Insurgency and Credible Commitment in Autocracies and Democracies

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The inability of political actors to make credible promises to broad segments of society—a previously unexplored determinant of civil war—causes both elected and unelected governments to pursue public policies that leave citizens worse off and more prone to revolt. Noncredible political actors are also less able to build counterinsurgency capacity. Popular dissatisfaction with rulers reduces the costs to counterinsurgents of overthrowing regimes, discouraging rulers from building counterinsurgency capacity in the first place; lack of credibility prevents rulers from writing contracts with counterinsurgents that maximize counterinsurgency effort. Empirical tests across numerous subsamples using various measures of political credibility support the conclusion that broad political credibility ranks at least as high as social fractionalization and natural resource rents as a cause of conflict. JEL Codes: D73, D74

The literature on the determinants of civil war has explored three reasons why normal politics give way to armed conflict: resource endowments raise the stakes of violent conflict and allow its financing; governments are unable to organize a response to rebellion, because of, among other reasons, country characteristics (such as mountains) that facilitate rebel activity; and social conditions, particularly ethnic fragmentation, increase popular support for insurgency or at least acquiescence to it. This article argues for a fourth possible explanation: in conflict-prone countries, "normal politics" are undermined by the inability of politicians to make credible promises to large segments of the citizenry.

The lack of political credibility increases the risk of conflict in two ways. First, in both democratic and nondemocratic countries weakly credible leaders have incentives to make policies only in the interests of the few groups that believe their promises. In this setting public goods are underprovided, and

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private good provision and corruption swell. In this distorted policy environment citizens are less likely to resist efforts, including efforts by potential insurgents, to replace the incumbent.

Second, noncredible leaders are less able than credible leaders to build counterinsurgency capacity. On the one hand, capable counterinsurgents always have the choice of overthrowing their employer. Like insurgents, their costs of doing so are lower when the government is unpopular. Noncredible governments must therefore pay more to dissuade counterinsurgents from undertaking coups. On the other hand, optimal compensation contracts for counterinsurgents entail future payments contingent on success. However, leaders who are noncredible to citizens broadly are less able to make the credible commitments to counterinsurgents that are essential to these contracts. Reduced counterinsurgency capacity and citizen support make noncredible governments more vulnerable to insurgency.

Sections I and II examine how political credibility reduces popular support for insurgencies in democracies and nondemocracies and increases the government's ability to mount a counterinsurgency capacity. The rest of the article tests the proposition that conflict is more likely where politicians are less credible, using three different proxies for political credibility. Two reflect political party characteristics: whether parties are programmatic or institutionalized. The third is continuous years of competitive elections. All significantly affect the risk of conflict. The evidence suggests that credibility differences distinguish conflict from nonconflict countries at least as definitively as determinants of conflict ranging from oil to ethnic fractionalization.

I. Political Credibility and Popular Support for Insurgency

The literature points to a significant role for credibility in starting and ending civil wars. Garfinkel and Skaperdas (2000) argue that political competitors resist peaceful redistribution because they cannot credibly refrain from using the benefits of redistribution to arm themselves in the future. Fearon (2004) explains that some types of interstate conflict are difficult to end because the prospect of significant and exogenous variation in the relative strength of contending states makes self-enforcing peace agreements difficult to create. In fact, as Walter (1997) reports, only 20 percent of civil wars have ended by agreement, and most of them relied on third-party enforcement.

This article focuses on a different aspect of credibility, one that is common in discussions of political competition more generally but less common in the literature on conflict: whether leaders can make credible promises to large numbers of citizens. Lack of credibility, by inducing governments to pursue policies that give privileged access to government resources to some while doing little for the vast majority of citizens, reduces citizen support for incumbents and encourages insurgency. While the conclusion is the same in both

democracies and nondemocracies, the underlying logic differs somewhat. The two cases are therefore treated separately.

Credibility and Popular Support for Insurgencies in Democracies

In democracies, if challengers cannot make credible promises, citizens have no reason to believe that they will enact different policies or perform better than poorly performing incumbents. Knowing this, incumbents ignore citizen welfare and throw themselves into rent-seeking. Ferejohn (1986) notes, however, that in real-world democracies, even where credibility appears to be absent, citizens are nevertheless able to hold politicians accountable. He attributes this to voter coordination on a performance threshold: if incumbents meet this threshold, they are re-elected; if they do not, they are thrown out of office. This type of postelectoral accountability mechanism exerts a relatively weak influence on government performance. Persson and Tabellini (2000) extend this logic to government decisions regarding the provision of private and public goods and rent-seeking.

Keefer and Vlaicu (forthcoming) relax the assumption that credibility is an exogenous characteristic of countries and allow politicians either to make promises with the assistance of patrons (who in turn appeal to their clients) or, at some cost, to make promises directly to some fraction of society. When the costs of appealing directly to some fraction of society are sufficiently high relative to the costs of employing patrons, credible promises can be made to only a few. This condition yields predictions consistent with actual policy choices in less credible states: policymaking is targeted to those few who believe political promises, leading to high levels of targeted or private spending, lower public goods provision, and greater rent-seeking by politicians. As welfare falls or inequity in the provision of public services increases, the threat of insurgency rises correspondingly.

For several reasons, politicians are less likely to be credible in younger than older democracies. Keefer (2007) finds broad evidence that the policy choices of young democracies exhibit precisely this policy pattern. This evidence is robust to numerous other explanations, ranging from per capita income to ethnic fragmentation to the formal political institutions that young democracies adopt. The number of years of competitive elections is therefore used as one of the proxies for political credibility in the analysis below.

Political parties are key to credibility in democracies. For promises to be credible candidates must bear a loss if they renege on them. That loss could be their personal reputation. However, if they come from a programmatic political party (one with a reputation for pursuing particular policies), the reputational costs of reneging affect not only the candidate but also all party members. Any single candidate who reneges on the policy promises of the party undermines the party's reputation, so that in future elections the party's label (now no longer credible) will contribute nothing to a candidate's electoral support. Party members therefore have strong incentives to police one another.

Programmatic parties are also better able to implement their promises, because they can enforce a higher degree of party discipline: party members lose the electoral benefits of the party label when they are expelled. Keefer (2005) presents evidence that where programmatic parties are absent, public goods provision is lower and private good provision and corruption greater. Two proxies for the extent to which parties are programmatic or the extent to which party members can discipline party leaders are used in the empirical analysis presented below.

Credibility and Popular Support for Insurgencies in Nondemocracies

Credibility is also a significant determinant of regime support in nondemocracies, where most civil wars occur (56 out of 71 since 1975, the period analyzed below). For example, clientelist strategies—the targeting of benefits to particular supporters at the expense of citizens at large—are a staple of decisionmaking in poorly performing nondemocracies (see, for example, Lewis 1998). In addition, like democracies, autocracies vary in their ability to make credible promises to large numbers of citizens. In many circumstances, as Haber, Razo, and Maurer (2003) argue in the case of the Mexican autocrat Porfirio Díaz, autocrats rely on personal relationships or family ties to make credible commitments. Their ability to make credible commitments only to a narrow slice of society correspondingly reduces the quality of public policies that benefit the vast majority, increasing incentives to mount insurgencies (such as the Mexican Revolution of 1910).

Autocrats can also use political parties to broaden their credibility. As in democracies, one strategy is to construct a ruling party with a strong ideological identity, one reflected in autocrat policies. Communist parties are prototypical examples of such a strategy, in which insurgency is deterred to the extent that insurgents cannot make similarly credible policy promises.

