



WORLD DEVELOPMENT REPORT 2011

BACKGROUND PAPER

GOVERNANCE AND CIVIL WAR ONSET

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1 Introduction

Since the end of the Cold War, violent political conflict has increasingly been seen as a major obstacle to the economic development of many low and middle-income countries. Despite a significant decrease since the peak in the early 1990s, the prevalence of serious civil conflict remains quite high. About one third of the world's population lives in countries affected by significant political violence, and about 25% of the world's population outside of China and India. Most of these are in low and middle income countries. In addition, very many people live in post-conflict countries that are recovering from major and minor civil wars.

It is obvious that major civil conflict prevents economic development. Indeed, it is obvious that major civil wars can and do destroy economic progress that may have been made in previous years. It is less obvious, however, what causes civil wars and lower levels of violent civil conflict to begin with. Why have some countries been more prone to violent conflict than others, and what explains why conflicts break out when they do?

In this paper I review and extend the findings of a growing, post-Cold War literature on the correlates of civil war that has relied primarily on cross-national statistical analyses. Using a version of the Uppsala Armed Conflict Database's coding of conflicts that distinguishes between high and low intensity conflict, I reproduce a number of the main findings of this literature. For the period since World War II (or since the early 1960s), the countries most prone to major civil war have been distinguished by low income, large populations, mountainous terrain, and possibly by oil production, a high share of "politically excluded" ethnic minorities, and (perhaps) greater gender inequality. Recent independence and recent changes in degree of democracy augur a higher risk of major conflict onset in the next few years. For all conflicts including low intensity conflicts, these same factors are statistically associated with conflict outbreak, as is, additionally, higher levels of ethnic fractionalization. Some factors that show no very consistent relationship with a propensity for violent civil conflict include income inequality (as measured by Gini coefficients) and level of democracy, although there is some indication that countries with "anocracy" (partial democracy) are at greater risk if we consider both low and high intensity conflicts.

As is the case for most of the literature in this area, these findings are based on analysis of a panel of 150 to 180 or so countries, observed anywhere from about 10 to 65 years depending on the time since independence or 1946. Many of the factors mentioned above, such as income, ethnic diversity, and mountainousness, vary little or not at all over time within countries. This means that many of the results are based on comparisons in rates of conflict outbreak across countries, and we may have substantial reason to worry that the observed associations are "spurious correlations" rather than estimates of causal effects. It could be that, for example, that low income does not directly cause higher conflict risk, but happens to be correlated with some third, unmeasured factor that does cause conflict. In some cases, it could be that the mere expectation of conflict for unmeasured reasons influences one of our presumed causal factors, such as income or perceived ethnic diver-

sity, again leading to spurious correlations. So it is important to keep in mind that much caution is called for in interpreting many of the associations reported below.

For a few variables that vary a lot over time within countries, such as recent independence or change in governing arrangements, we can ask whether change in these variables is reliably followed within countries by a higher risk of conflict outbreak (this is called a “fixed effects” analysis). Recent independence, change in governing arrangements, and the onset of partial democracy all predict conflict outbreak in this sense in the data examined here, and so may have a stronger claim on being (or marking) causal effects.

The main novelty of this paper is an examination of the relationship between several indicators of *the quality of country governance or institutions* and conflict onset. The correlational literature on civil war onset has seen some debate and discussion over how to interpret the fact that low income is one of the strongest and most significant correlates of a country having a higher propensity for civil war (although there is at best weak evidence that increasing income within a country lowers its civil war risk). Some have argued for a direct causal effect via a country’s labor market. For example, perhaps poverty makes joining a rebel band relatively attractive for underemployed young men, whether for financial or more ideological reasons. Others argue that the strong association is more likely due to a spurious correlation – perhaps low income is a proxy for countries that have central states with low administrative and coercive capabilities, and/or are ineffective at supplying public goods.

