1953-53	Darul Islam	YUGOSLAVIA	1991-91	Croatia/Krajina
INDONESIA	1958-60	Darul Islam, PRRI, Permesta	ZIMBABWE	1972-79	ZANU, ZAPU

Figure 1