Another strategy is to create ruling party institutions that raise the costs to the autocrat of reneging on promises to the supporters of the ruling party. Ruling party institutionalization can take many forms. For example, autocrats can share power within the ruling party, creating intraparty political checks and balances that reduce the scope for arbitrary treatment of party members by leaders. Following the Glorious Revolution of 1688, for example, the British Crown acceded to institutional arrangements (a strengthened Parliament) that tied its hands; Parliament represented a tiny fraction of the British population but a considerably larger fraction of its moneyed citizens (North and Weingast 1989). Investment opportunities for the members of Parliament and employment opportunities for the rest of society opened up, reducing the risk of revolt.

Gehlbach and Keefer (2006) analyze a second type of ruling party institutionalization, the decision of leaders to allow more supporters into the ruling party, where they can exchange information and coordinate more freely (to overthrow the leader, for example) than nonparty members (in response to

leader expropriation, for example). When the ruling party is larger, welfare is higher, both because party members are more numerous and richer and because they employ more nonparty members. Insurgency is correspondingly less likely.

China illustrates the key role of ruling party institutionalization. It moved from a system in which the top leadership's capacity for arbitrary decisionmaking (under Mao) was nearly unfettered to one in which a number of intraparty institutional arrangements place checks on arbitrary leader behavior. These include rules governing intraleadership decisionmaking (checks and balances at the top of the party) and expensive and elaborate personnel evaluation schemes that may have facilitated coordination among party members. Such institutional arrangements increased the credibility of leadership promises to millions of Communist Party cadres, who responded to economic liberalization with massive investments, convinced by the new intraparty institutional arrangements that Beijing would not expropriate the proceeds of those investments (Keefer 2006).

Despite their usefulness, institutionalized ruling parties are uncommon in nondemocracies. Institutionalization require leaders to surrender authority and, therefore, rents. The autocratic leader who decides to share power with a junta of nine other autocrats could see his rent share drop 90 percent. In countries with large natural resource deposits the rents from faster growth, reduced insurgency threats, and enhanced counterinsurgency capacity may be lower than the rents lost from sharing. The empirical analysis below uses the two political party variables to examine whether there is an association between insurgency and the institutionalization of ruling parties in autocracies.

II. Political Credibility and the Construction of Counterinsurgency Capacity

Lack of credibility increases the probability of insurgency by raising the costs of building counterinsurgency capacity, in two ways. First, the low popularity of noncredible rulers not only reduces the likelihood of popular resistance to insurgency, but it also reduces the likelihood of resistance to coups by counterinsurgents. Second, while rulers would prefer to pay counterinsurgents contingent on the success of their efforts, they cannot do so if they are not credible.

With respect to the first issue, citizens, who are less happy under noncredible leaders are also less likely to oppose coups by counterinsurgents against noncredible leaders. In response, rulers can raise the share of rents they offer to counterinsurgents, reducing counterinsurgency incentives to overthrow the ruler. This is an expensive solution: the rent share necessary to make it succeed may far exceed the effort counterinsurgents must invest to put down rebellion. In fact, military spending is about 2 percentage points of GDP

higher in nondemocracies that lack a programmatic ruling party than in those that have one.[1] These costs are a substantial disincentive to forming a large and capable counterinsurgency force, encouraging insurgency.

Rulers can also organize counterinsurgency forces in a way that makes it more difficult for the forces to launch coups. They can increase the coordination costs of counterinsurgents by creating separate and competing counterinsurgency units, rotating unit leaders frequently, or putting family members in command of the units. Haber (2006) makes a similar argument in the context of dictators who would like to prevent their supporters from overthrowing them, but would still like to make credible promises to them. Unfortunately, this strategy weakens counterinsurgency capacity, because uncoordinated security forces are less able to fend off insurgencies, and uncles and brothers may be incompetent.

Credible rulers are also better able to make credible offers of future compensation to counterinsurgents. The link between the credibility of ruler promises to citizens broadly and to counterinsurgents specifically is straightforward. If credible rulers fail to compensate counterinsurgency forces as agreed, public security declines, as security forces react to nonpayment by quitting, striking, or using their arms against citizens. At the same time, the tenure in office of broadly credible politicians is more likely to depend on their ability to provide the public good of law and order. They therefore lose credibility with citizens and shorten their tenure in office when they fail to honor commitments to security forces, increasing the costs to them of reneging on agreements with security forces.

The inability to make credible promises of future compensation undermines counterinsurgency capacity because leaders cannot easily observe counterinsurgency effort. They would therefore prefer to offer success-based contracts to counterinsurgents, with some payments contingent on defeating the insurgency. If counterinsurgents believe rulers will renege on compensation promises should they succeed, however, they will demand payment upfront, weakening their incentive to exert effort against insurgents. Broadly noncredible rulers can build their counterinsurgency capacity around individuals who believe their promises (for example, family members). These players may not be the most capable, however, making the counterinsurgency less effective than it otherwise would be.

In addition, the current costs of fighting an insurgency may exceed the current ruler's resources, including those from borrowing, so that a successful counterinsurgency effort can be mounted only if promises of future compensation are possible. Assume that governments cannot borrow and that with a total effort by counterinsurgents of E (expressed in units of rents), an insurgency can be defeated, extending the expected duration of the government to

1. It is possible that these countries confront higher risks of international military conflict; of course, that risk may itself be endogenous to the political dynamics inside the country.

T years. The expected value of rents that the government receives from each additional year of tenure is given by r; total rents are given by $R = rT$. If ruler promises are credible, counterinsurgents will accept future rewards for current effort and the government can expend up to R to defeat the insurgency, prevailing if $R > E$. If these assurances are not credible, the government can defeat the insurgency only if $r > E$.[2]

III. Country Examples

Examples of specific conflicts support the argument that the lack of political credibility makes conflict more likely. The insurgencies reviewed here all occurred when governments could not make broadly credible promises and pursued policies consistent with their limited credibility. In cases in which civil wars ended with peace accords, as opposed to total victory, a key attribute of the accords was the enforceability of policies that broadly credible politicians would, in any case, have had incentives to pursue (such as broadly redistributive policies). The cases here also underline the close connection between the lack of broad political credibility and ethnically driven conflict.

Guatemala

The redistributive demands of rebels during the Guatemalan civil war were precisely those that credible governments in poor democracies with significant income inequality would have been driven to provide naturally, even in the absence of rebellion. These policies did not emerge, however, because pre-civil war governments could not make broadly credible promises. In 1995, the year before the final peace accord was signed, taxes in Guatemala amounted to only 8 percent of GDP, the lowest in Latin America. From 1975 to 1995, according to party data in the Database of Political Institutions (Beck and others 2001), Guatemala had no broadly credible left-wing party. Rebellion driven by redistributive demands was therefore more likely.

The Guatemalan case also illustrates the importance of the reputational effects of continuous competitive elections. Negotiations to end more than 30 years of conflict began when the first democratic government of Vinicio Cerezo took office in early 1986. They resumed in 1991, under the (elected) government of Jorge Serrano; continued under his elected successor, Ramiro de Leon; and concluded under the presidency of Alvaro Arzu in 1996 with the signing of the last 7 of the 12 accords that constituted the agreement to end the conflict (Rodas-Martini 2007).