One obstacle to assessing these arguments has been the lack of more direct measures of the quality of a country’s “governance” or “institutions.” In this paper I employ several sets of governance measures that are based ultimately on expert ratings, in particular, the World Bank’s Country Policy Institutional Assessment index, the Worldwide Governance Indicators (Kaufmann, Kraay and Mastruzzi 2009), and some of the International Country Risk Guide indicators. To a limited extent some of these measures have been used in research on governance quality and economic growth, but they have not been much employed in research on civil conflict.¹

Not surprisingly, and consistent with the view that low income might stand in for low institutional capabilities, governance indicators and per capita income tend to be highly correlated. However, there is some variation in governance quality even conditional on income, and this is what I try to exploit to assess the impact of the former on the latter. I find that a country that was judged in one year to have worse governance than one would expect given its income level has a significantly greater risk of civil war outbreak in subsequent years. This is true for all three sets of governance indicators considered here, although effects are barely present for the CPIA aggregate indicator. Remarkably, given that governance indicators change little over time, I find some evidence that even within countries, improvements in governance tend to reduce subsequent conflict risk. Both

¹But see Fearon (2005) and papers in the May 2010 special issue of the *Journal of Peace Research*, which focuses on state capacity.

in the fixed effect and cross-sectional models, including measures of governance tends to weaken or in some cases eliminate the association between income and conflict onset.

Moreover, it may be interesting to learn that it does not matter much which governance indicator one chooses: “government effectiveness” (WGI), “investment profile” (ICRG), “corruption” and “rule of law” (several of them) all work similarly. This could mean that different dimensions have similar effects on conflict; that in practice, good governance is like a syndrome and “all good things tend to go together”; or that expert ratings are not very good at distinguishing different dimensions of governance/institutions.

Overall, the results tend to support the view the low income countries have been at greater risk of violent conflict due to poor governance and weak institutions, more than due to direct labor market effects. The strategy for identifying the causal effect of good governance here is admittedly not iron-clad. It could be, for example, that some unmeasured factor, completely distinct from governance, is causing both “surprisingly good governance” given income, and also lower conflict risk. However it is not obvious what such factors might be, and there is at the same time a plausible argument for why the strategy employed here would underestimate the total impact of governance on conflict risk, if such an effect exists. Namely, since current income incorporates the effects of past governance and current governance is surely measured with significant error, income may tend to pick up effects of governance.

In terms of policy implications, the results lend support to the view that aid in conflict-affected countries needs to do more than try to raise incomes through project lending. If capable government is indeed the root of the problem of conflict and development more than a “poverty trap,” for example, then a more integrated approach that draws from the “peace building” and “state building” experience of U.N. and other peacekeeping operations may be necessary.

In the next two sections I reexamine some of the most common findings in the recent literature on civil war onset using cross-national panel models and the coding of civil conflict that is employed for the 2011 World Development Report. Section 3 introduces the conflict data. Section 4 presents a series of “onset models,” and includes consideration of some factors that have not been much examined in the existing literature, such as the relationship between human rights abuses by government and civil war risk. In section 5 I then discuss what these essentially correlational findings may or may not imply about the causes of civil war and lower levels of violent conflict. Section 6 returns to empirical analysis, examining the relationship between governance indicators and conflict onset.

2 Correlates and causes of civil war and lower-level violence conflict

Since the end of the Cold War, a moderately large literature has developed that uses cross-national data to study the correlates of large-scale civil violence, usually for the 65 years since the end of the World War II. The typical design is an annual panel for roughly 160 countries (micro-states are often omitted for lack of data or other reasons) with on average 35 to 45 observations per country.