One explanation for the extended process was insurgent doubt over whether government commitments reached in the peace accords were credible. Insurgents would have been more reluctant to trust elected governments in new

2. Rulers also compensate counterinsurgents extracontractually, by providing them opportunities for looting or drug-dealing, for example. These strategies effectively reduce R, however.

democracies, because they were unlikely to be broadly credible. This in turn meant that they had weaker political incentives to engage in broad-based redistribution. Agreement was reached when it was more credible to guerrillas that governments saw it in their own interests to increase taxes and spending. This in turn was more likely in a democracy that had seen 10 years of continuous competitive elections and several changes of president than it had been earlier, when democratic institutions were newer or nonexistent.[3]

Uganda

Uganda also suffered from a decades-long internal conflict, one with a strong ethnic character. Most observers blame ethnic conflict on the governing practices of the British during the colonial period (Ndikumana and Nannyonjo 2007). The British strongly favored the Baganda, in the south, at the expense of other groups, especially those in the north. When the Baganda became restive, the British began to arm northern groups, establishing a rough balance between the economic power of the Baganda in the south and the military power of the Acholi and Langi in the north. These practices meant that, postindependence, leaders from the Acholi or Langi could not easily make credible promises to the Baganda and instead focused on their own ethnic groups at the expense of others. The lack of cross-ethnic political credibility, combined with the imbalance of military forces, made conflict more likely.

The first postindependence regime of Milton Obote, a Langi, was rooted in the northern ethnic communities of the Acholis and Langis. Obote immediately began to discriminate against the Buganda region, to increase northern domination of the military, and to cement his regime with repression (Ndikumana and Nannyonjo 2007). In 1971 Idi Amin, from the Kakwa community, overthrew Obote and targeted not only the Baganda but also the Acholi and Langi, as well as foreign-owned businesses, whose assets he redistributed among his closest supporters. Yet another group overthrew Amin in 1979; it was replaced in 1980 by Obote, who pursued repressive policies that further widened the gulf between the Langi and other ethnic groups, including the Acholi. An Acholi replaced Obote in 1985, but in 1986 Yoweri Museveni, with support rooted in the Buganda and some western communities, defeated Obote's army. Immediately, though, conflict resumed in the north between Acholi-dominated groups and the Langi and between the Acholi groups and the Museveni government (which has included the president's brother as defense minister).

The usual lesson drawn from the Uganda experience is that ethnic conflict emerges when armed ethnic groups cannot make credible promises not to attack one another. The argument here suggests two further lessons. First,

3. By 2004 the Guatemalan government had not honored the letter of the peace accords (calling for an increase in the tax ratio to 12 percent of GDP). However, after oscillating between about 6.5 and 8.0 percent of GDP from 1990 to 1995, the gross tax burden rose 35 percent from 1995 to 2004, rising continuously from 8.0 percent to 10.8 percent of GDP.

ethnic conflict is also more likely when ethnic groups simply cannot make credible promises to make policy decisions that are broadly beneficial. Second, conflict is more likely when political leaders cannot make broadly credible promises even within their own ethnic groups. In Uganda political incentives were to provide targeted payoffs to narrow segments of political leaders' ethnic constituencies, paid for by predation on other constituencies. Leaders could still retain the support of their ethnic constituency since even the members of the constituency who did not benefit—the majority—could see that their own leaders were their only defense against predation by other groups. Unfortunately, the net result of such a political equilibrium is enhanced risk of conflict.

Lebanon

The fragility of interethnic agreements and the particularistic nature of intraethnic political promises also characterize conflict in Lebanon (Makdisi and Sadaka 2005). Until 1975 Lebanon had been governed by a coalition of Maronite Christians, Shia Muslims, and Sunni Muslims. This equilibrium was disrupted by the emergence of Palestinian organizations and disaffected, non-sectarian Muslims. They undermined the balance of power among the sectarian groups that had allowed the coalition arrangements to be self-enforcing, precipitating violence.

Why was the equilibrium sufficiently vulnerable to shocks as to precipitate conflict? One answer appears to be the large mass of disaffected Muslims. Their dissatisfaction was a natural consequence of sectarian leaders who relied for support not on the promises of broad policies and public goods that would improve the welfare of all members of their respective groups but rather on promises of particularistic policy benefits to some members of their groups. Appointments to most, if not all, public administration positions were subject to sectarian considerations, with party leaders having a large say in which members of their sectarian group would receive jobs. Parliamentary seats were also distributed to communities according to sectarian formulas. Not surprisingly, there was little political support for the public good of a well-performing public administration, which was instead characterized by corruption and laxity (Makdisi and Sadaka 2005).

Macedonia

In Macedonia, as in Lebanon, politics was organized around ethnic appeals and political parties that depended on particularistic policies and charismatic leaders (Lund 2005). Why did the lack of political credibility not precipitate civil war? Lund suggests two reasons, both absent in Uganda and Lebanon. First, multiple parties competed for support within each major ethnic group. This increased political pressures on the parties not to concentrate the benefits of clientelism on narrow groups within their ethnic communities, which reduces popular dissatisfaction with incumbent governments. Second, a broad

international presence provided large rents that were contingent on continued peace. "The key to obtaining political support was patronage, the key to patronage was international assistance, the key to assistance was international recognition, and the key to recognition was responsiveness to international norms" (Lund 2005, p. 237). Such assistance, which was relatively minor in Lebanon and was not provided to Uganda, reduced the relative payoffs for any ethnic group of capturing the Macedonian state through conflict.

IV. Specification and Data

More systematic evidence also points to the importance of credibility. The proposition examined here is that the lack of broad political credibility increases the risk of insurgency, whether because it reduces popular resistance to insurgency or because it makes the development of effective counterinsurgency capacity more difficult. More continuous years of competitive elections (in democracies) and institutionalized or programmatic political parties (in both democracies and autocracies) endow rulers with credibility and reduce the risk of insurgency.

To test these propositions, the empirical work presented below follows the literature (for example, Fearon and Laitin 2003), estimating the following logistic specification:

$$(\text{Onset of Conflict})_{it} = f(\text{political variables, income/capita, ln(population), ln(mountainous), oil, ethnic fractionalization, religious fractionalization, and the noncontiguity of state territory})_{i,t-1} + \varepsilon_{it}.$$

The conflict data come from the correlates of war data base (Sambanis 2004) and, like the nonpolitical control variables, are used in Fearon and Laitin (2003). A country is coded as experiencing a civil war when three conditions are met: the conflict caused more than 1,000 deaths, it threatened the sovereignty of the state, and rebel opposition to the state was militarily organized. The control variables are taken from Fearon and Laitin because they went to great lengths to supplement the usual sources of data, which are often missing in poor, conflict-torn countries. Population and geographical variables capture the ease of mounting an insurgency; oil is a dummy variable (0–1) constructed by Fearon and Laitin indicating whether a country is a significant oil exporter; and fractionalization variables proxy for social polarization.[4] To capture geographic features of countries that affect the cost of insurgency, Fearon and Laitin control for how mountainous countries are (the underlying index ranges from 0 to 94.3, where Bahrain is among the countries at 0 and Bhutan is 94.3)

4. Fractionalization variables also enter in other ways in the literature to better capture polarization, as defined axiomatically by Esteban and Ray (1994): quadratically, in Keefer and Knack (2002), or transformed according to a formula that links fractionalization more precisely to their formalization of polarization, in Reynal-Querol (2002). The focus here, however, is on the political variables; the linear specification was therefore retained.

and include a dummy variable indicating whether territory is contiguous (24 countries are noncontiguous).

Fearon and Laitin constructed the per capita income variable based on Penn World Tables 5, 6 (covering the years 1950–92) data on real per capita income (chain method), measured in 1985 U.S. dollars. They extrapolated from 1950–99 using per capita income growth rates from *World Development Indicators 2001*. For countries for which they had at least 11 observations, they added additional observations based on regressions of income on year and energy consumption. For the period 1975–99, their data set includes income data on many more poor countries than the *World Development Indicators* does.[5] This allows 13 more conflicts (out of a total of 73) to be analyzed than is possible using *World Development Indicators* data.