Researchers have formulated the dependent variable in these statistical models in two main ways. Most of the literature examines correlates of *civil war onset*, meaning that the dependent variable is coded as “1” for country years with a civil war onset, and zero for other country years.² Other researchers look at the correlates of *civil war incidence*, meaning that the dependent variable is coded as “1” for *every* country year with (at least one) civil war occurring, and zero otherwise (Montalvo and Querol 2005; Besley and Persson 2009). A problem with the latter approach is that the estimated coefficients are complicated averages of the “effect” of a covariate on both the onset and the duration of civil conflicts. Distinguishing between onset and continuation (duration) almost invariably shows that we can reject the hypothesis that an independent variable has the same relationship to onset as it does to continuation. A more natural alternative is to study determinants of conflict onset and duration separately, using survival models for the latter (Balch-Lindsay and Enterline 2000; Collier, Hoeffler and Söderbom 2004; Cunningham 2006; Fearon 2004). In this paper I consider factors related to the *onset* of civil conflict and war, rather than incidence or duration.³

For the most part, the cross-national statistical models of civil war onset and incidence should be viewed as descriptive more than structural (or causal). That is, they have been of great value for making clearer which political, economic, and demographic factors are associated with higher civil war propensity in the last 60 years, which factors are not, and which are associated with onset when you control for other factors. But for many covariates found to be statistically and

²Some drop country years with ongoing civil war (Collier and Hoeffler 2004), others include them and introduce a variable indicating whether war was ongoing in the previous year as a control (Fearon and Laitin 2003). The latter approach avoids dropping onsets that occur when another civil war is already in progress.

³Duration dependence is also a bigger problem in incidence models than in onset models. To date, researchers who have taken incidence as the dependent variable have tried to fix the problem simply by clustering the errors within countries, but not including explicit dynamics in the form of, say, a lagged dependent variable. Implicitly, then, they assume that there is no direct causal effect of war in one year on the probability of war in the next year. This is strongly counter to theory and case-based evidence, since it is clear that there are large fixed costs to generating an insurgent movement, and major commitment problems can prevent easy dissolution of a movement once it has started (Walter 2002; Fearon and Laitin 2007). Omitting a lagged dependent variable when it should be there in an incidence regression tends to bias sharply upwards the estimated coefficients for any covariates that are positively serially correlated.

substantively significant in these models, the argument for interpreting the estimated coefficients as causal effects is tenuous or speculative. For instance, do we really believe that if the share of mountainous terrain in, say, Namibia, increased from 10% to 20% that its expected annual odds of civil war onset would increase by about 15%? Maybe, but this is not an experiment that we will ever get to run. Even so, it is of some interest to learn that in the last 65 years, there has been some tendency for more mountainous countries to have more civil wars, even controlling for a range of other country characteristics.

At least for the literature to this point, found opportunities for natural experiments to allow cleaner identification of causal effects have been rare. Miguel, Satyanath and Sergenti (2004) cleverly used exogenous variation in rainfall in sub-Saharan Africa to estimate the effect of changes in income on civil conflict propensity (see also Brückner and Ciccone (2010^a), who examine the same data and reach a quite different conclusion). And there are a few papers that use variation in international commodity prices in a similar fashion (Besley and Persson 2009; Brückner and Ciccone 2010^a). But even these papers can be of limited value for understanding why an effect is observed – what is the causal mechanism connecting changes in income to civil war propensity? – and thus whether and how it might generalize. For these reasons, arguments about causes of civil conflict have generally taken the form of attempts to make sense of the complicated pattern of associations observed in the cross-national panel data, often informed by theoretical arguments and additional evidence from cases.

In the next two sections, I review the major findings on correlates of civil war and conflict onset, reproducing and revisiting these using the conflict codings of the Uppsala Armed Conflict Database. In section 5, I consider in more detail arguments about causes of civil conflict based on these findings.

3 The conflict data and global trends

3.1 The UCDP conflict measure

The UCDP/PRIOR Armed Conflict Database (ACD) codes for each country and year since 1945 whether a violent conflict occurred between a named, non-state armed group and government forces that directly killed at least 25 people.⁴ I work with a version of the data used in the preparation of the 2011 World Development Report that has (rough) estimates of annual battle deaths for each conflict. Following the WDR's categorization scheme, we will distinguish between “major” conflicts, or civil wars, that are estimated to have killed at least 1000 on average per year over the whole episode of conflict; “medium” conflicts estimated to have killed an average per year between

⁴<http://www.prio.no/CSCW/Datasets/Armed-Conflict/UCDP-PRIO/>

500 and 999; “small” conflicts estimated to have killed a per year average between 250 and 499; and “minor” conflicts estimated to have killed between 25 and 249 on average per year. “Killed” in all cases is intended to refer to battle deaths rather than “indirect” deaths due to starvation, deprivation from medical services, and so on. These levels are thus measures of the *intensity* of the conflict.

Two features of the ACD conflict data should be noted before we present some descriptive statistics. First, the ACD does not commit to any particular scheme for identifying *episodes* of civil war. That is, the raw data simply records whether there is enough fighting going on in the country year (and other conditions are satisfied on the nature of the fighting) to qualify for inclusion. So it is difficult to know how to use these data to produce a list of distinct civil wars or conflicts, which is unfortunately just what we need if we want to study determinants of civil war onset or duration. For that we need to know when a conflict started and when it ended. In many cases this is intuitively clear, but in many others it is not. For example, some low-level conflicts flit above and below the 25-dead threshold for a period of many years. Is that one conflict, or many?

In this paper I will follow the suggested practice for WDR 2011 (which is also employed by many other scholars using the ACD). We will consider a new civil war or conflict to have started if it is preceded by at least two years of peace between the present adversaries. Thus, if a ‘1’ represents a year with a conflict that killed at least 25 in a country, then a sequence like . . . , 0, 0, 1, 1, 0, 1, 1, 1, 0, 0, . . . would be coded as one civil war episode despite the one year break in fighting above the 25 dead threshold. By contrast, a sequence like . . . 0, 0, 1, 1, 0, 0, 1, 1, 0, 0 . . . would be coded as having two civil wars onsets. An advantage of this approach is that it avoids potentially more subjective or difficult judgments about whether a break in fighting is an “end” or a merely a pause in a continuing war. A disadvantage is that it tends to render long-running, relatively low level conflicts as *any* civil wars. For example, if we consider all levels of conflict (minor through major), then by this coding the most onset-prone countries since 1945 are India with 21, Burma with 19, and Ethiopia with 13 conflict onsets. Though each country has certainly suffered a great deal of violent conflict, the main reason that these numbers are so high is that they have suffered a particular form of conflict – small rebel groups fighting mainly over specific territories that often jump above or below the 25 dead threshold. At the highest level of intensity (1000 or more dead per year on average), this problem is much less serious. At that level, the most onset-prone countries in the sample are Indonesia with 6, and Iraq and China with 5.

A second consequential feature of the ACD coding rules concerns the treatment of multiple conflicts within a country. For example, if two rebel groups are fighting the government at the same time, is that two civil wars or one civil war? At one extreme one could argue for making the thing to be explained whether there is *any* war (or conflict) that starts in a country in a given year when the country doesn’t already have a war occurring, thus ruling out the possibility of multiple civil wars in one country. The ACD approach comes closer to the opposite extreme of coding a new civil war every time there is a conflict with a new rebel group that meets the threshold requirements. ACD codes conflicts as being of two types, “government” where the rebel group aims to capture

the central government, and “territorial” where the rebel group aims to secede or win increased autonomy for a specific region.⁵ As a result, every time a violent rebel group appears that advocates for a particular region or part of the country, the country gets a new conflict in the ACD data. This leads to coding of large numbers of distinct civil conflicts in countries like Burma, Ethiopia, and India, where multiple, often very small territorial rebel groups have operated, and often in the context of what is arguably a larger civil war.

By contrast, if one rebel group defeats the central government and then some new rebel group immediately arises to contest its control – think of Afghanistan or Somalia in 1991 – then for ACD there is no new civil war since the fighting is still about taking over the government. An alternative approach codes a new civil war when one or both of the main combatants has changed (e.g., Fearon and Laitin 2003, COW). Both approaches seem defensible.