The three political measures used in the analysis to capture various notions of credibility are new to the conflict literature. The first, following Keefer (2007), is the number of years of continuous competitive elections. Consistent with the assumption that these are associated with broad political credibility, the lower is this variable, the lower is public goods provision (such as secondary school enrollment, bureaucratic quality, and public information flows) and the greater are corruption and private good provision (government jobs and public investment). The elections variable comes from the Database of Political Institutions (DPI) (Beck and others 2001) for the period 1975–2004. This variable is zero in countries in which leaders were not elected in competitive elections (elections in which multiple candidates compete in legislative and executive elections and none receives more than 75 percent of the vote, as judged by the Legislative and Executive Index of Electoral Competitiveness from the DPI) and one otherwise.

Two political party variables are also used, both derived from the DPI. One is a dichotomous variable indicating whether the main governing party is programmatic. The DPI indicates whether parties can be judged as right, left, or center. If they can be, this is an indication that they can make more programmatic appeals to citizens. The DPI assessment of this variable is based on sparse information. However, it is highly correlated with assessments in detailed regional studies of whether parties are programmatic (Kitschelt and others 1999 in Eastern Europe; Jones 2005 in Latin America).

The other party variable is the number of years by which the age of the political party of the country's leader exceeds the years that the leader has been in power. When this number is low or negative, it is more likely that the party is constructed around the personality of the leader. In this case, it is only the reputation of the leader that is hurt when party members fail to pursue the leader's program. When, however, the number is positive and high, the party is more

5. For 1976 Fearon and Laitin (2003) have data on 32 countries that are missing in the *World Development Indicators 2007* (World Bank 2007). The average income of these countries is about 40 percent less than the average income of the countries for which data from both sources are available.

likely to have an existence independent of the leader, and party members have greater incentives to develop and preserve the party's programmatic reputation. The age variable is also likely to be associated with the extent to which rulers confront intraparty checks and balances, which is smaller when the party is younger than the ruler's tenure.

These political variables offer more nuances to the description of pre–civil war politics than is typically the case in the conflict literature. The literature generally characterizes countries using the degree of democratization, as measured broadly by the 20-point Polity democracy-autocracy measure (Jaggers and Gurr 1995). Although Polity covers more years than any other political dataset, it does not include data on political parties, an essential element of the analysis here. In addition, the Polity democracy-autocracy measure is more difficult to interpret than the DPI variables. The Polity democracy measure is a composite of other Polity variables, each assigned a weight and then summed to yield the final democracy score. By construction, similar democracy measures over time can be associated with significantly different configurations of the component indicators, as long as they add up to the same final number. Similarly, quite different models for constraining executives or electing leaders could drive similar values of the component variables that make up the democracy measure.[6]

Political credibility is significantly related to many of the control variables that are common in the conflict literature. Political strategies to make clientelist rather than broad programmatic promises, for example, are more likely in poor countries; ethnic appeals are more likely when politicians are not broadly credible.[7] Inclusion of such controls as income and social cleavage therefore biases the estimations presented below against finding a significant role for political variables. This issue is discussed more extensively below.

V. Does Political Credibility Reduce the Risk of Conflict?

The proposition that political credibility reduces the probability of civil war is tested in four samples of countries: all countries, all poor countries, all nondemocracies, and all poor nondemocracies. All of the political variables are significant in each of the three specifications in the sample of all countries

6. At the same time, some values of the Polity measure are, by construction, endogenous to insurgency. For example, Hegre and others (2001) and other researchers find that countries with intermediate values of the Polity measure are most vulnerable to civil war. Their operationalization of the Polity measure is common in the literature. However, countries can fall into the intermediate category, for several reasons. One is the violent overthrow of the regime, which leads Polity evaluators to downgrade the country with respect to the institutionalization of political competition (PARREG), one of the elements of the Polity democracy index.

7. This is the case, for example, to the extent that members of the same ethnic group believe that candidates' individual welfare is tied to the welfare of their ethnic group or that candidates have had more repeated interactions with members of their ethnic group than with others.

TABLE 1. Credibility Effects in the Whole Sample (Dependent Variable: Conflict Onset, 0–1)

Variable	(1)	(2)	(3)
Age of largest party minus leader years in office	0.987 (0.04)		
Years of continuous competitive elections		0.959 (0.02)	
Largest government party programmatic? (1–0)			0.507 (0.05)
Ln (population)	1.186 (0.06)	1.145 (0.12)	1.175 (0.07)
Oil exporter dummy variable (0–1)	1.797 (0.04)	1.466 (0.15)	1.387 (0.31)
Ethnic fractionalization	3.036 (0.05)	2.890 (0.07)	3.486 (0.03)
Religious fractionalization	4.174 (0.05)	4.074 (0.04)	3.301 (0.06)
N (country years)	2,603	2,822	2,847
Log-likelihood	−272.233	−298.801	−310.844
Pseudo *R*-squared	0.059	0.057	0.054

Note: Logistic estimation with clustered standard errors. All time-varying independent variables are lagged one year. Odds ratios are reported with *p*-values based on robust, clustered, standard errors in parentheses: coefficient values below one indicate that the odds of conflict fall with increases in the corresponding variable. The *p*-values for years and party age are calculated assuming that the interaction term is zero. All variables are from the year before the crisis. Other independent variables (noncontiguous state territory, mountainous terrain) and the constant are not reported.

Source: Political variables, Database of Political Institutions (Beck and others 2001); conflict variables, Correlates of War (Sambanis 2004); control variables, (Fearon and Laitin 2003).

(table 1). Their effects are also economically meaningful. To show this more clearly, the table reports the odds ratios, constant across parameter values, rather than coefficient values. Odds ratios less than one imply that an increase in the variable reduces the risk of insurgency; odds ratios greater than one imply that increases raise the risk of insurgency.[8]

The presence of a programmatic governing party reduces the odds of conflict by half. Every additional year by which the age of the governing party exceeds the years a ruler has been in office reduces the odds of conflict by about 2 percent a year. The regressions in the all-country sample omit per capita income. In the presence of per capita income none of the political variables is significant, but this is almost surely because of the effect of income on political incentives to invest in the credibility of broadly programmatic promises, as discussed at length in the next section.

Among other determinants of insurgency, oil is statistically significant in only one regression; religious and ethnic fractionalization are significant determinants of insurgency in all cases. An increase in religious and ethnic fractionalization from the minimum possible value (zero) to the maximum (one)

8. The odds ratio reveals how the odds of conflict change after a one-unit increase in the independent variable. If the odds before the change in a variable are 3:1 and the odds after are 4:1, the odds ratio is 1.33. The marginal effects of coefficient changes estimated from logistic specifications depends on parameter values.

TABLE 2. Credibility Effects in Poor Countries (Dependent Variable: Conflict Onset, 0–1)

Variable	(1)	(2)	(3)
Age of largest party minus leader years in office	0.982 (0.04)		
Years of continuous competitive elections		0.968 (0.10)	
Largest government party programmatic? (1–0)			0.572 (0.11)
Income per capita	0.597 (0.14)	0.631 (0.16)	0.653 (0.20)
Ln (population)	1.178 (0.07)	1.135 (0.21)	1.155 (0.10)
Oil exporter dummy variable (0–1)	1.558 (0.21)	1.266 (0.55)	1.152 (0.72)
Ethnic fractionalization	0.687 (0.54)	0.967 (0.96)	0.926 (0.89)
Religious fractionalization	2.741 (0.20)	2.33 (0.29)	2.961 (0.14)
N (country years)	1,272	1,353	1,377
Log-likelihood	−185.416	−203.590	−211.887
Pseudo *R*-squared	0.047	0.034	0.043

Note: Logistic estimation with clustered standard errors. All time-varying independent variables are lagged one year. Odds ratios are reported with *p*-values based on robust, clustered, standard errors in parentheses: coefficient values below one indicate that the odds of conflict fall with increases in the corresponding variable. The *p*-values for years and party age were calculated assuming that the interaction term is zero. All variables are for the year before the crisis. Other independent variables (noncontiguous state territory, mountainous terrain) and the constant are not reported. Poor countries in year *t* are those with incomes below the world median that year.

Source: Political variables, Database of Political Institutions (Beck and others 2001); conflict variables, Correlates of War (Sambanis 2004); control variables, (Fearon and Laitin 2003).

increases the odds of conflict by 300–400 percent. Consistent with the argument that fractionalization is associated with conflict precisely when politicians are not credible and that large natural resource rents create a disincentive for politicians to invest in broad credibility, the omission of any of these variables substantially boosts the significance of the two party variables. Population size is also significant: countries with larger populations have a greater likelihood of experiencing conflict in this specification.[9]

The vast majority of civil wars occur in countries that are poorer than the world median in the year they occur. In the period 1975–2000, 57 conflicts broke out in countries with per capita income at or less than the world median. Of the remaining 15 countries in which conflict occurred, 9 had no income data. Of these, at least six (Afghanistan, Azerbaijan, Georgia, Liberia, Moldova, and Yemen) were near or below the median the year before war broke out. Examining poorer countries separately is therefore a sensible estimation strategy, unless one has strong prior reasons to believe that the dynamics of civil war are on average the same in poor and rich countries. Given that citizen income affects political incentives to invest in credible,

9. The population result is common. Collier and Hoeffler (1998) attribute it to the increased desire for secession in larger countries. Fearon and Laitin (2003) point to the greater difficulties that larger populations create for control by the state. The argument in this article suggests a third explanation: the costs to politicians of making credible appeals to citizens rises with population size.

TABLE 3. Credibility Effects in Nondemocracies (Dependent Variable: Conflict Onset, 0–1)

	All nondemocracies		Poor nondemocracies	
Variable	(1)	(2)	(3)	(4)
Age of largest party minus leader years in office)	0.990 (0.35)		0.980 (0.09)	
Largest government party programmatic? (1–0)		0.469 (0.05)		0.407 (0.04)
Income per capita			0.597 (0.20)	0.680 (0.32)
Ln (population)	1.277 (0.06)	1.250 (0.04)	1.298 (0.09)	1.224 (0.15)
Oil exporter dummy variable (0–1)	1.314 (0.41)	1.080 (0.82)	1.049 (0.94)	0.883 (0.83)
Ethnic fractionalization	1.519 (0.51)	1.520 (0.52)	0.536 (0.38)	0.656 (0.50)
Religious fractionalization	3.623 (0.16)	2.820 (0.20)	2.399 (0.41)	2.093 (0.45)
N (country years)	1,553	1,678	1,019	1,058
Log-likelihood	−202.658	−215.868	−149.379	−156.230
Pseudo *R*-squared	0.038	0.040	0.041	0.046

Note: Logistic estimation with clustered standard errors. All time-varying independent variables are lagged one year. Odds ratios are reported with *p*-values based on robust, clustered, standard errors in parentheses: coefficient values below one indicate that the odds of conflict fall with increases in the corresponding variable. The *p*-values for years and party age were calculated assuming that the interaction term is zero. All variables are for the year before the crisis. Other independent variables (noncontiguous state territory, mountainous terrain) and the constant are not reported. Poor countries in year t are those with incomes below the world median that year.

Source: Political variables, Database of Political Institutions (Beck and others 2001); conflict variables, Correlates of War (Sambanis 2004); control variables, (Fearon and Laitin 2003).

programmatic promises to citizens, there is ample reason to suspect that the opposite is true.

Unlike the specifications in table 1, the poor-country specifications include per capita income (table 2); the political results strengthen if income is excluded, however. Despite the presence of per capita income, the relevant political variables are highly significant and the odds ratios are little changed from the all-country results. Equally important, however, is that the significance of other commonly investigated correlates of rebellion changes substantially in poorer countries. Though income is significant in the sample of all countries (not reported), it is insignificant in the sample of poor countries. It appears that income effects are driven by the complete absence of conflicts among the richest 30 percent of countries. The effects of oil exports and ethnic and religious fractionalization all fall significantly compared with the results for all countries. Even if income is excluded, ethnic fractionalization and oil exports remain insignificant. The insignificance of ethnic and religious fractionalization is not driven by their simultaneous entry into the specifications; although they are correlated at 0.32, neither becomes significant when the other is dropped.

The hypotheses related to the institutionalization of political parties are relevant to both democracies and nondemocracies; nondemocracies are therefore

also examined separately. Countries are identified using two DPI variables, the legislative and executive indices of electoral competition. Countries in which each of these indices takes a maximum score of seven (multiple parties compete in the elections and none receives more than 75 percent of the vote) are classified as democracies; the remainder are classified as nondemocracies.

Two sets of regressions are presented in table 3: those looking only at nondemocracies, using the specifications of table 1, and those looking only at poor nondemocracies, using the specifications of table 2 (the years of continuing competitive elections are omitted, because they are always zero in the nondemocratic subsample). The presence of a programmatic governing party has an even stronger effect on outcomes, whether compared with all countries or only poor countries (the presence of a programmatic party reduces the odds of conflict more than in tables 1 and 2). The party age variable also has a stronger effect on poor nondemocracies than it does on poor countries only, although the party age variable is insignificant in the sample of all nondemocracies.

Other common determinants of conflict are uniformly insignificant across all four nondemocracy regressions. Per capita income is insignificant in the sample of poor nondemocracies. The magnitudes of the effects of oil exports and religious and ethnic fractionalization drop substantially; they are insignificant in both subsamples. Only population, among the alternative explanations of insurgency, continues to matter.

VI. Robustness: Income, Oil, Social Polarization, and Political Credibility

The results in tables 1–3 are striking because they are almost entirely robust to controls for variables that theory predicts should influence or be influenced by political credibility: per capita income, ethnic and religious fractionalization, and oil. Oil is a typical control variable in the conflict literature, because, as Collier and Hoeffler (1998) and Elbadawi and Sambanis (2002) argue, access to natural resource rents is both a goal of rebel movements and a way for rebel movements to finance themselves (Fearon 2005 disputes the importance of natural resources in civil war). However, oil or other natural resource rents also make rulers more reluctant to invest in credibility. For example, when rents are high, the benefits to rulers of adopting institutions that force them to share rents (for example, larger, more-institutionalized ruling parties) are smaller.

Indeed, there is a strong negative correlation between countries coded by Fearon and Laitin (2003) as oil exporters and the variables used here to capture the extent to which politicians can make broadly credible promises to voters. Moore (2004) provides an illustration of the discovery of oil leading to new and more circumscribed institutional arrangements. Before the discovery of oil, Kuwaiti merchants shared political control with the sheiks of the dominant al-Sabah family: "Manpower and financial power gave Kuwaiti

merchants an early sense of equality with the ruling al-Sabahs . . . . Commerce was not viewed as subordinate to politics. Indeed, politics needed commerce" (p. 31). However, with the discovery of oil in 1938, "the elected municipality board, which had served as an *asil* [elite] merchant enclave since 1932, was replaced with an appointed board of shaikhs. As royal family members took control of government ministries, merchant committees within those bodies, designed to provide policy input, were disbanded" (p. 42). Nevertheless, despite the relation between political variables and rents, the estimates of the political variables in tables 1–3 are not sensitive to the presence of the oil control.

Using various measures, researchers (Reynal-Querol 2002; Soysa 2002) have also found that polarization is a determinant of civil war (Fearon and Laitin 2003 argue that this relation is not robust). The argument is that sharp differences in policy preferences across social groupings create social cleavages that increase insurgent pressures. However, as the earlier discussion of several civil wars concludes, social polarization is endogenous to political decisionmaking. In particular, noncredible politicians seek to expand the set of citizens to whom they can make credible commitments. In socially fragmented countries the low-cost way for them to do this may be by making ethnic or religious appeals. Politicians build a reputation for policy stances favoring particular ethnic groups, but in broadening the reach of their credible commitments, they exacerbate political polarization.