As it happens, many of the cross-sectional patterns are fairly robust to different civil war codings. The main difference that results from using an ACD-based series versus other common alternatives⁶ is that the ACD approach increases the number of civil war onsets in some highly ethnically fractionalized countries that have had long-running internal conflict. And not just any ethnically fractionalized countries, but in particular ones where there is a predominant ethnic group that controls the center so there is not much chance that ethnic minorities at the periphery can take power or play a significant role in coalition politics. As noted, the ACD approach to coding multiple civil conflicts leads to large numbers of conflict onsets for Ethiopia, Burma, and India, almost entirely due to distinguishing conflicts between the state and multiple regional minorities as distinct civil wars.⁷ The effect in conflict regressions is to increase the strength of association between onset and various measures of ethnic fractionalization, when the dependent variable includes low-level conflict. These tend not to be significantly related in other civil war lists with higher magnitude or intensity thresholds (Collier and Hoeffler 2004; Fearon and Laitin 2003), but they are often significant in ACD-based analyses, especially when researchers include minor-level conflicts.

To foreshadow the results on ethnic diversity and civil war risk discussed more below, the upshot is that while more ethnically diverse countries have not been much more likely to have civil war over the whole post-war period, they have been more likely to have lower-level conflicts with multiple rebel groups fighting in the name of diverse regional minorities. This appears to be particularly

⁵This leads to some odd codings for cases where there is ambiguity or variation over time in announced objectives. For example, Anya Nya in Sudan is coded as territorial, but both the SPLM and Darfur are coded as “government.” Conflicts between Israel and Palestinian groups are coded as territorial.

⁶Such as the Correlates of War-based coding used by Collier and Hoeffler (2004), Sambanis’ (2001), or Fearon and Laitin’s (2003).

⁷For example, in India: Nagaland, 4 conflict onsets; Mizoram, 1; Tripura, 2; Manipur, 3; Punjab, 1; Kashmir, 1; Assam and Bodoland, 2 each. Other countries with a similar ethnic configuration and many ACD onsets include Indonesia, Iran, and Pakistan.

true for countries with many small minority groups, and relatively large “dominant” ethnic group at the center.

3.2 Global and regional trends

Figure 1 below uses the ACD data as described above to display the number of civil wars (major) and all conflicts (major, medium, small, and minor) by year from 1946. Figure 2 is the same except that it shows the percentage of countries with a civil war or any conflict by year.

The basic features of these graphs track with previous studies using other civil war and conflict lists. There was a steady increase in conflicts from the end of World War II to the early 1990s. Since then there has been something of a decline, although civil war prevalence remains quite high, with total conflicts in the 30s in 2008, in a bit more than 12% of 193 independent countries. One interesting and possibly novel observation from the data shown here is that (major) civil wars have continued to trend down in the last five or six years, but minor conflicts have jumped back up. This could augur a return of more major conflicts (since minor conflicts become major if they continue over time), or it could reflect a change in the distribution of conflict sizes.

Figure 3 presents the same data but broken down by regions, which sheds some interesting light on the sources of the decline in total conflicts in the last 15 years. In Eastern Europe and the former Soviet Union the spate of early 90s conflicts quickly subsided, or “froze” in some locales. There has also been striking decline of both major and minor conflict in Latin America, which is plausibly related to the end of the Cold War for most cases. Elsewhere, in subSaharan Africa and Asia there have been some declines in major, high intensity conflicts, but no change in minor conflict in Asia and at best a slight decline in Africa. The number of major conflicts has stayed fairly steady in North Africa/Middle East, while the number of all conflicts there has declined slightly.

Figure 4 shows that despite the post-Cold War decline in the number and proportion of countries with civil conflict, the share of the world’s population living in conflict-affected countries has remained fairly steady or even increased, to one-third in 2008. Of course this does not mean that one third of the world’s population has been directly affected by organized violent conflicts, since they are mostly localized within countries. Still, it is a measure of prevalence and impact, and reflection of the fact that conflict is much more likely in larger countries (discussed below).