Van de Walle (2003) documents this in the case of emerging African democracies, where he finds that the overwhelming organizational principle of new political parties is ethnic or linguistic. Under these conditions, social polarization leads to conflict, because noncredible politicians use social identity to build support. Nevertheless, the presence of controls for the social polarization variables does not affect the credibility results reported earlier.

Per capita income is the most troublesome of the three controls, because none of the results in table 1 is significant when a control for income is added. However, the arguments linking income to clientelist political strategies are well established in the literature. Poorer voters are more susceptible than richer voters to targeted transfers (Dixit and Londregan 1996). The political advantages of relying on clientelist strategies that target the few voters who believe politician promises are therefore greater in poorer countries, reducing political incentives to invest in credibility but also increasing insurgency incentives and giving rise to an association between insurgency and income that runs through political credibility.[10]

10. The fact that the lack of political credibility might be both a cause and consequence of low incomes suggests the possibility of a credibility-based poverty trap. Keefer and Vlaicu (forthcoming) anticipate such a possibility, proving that where the costs to politicians of clientelist electoral strategies are sufficiently low, they will delay the investments needed to make their promises broadly credible.

While it is reasonable to exclude per capita income from the table 1 regressions, given the links with the credibility variables, there is some concern that to the extent that there are additional reasons for an association between income and civil war that do not operate through credibility, the exclusion of income biases the results. The fact that income is insignificant in tables 2 and 3 provides some reason to believe that this bias is small. At the same time, the three reasons in the literature for including income seem less persuasive than the arguments that link income to political credibility. Moreover, two of these reasons have a credibility interpretation that reinforces the argument here for excluding income.

Some authors (including Collier and Hoeffler 1998, Elbadawi and Sambanis 2002, and Fearon and Laitin 2003) argue that income captures state capacity to combat insurgencies. However, state capacity is a public good that broadly credible politicians have a greater incentive to provide. For example, law and order and strong administrative capacity, from which counterinsurgency capacity springs, benefit all citizens. The earlier discussion predicts that such public goods will be underprovided in low-credibility states. In fact, Keefer (2007) presents evidence that both the rule of law and bureaucratic quality are lower in low-credibility democracies.

Moreover, it is not clear whether income is a binding constraint on the development of state capacity in any case. Military spending, for example, seems to bear no systematic negative relation with per capita income; if anything, military spending as a fraction of GDP varies inversely with per capita income. Moreover, countries confronting threats show remarkable agility in increasing military spending independent of income. Despite roughly similar incomes per capita, India, Pakistan, and Sri Lanka exhibit vast differences in military spending: Pakistan spends twice as much as India and Sri Lanka 50 percent more than India, differences that seem more correlated with the fraction of their territory the countries view as vulnerable to armed dispute than with differences in per capita income.[11]

Hegre and others (2001) posit a second channel through which income might drive conflict: as incomes rise, class conflicts moderate and opposing segments of society are more amenable to peaceful resolutions of their disagreements. One indicator of class-based mobilization is the existence of political parties advocating the interests of different classes. The argument here predicts that it is precisely the absence of such parties that makes conflict more likely. The tests below examine this thesis directly, showing that direct measures of programmatic—possibly class-based—parties reduce the probability of insurgency.

Collier and Hoeffler (1998) and Elbadawi and Sambanis (2002) interpret income as the opportunity cost of rebel labor: insurgencies can form at lower

11. India, with per capita purchasing power parity–adjusted income of $2,312 in 1999, is richer than Pakistan ($1,818) but poorer than Sri Lanka ($3,229).

cost in poorer countries. However, the same argument could be made about the ability of governments to recruit soldiers into its counterinsurgency forces, suggesting no clear bias from omitting income from a conflict regression.

VII. Other Robustness Issues

The results in tables 1–3 raise two broad concerns. One is that the specifications do not control for reasonable alternative explanations for conflict; the other, related concern is that the results are the consequence of endogeneity bias, which reflects either reverse causality or omitted-variable bias. These concerns are endemic in estimating the determinants of conflict.[12] Valid instruments are rare and entirely absent for investigations of the political drivers of conflict.

One response to endogeneity bias is to conduct case studies, which may reveal causal mechanisms using information that is not available with aggregate data. The earlier thumbnail sketches of several conflicts illustrate the role of political credibility in conflict. The estimations reported in this section are based on two additional strategies: estimation across different subsamples and estimation with additional control variables. Each captures at least a substantial subset of potential causes of endogeneity bias. The all-country results of table 1 are robust to all alternative estimation strategies; other results are somewhat more fragile, although the smaller sizes of the samples make this unsurprising. In both tables 4 and 5 the estimation of the effects of party age relative to executive tenure in the case of all nondemocracies is omitted: this variable is not significant in the original regression in table 3 or in any of the specifications in tables 4 and 5.

Estimates across Subsamples

Conflicts are rare: the onset of conflict occurs in less than 2 percent of the country-year observations investigated here. As a consequence, subsamples can vary significantly in the number of conflicts they contain, affecting the robustness of results simply because the number of conflicts in the subsample is small. Nevertheless, one might reasonably ask whether the results reported in tables 1–3 are robust to four additional subsamples.

First, the fall of communism might have had a substantial effect on civil war; the end of the Cold War ended proxy wars contested and financed by the two superpowers. Nevertheless, results reported earlier are robust when one uses only observations from the 1990s (table 4, row 1). In only one case is a significant result from the earlier tables insignificant in the 1990s subsample. The magnitude of the effect of years of continuous competitive elections is

12. Conflict almost always shortens the duration of democracies and the lives of parties. The estimations in tables 1–3 avoid this source of endogeneity, however, because all of the explanatory variables are measured in the year before conflict occurs.

TABLE 4. Political Credibility and Conflict Risk across Subsamples (Dependent Variable: Conflict Onset, 0–1)

	All countries			Poor countries			Nondemocracies	Poor nondemocracies	
Subsample	Party age (years in office)	Election years	Programmatic party	Party age (years in office)	Election years	Programmatic party	Programmatic party	Party age (years in office)	Programmatic party
1990s only	0.977 (0.002)	0.79 (0.00)	0.370 (0.06)	0.981 (0.06)	0.916 (0.35)	0.716 (0.54)	0.289 (0.03)	0.980 (0.10)	0.27 (0.02)
Excluding country years with two or more conflicts	0.985 (0.06)	0.964 (0.04)	0.363 (0.005)	0.98 (0.06)	0.993 (0.82)	0.406 (0.03)	0.46 (0.06)	0.978 (0.12)	.303 (0.02)
Cross-section, average 1975–99 (ordinary least squares, not odds ratios)	−0.005 (0.04) $N = 122$, N (conflicts) = 42	−0.011 (0.001) $N = 124$, N (conflicts) = 44	−0.68 (0.004) $N = 125$, N (conflicts) = 45	−0.005 (0.37) $N = 60$, N (conflicts) = 21	0.014 (0.77) $N = 60$, N (conflicts) = 21	−0.74 (0.031) $N = 61$, N (conflicts) = 21	−1.59 (0.001) $N = 35$, N (conflicts) = 13	−0.008 (0.45) $N = 20$, N (conflicts) = 5	−1.97 (0.002) $N = 11$, N (conflicts) = 5
Cross-section, conflicts 1990–99, political variables 1975–90 (ordinary least squares)	−0.002 (0.07) $N = 104$, N (conflicts) = 19	−0.009 (0.001) $N = 124$, N (conflicts) = 26	−0.41 (0.005) $N = 125$, N (conflicts) = 27	−0.003 (0.54) $N = 56$, N (conflicts) = 11	−0.03 (0.12) $N = 60$, N (conflicts) = 14	−0.54 (0.007) $N = 61$, N (conflicts) = 14	−0.60 (0.03) $N = 35$, N (conflicts) = 11	−0.001 (0.80) $N = 18$, N (conflicts) = 3	−1.77 (0.06) $N = 11$, N (conflicts) = 5

Note: Estimated coefficients of respective political variables using specifications from tables 1–3, with change noted at far left. See notes to tables 1–3. N (conflict) indicates number of countries in which conflicts occurred over the period.

Source: Political variables, Database of Political Institutions (Beck and others 2001); conflict variables, Correlates of War (Sambanis 2004); control variables, (Fearon and Laitin 2003).

TABLE 5. Political Credibility and Conflict Risk with Additional Controls (Dependent Variable: Conflict Onset, 0-1)

	All countries			Poor countries			Nondemocracies	Poor nondemocracies	
Control variable	Party age (years in office)	Election years	Programmatic party	Party age (years in office)	Election years	Programmatic party	Programmatic party	Party age (years in office)	Programmatic party
Controlling for year effects	0.986 (0.03)	0.958 (0.02)	0.484 (0.04)	0.978 (0.02)	0.965 (0.08)	0.493 (0.08)	0.493 (0.08)	0.976 (0.04)	0.380 (0.04)
Substituting per capita income from *World Development Indicators*, filling missing values through extrapolation	n.a	n.a	n.a	0.98 (0.02)	0.96 (0.02)	0.55 (0.10)	n.a	0.980 (0.10)	039 (0.09)
Controlling for per capita income growth	0.986 (0.03)	0.954 (0.020)	0.553 (0.10)	0.98 (0.02)	0.967 (0.08)	0.673 (0.24)	0.628 (0.26)	0.983 (0.14)	0.528 (0.17)
Controlling for continent dummies	0.988 (0.07)	0.951 (0.02)	0.502 (0.04)	0.977 (0.02)	0.939 (0.01)	0.574 (0.15)	0.546 (0.11)	0.986 (0.26)	0.478 (0.06)
Controlling for Köppen-Geiger climate zones	0.988 (0.062)	0.96 (0.04)	0.60 (0.11)	0.98 (0.01)	0.96 (0.006)	0.674 (0.238)	0.630 (0.22)	0.980 (0.09)	0.53 (0.17)

n.a Not applicable.

Note: Estimated coefficients of respective political variables using tables 1–3 specifications, with change noted at far left. See notes to tables 1–3. Five Köppen-Geiger variables report the fraction of country covered by each of five climate zones.

Source: Political variables, Database of Political Institutions (Beck and others 2001); conflict variables, Correlates of War (Sambanis 2004); control variables, (Fearon and Laitin 2003); Köppen-Geiger climate variables: Sachs (2003).

TABLE 6. Political Credibility and Political Institutions (Dependent Variable: Conflict Onset, 0–1)

	Table 1: All countries			Table 2: Poor countries		
Formal institutional variables	Party age (years in office)	Election years	Programmatic party	Party age (years in office)	Election years	Programmatic party
Political variables from column heading	0.97 (0.00)	0.95 (0.01)	0.31 (0.01)	0.972 (0.00)	0962 (0.00	0.46 (0.03)
Presidential = 0, semipresidential = 1, parliamentary = 2	1.003 (0.00)	1.002 (0.02)	1.002 (0.04)	1.002 (0.00)	1.003 (0.00)	1.003 (0.00)
District magnitude	1.00 (0.93)	1.00 (0.94)	1.00 (0.86)	1.00 (0.15)	1.00 (0.43)	1.00 (0.43)
Plurality = 1; Proportional representation = 0	1.00 (0.94)	1.00 (0.91)	1.00 (0.86)	1.00 (0.19)	1.00 (0.19)	1.00 (0.25)
Number of observations	1,847	1,972	1,972	795	847	847
R-squared	0.12	0.12	1972, 0.12	0.09	0.10	0.11

Note: Each column reports the results of specifications corresponding to tables 1 and 2, augmented with the formal political institutions. Other table 1–2 control variables are not reported. Institutional variables reduce sample sizes compared with tables 1 and 2, but samples are close to tables 1 and 2 and results are robust if district magnitude and voting rule variables are excluded.

Source: See text.

much greater in the poor country, 1990s-only regressions of table 4 than in the poor-country regressions of table 2. However, their impact on conflict is no longer statistically significant.

For all of the regressions in tables 1–3, clustered standard errors are reported; this reduces the risk that multiple conflict countries drive the significance of the results reported in these tables. This may not eliminate all sources of bias resulting from including country-year observations in countries that experienced two or more conflicts, however. To be sure that this is the case, regressions in tables 1–3 are re-estimated excluding all country-year observations beginning with the onset of a second conflict (table 4, row 2). The earlier results remain largely robust. One of the significant results in table 2 and one in table 3 are insignificant in table 4; however, the insignificant result in table 2 (the effect of programmatic parties) is highly significant in the second row of the poor-country sample in table 4.

Most of the explanatory power of the political variables comes from their cross-country variation.[13] It might be argued, therefore, that the statistical significance of the results in tables 1–3 is exaggerated by the large sample sizes of panel data. The clustered standard errors used in tables 1–3 substantially reduce this downward bias in standard errors. An additional solution to this problem is to examine only the purely cross-section variation in the conflict and control variables (table 4, row 3). The left-hand side variable is therefore the total number of conflicts a country experienced between 1975 and 1999. The control variables are averaged over the period, and coefficients are estimated using simple ordinary least squares. Once again, all of the results from table 1, and five of eight of the significant results in tables 1–3, remain significant. Moreover, one of the insignificant results (in table 2) is significant in table 4.

The row 3 results, using whole-period averages of both the political and conflict variables, could be driven by reverse causality. To determine if this is the case, the estimation is repeated, this time averaging the political variables over the period 1975–90 (row 4). The effects of early political variables are then estimated on the number of conflicts over the period 1990–99 (the other control variables are 1975–99 averages, as in row 3). While the coefficient estimates drop somewhat compared with row 3, the pattern of statistically significant results is the same. This suggests that reverse causality is not driving row 3 results, indicating that the significance of the results in tables 1 is not the spurious result of using the many observations contained in the panel data.

13. More important, it is not clear how tests should be specified over time. Among other things, it is unclear over what time period changes in the political variables should begin to exert an effect on the risk of conflict. Yearly fixed-effects estimations are inappropriate, for example, because early increases in party age or years of continuous elections almost surely do not have the same effects as later increases, while the effect of programmatic parties is unlikely to emerge the year after they are recognized.

Adding New Control Variables

It is also possible that the political variables in table 1–3 are sensitive to the use of particular versions of some control variables (for example, income or social polarization) or some alternative explanations of conflict are omitted. Numerous alternative specifications indicate that this is unlikely to be the case. The first row of table 5 includes controls for year effects: as in the 1990s regressions in table 4, global shocks such as the end of the Cold War could have influenced both political institutions within countries and the risk of conflict. However, all of the significant results and one of the insignificant results in tables 1–3 are significant when year effects are taken into account (row 1).

The per capita income variable taken from Fearon and Laitin (2003) is not adjusted for purchasing power parity. The political variables could therefore be significant because they spuriously capture mismeasured income effects. In fact, this is not the case. *World Development Indicators* does not provide income data of any kind for many countries in the mid-1970s, including a number of conflict countries. To adjust for this, missing values of the available purchasing power parity–adjusted per capita income data were filled in using simple linear extrapolation (this procedure never accounted for more than four years of a country's income data). The results are robust to the use of purchasing power parity–adjusted per capita income (table 5, row 2).