Figure 5 considers where conflict-affected countries are found in the global distribution of income over time. Notice that close to 80% of conflicts have occurred in countries with incomes below the global median over the whole period, a number that shows no strong trend. However, there has been a fairly steady increase in the share of conflicts in the second quartile on income, while the share occurring in the poorest 25% has declined from almost two thirds in the early 60s to about one third. Thus there has been some tendency for conflict to become more common among middle- and lower-middle income countries.

We should also describe the variation in the main outcome variables of interest in the analysis that follows – conflict onset at different levels of intensity. Table 1 shows the total number of conflicts at different intensity levels by region, both for the whole 1946-2008 period and for the post-Cold War years (1990-2008). SubSaharan Africa and Asia have had the largest number of both large and small conflicts over both periods. We see again that Latin America and, in terms of new conflicts, Middle East/North Africa, have become rather more peaceful since the end of the Cold War.

Finally, note that while for the whole period major and minor conflicts were similar in frequency, since the end of the Cold War there have been relatively fewer high intensity wars and relatively more low-level conflicts. Figure 6 shows more directly. The decline in the proportion of high intensity conflict is probably mainly due to the end of the Cold War, since it is known that foreign intervention is strongly related to more intense conflicts and there was more foreign intervention by major powers during the Cold War. However, improved international response through peace-keeping and related methods may also be a factor.

Table 1: # conflicts by region and intensity

	1946-2008					1990-2008				
	major	medium	small	minor	all	major	medium	small	minor	all
SSA	24	22	8	40	94	8	11	3	28	50
Asia	31	21	8	25	85	4	6	7	16	33
MENA	20	6	3	16	45	5	1	0	9	15
L. America	11	4	3	17	35	0	0	0	5	5
E.Eur/FSU	12	7	2	11	32	10	5	2	10	27
West	1	0	0	5	6	0	0	0	2	2
total	99	60	24	114	297	27	23	12	70	132

average kia/year: major ≥ 1000 , medium 500-999, small 250-499, minor 25-249.

4 Correlates of civil war onset

4.1 Baseline models

Table 2 reports the results of logit models with civil war or conflict onset as the dependent variable, using as covariates things that previous studies (and in particular Fearon and Laitin (2003)) found to be substantively and significantly related. The first model is for ACD civil wars, major conflicts that had average annual fatalities greater than 1000. The second uses an up-to-date version of

Fearon and Laitin's civil war list, and shows that the results are nearly the same.⁸ In the third model the dependent variable is the onset of *any* level of ACD conflict, from minor to major. Results are again similar, although a number of coefficients shrink a bit towards zero. The most striking difference is that ethnic fractionalization has a much larger apparent "effect" when we include minor conflicts; this is mainly for the reasons discussed in the previous section.

Models 4 and 5 are Models 1 and 3 but estimated with country and decade fixed effects. The factors that vary a lot within countries over time have similar effect estimates. Those that do not, like income and population, change markedly. This already suggests that these variables may appear to "matter" in the cross-sectional analysis due to omitted variables rather than a direct causal effect (to be discussed more below).

The variables are:

- log of per capita income in the previous country year, in 2005 U.S. dollars, using Penn World Tables 6.3 data extended where possible and necessary by World Bank growth rates.⁹
- Log of country population in the previous country year.
- Log of the percentage of mountainous terrain in the country (plus one), as judged by geographer John Gerrard.
- *oil*, which marks whether the country is a major oil producer, coded by whether one third or more of its GDP comes from natural resources (based on World Bank data).
- *ne□state*, which marks whether the country is in its first two years of political independence.
- *□olitical insta□ility*, which marks whether, in the previous country year ($t - 2$ to $t - 1$), there was any change in the Polity 2 score. The Polity score is a measure of democracy that runs from -10 (extreme autocracy) to 10 (full democracy).
- *anocracy*, which marks whether the country's Polity 2 score is between -5 and 5 (inclusive) in the previous year. This is a measure of partial, or weak democracy.¹⁰

⁸FL's threshold criterion is 1000 total killed, rather than the annual intensity threshold used here. As a result the FL is slightly more permissive in the sense of letting in more conflicts. It is, in effect, intermediate between "major" and "all ACD," though much closer to major.