Results are similarly robust when alternative measures of ethnic and religious fractionalization compiled by Alesina and others (2002) are substituted into the regressions of tables 1–3 (results not reported). Specifically, all of the political coefficients in tables 1 and 2 are significant (including one that was previously insignificant). However, only the last specification in table 3 (which looks at the effects of programmatic parties in poor nondemocracies) is significant (results not reported). It is not surprising that the nondemocratic regressions are less robust, because the political variables are less noisy, more-accurate descriptions of political credibility in democracies than in nondemocracies.

Economic growth is a proximate cause of conflict and therefore captures political sources of conflict (such as credibility) that affect both growth and conflict. Despite this, five of the eight significant results in tables 1–3 remain significant when growth is added; as with the fractionalization variables, the table 1 and 2 results are particularly robust (table 5, row 3).

Continent controls (for Africa, East Asia, and South Asia) and climate zone variables (five Köppen-Geiger climate zones, such as whether countries are primarily desert, tropical, dry, or wet) are separately added to the table 1–3 regressions to capture other unobserved influences on conflict. Their inclusion is not precisely a robustness test of the regressions in tables 1–3, however, because theory predicts that their influence should operate in part through the political variables under examination here. For example, political incentives to build broadly credible institutions (for example, programmatic political parties or institutionalized ruling parties in autocracies) are directly related to the growth

payoff that leaders can expect if they encourage investment with such institutions. Unfavorable locations (for example, poor climactic conditions unfavorable to agriculture or continents characterized by adverse neighborhood effects, such as generally thin population densities) have more limited growth potential. The lower the growth payoff to credible institutions, the lower is the increment to regime security that leaders can achieve by adopting these institutions. Under these circumstances one is less likely to see the political arrangements that are the focus of attention here. In fact, continent and climate zone dummy variables are significant determinants of each of the three political variables used here, in the all-country, poor-country, and nondemocracy samples.

Despite the significant, theoretically predicted relation between continent and climate zone dummy variables and the political variables, nearly all of the regressions in tables 1 and 2 are robust to the inclusion of the dummy variables (rows 4 and 5, table 5). Most of the significant nondemocracy results from table 3, which are more vulnerable in any case to the noisiness of the political variables, are insignificant. However, even in these cases, the estimated impact of the political variables remains large. For example, though not statistically significant, the presence of programmatic parties still reduces the odds of conflict in nondemocracies by about 46 percent.

Reynal-Querol (2002) argues that democracies with more inclusive institutions (proportional representation as opposed to majoritarian or presidential systems) confront a lower risk of ethnic civil war. It is possible that the political effects measured here are significant only because they spuriously capture the effect of these formal institutions. However, results are robust to controls for three formal institutional variables: whether countries are presidential, semipresidential, or parliamentary; whether they are majoritarian, as reflected in their district magnitudes (low indicates more majoritarian); and whether they use plurality or majoritarian electoral rules. All of these are available in the DPI.

All of the results in tables 1 and 2 (the only samples containing democracies) are actually stronger when formal institutional variables are taken into account (table 6). At the same time, the estimated effects of these formal institutions on the probability of civil war differ from those found in previous research. Neither district magnitudes nor electoral proportionality are significant determinants of insurgency. Contrary to expectations, the odds of a conflict occurring are significantly higher in parliamentary systems. This result contrasts with predictions in the insurgency literature and is related to an ongoing debate in the literature regarding the stability of presidential and parliamentary systems. One study (Cheibub 2006) also finds that, contrary to received wisdom, presidential systems are not less stable once one controls for whether democracy is preceded by military government.

It might be the case that the political variables simply capture the fact that countries that have been sovereign for a longer period have had more time to develop key political institutions and to work out underlying conflictual issues. *The World Factbook* of the Central Intelligence Agency (2007) has data on

countries' first year of sovereignty. The years since the first year of sovereignty, particularly in log form, are positively correlated with the age of the largest government party and with the years of continuous competitive elections. However, whether in log or linear form, the variable *years of independence* has no effect on the results reported in tables 1–3. In all cases the magnitude and significance of the estimated political effects hardly change. Only the log of years since independence is ever close to significant and then only in the poorer country samples. It turns out that more years of independence is weakly associated with greater odds of experiencing a conflict.

Despite the robustness of the results to a variety of control variables, it is still possible that omitted variables drive the negative correlation of conflict risk and political credibility. One argument made here is that the ability to make credible promises, as captured by the political variables used in the empirical analysis, undermines counterinsurgency capacity and encourages insurgency. It is conceivable, however, that some governments simply enjoy a better counterinsurgency endowment or, more generally, are endowed with unobserved social conditions that lower the risk of conflict. Given the reduced risk of conflict, politicians find it more cost-effective to invest in the ability to make credible promises into the future. To the extent that this is true, results reported here would be the product of reverse causality.

While the empirical analysis does not completely foreclose the possibility of reverse causality, this is not the most plausible explanation for the results here. The row 4 results in table 4—which show that in the cross-section estimations, the 1975–90 values of the political variables are significant predictors of conflict over 1990–99—suggest that any such reverse causality would need to be driven by time-invariant omitted variables. Furthermore, these omitted factors would have to be largely uncorrelated with the control variables examined here, including years of independence, geography, ethnic and religious fractionalization, and per capita income, and strongly related to the political variables that are the object of the analysis.

It is possible that such time-invariant omitted effects exist. However, it is also difficult to identify social conditions that reduce the risk of conflict without also directly affecting political incentives to invest in the ability to make credible commitments. Unobserved ethnic tensions, for example, may make conflict more likely, but they do so in large part by raising the costs to politicians of making credible promises across ethnic groups. The endowment of an effective counterinsurgency force may deter insurgencies from forming, but it is difficult to see how leaders can sustain effectiveness if they are unable to make credible commitments to the force. Indeed, it is more likely that the omitted variables that would have the greatest impact on the analysis here are precisely those that influence political incentives to make credible commitments. The influence of omitted factors such as these, however, is entirely consistent with the argument proposed here.

VIII. Conclusion

The credibility of political promises plays a crucial role in "normal" politics. The evidence presented here shows that it also heavily influences the transition from normal politics to civil war. In both democratic and nondemocratic countries, where political actors are unable to make credible promises to a sufficient fraction of citizens, they pursue public policies that benefit a few at the expense of most citizens, increasing citizen tolerance for insurgent movements. Moreover, the willingness and ability of governments to build effective counterinsurgency capacity declines. As a consequence, political credibility—proxied by the age of the ruling party, whether the ruling party is programmatic, or by the years of continuing competitive elections—significantly reduces the probability of civil war.

These results are relevant to postconflict recovery strategies. Countries that have experienced conflicts are at particular risk of subsequent conflicts; the evidence here suggests that this is related to the effect of past conflicts on the credibility of a country's political competitors. Efforts to build the broad credibility of political actors is therefore likely to be key to successful postconflict recovery. Doing so implies, among other things, ensuring that citizens give governments (rather than donors) credit for improvements in welfare; controlling access to rents to reduce incentives for renewed conflict in noncredible settings; and supporting the informal institutions of politics (political parties and information transmission to voters).

These efforts contrast with other alternative reform priorities that are often voiced. For example, some argue for delivering quick wins in the form of enhanced service delivery; doing so contributes to sustainable recovery, however, only if it builds political reputations for broad public goods provision. Others argue for fine-tuning institutional arrangements that grant contending groups a veto over policymaking, making intergroup agreements credible. These arrangements need to be supplemented by changes that allow the leaders of contending groups to make credible promises to citizens more broadly.

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