⁹Listwise deletion due to missing income data is a big problem for civil conflict regressions, because it is not missing at random – civil war countries are more likely to have missing income data. The data used here is a relatively complete set of estimates, drawing primarily on PWT6.3 but using World Bank and Maddison estimates for some missing years and countries

¹⁰As Vreeland (2009) points out, some components of the aggregate Polity measure may be coded in part on the basis of observation of armed conflict, in which case the "effect" of anocracy may not be an effect of a

- EFG is a measure of the ethnic fractionalization of the country, based on estimates of ethnic group populations from a Soviet ethnographic atlas compiled in the early 1960s, and updated for some newer countries (Fearon and Laitin 2003). It can be interpreted as the probability that two randomly drawn individuals from the country are from different ethnic groups.
- *religious fractionalization* is a similar measure of religious diversity (Fearon and Laitin 2003).

4.2 Effect magnitudes

Table 2 provides estimates of the substantive magnitude of “effects” in Table 1, Model 1. The baseline country in this example is a stable, non-oil producing autocracy with no civil war in progress, with the median income (\$4,100), population (8.3 million), and “mountainousness” (9%). Such a country had about a .57% chance of civil war outbreak in any given year, which translates to 2.8% over five years and 5.6% over ten. These small numbers should remind us that new civil wars do not break out that often. With the ACD major wars, the rate is only 1.14% of all country years from 1946 to 2008, and about 1.44 new civil wars per year (using the sample with data on the covariates, which has about 156 countries in it). Counting all conflicts, major through minor, the corresponding rates are 3.3% and 4.1%.

The “relative risk” column shows how the indicated change affects the odds of civil war onset in the next year. For example, moving from the 75th to the 25th percentile on per capita income is associated with a 2.03-fold increase in the annual odds of onset. Thus, the relative risk score provides a rough way to compare the magnitude of the associations across covariates.

By this measure, the most striking pattern is that newly independent states have a much higher risk of civil war onset than other states – more than six times greater, using the ACD data. The fact that the effect estimate is similar in the fixed-effects model shows that this is not an artifact of states that became independent since 1945 having higher average conflict risk than that of older states. Rather, even within former colonies, the first two years are the most dangerous. I discuss the implications and interpretation of this observation in section 5 below.

After “new state,” oil producers are estimated in this model to have about triple the annual risk of major civil war onset. Any change in the Polity index in the previous year (whether in a democratic or autocratic direction) about doubles the subsequent odds, as does moving from 75th to 25th percentile on income or from the 25th to 75th on rough terrain. Anocracy (partial democracy) is associated with 29% greater annual odds of civil war outbreak, although this is not statistically significant in this model where the dependent variable is major war).

class of political systems. Instead, it could be that “partial democracy” is at least partly picking up incipient

Table 2: Correlates of civil war onset, 1946-2008

Model	1	2	3	4	5
DV	ACD major war	FL civil war	all ACD	ACD major war	all ACD
log(gdp _{t-1})	-0.404*	-0.404***	-0.351***	0.012	0.123
	(0.177)	(0.111)	(0.099)	(0.384)	(0.220)
log(pop _{t-1})	0.203**	0.313***	0.238***	-0.192	-0.588
	(0.067)	(0.066)	(0.056)	(0.654)	(0.423)
log(% mountains)	0.360***	0.186*	0.151*		
	(0.100)	(0.072)	(0.062)		
oil producer	1.095***	0.698**	0.715**	0.978	0.326
	(0.297)	(0.242)	(0.219)	(0.617)	(0.415)
new state	1.862***	1.913***	1.336***	1.084*	1.125***
	(0.377)	(0.307)	(0.292)	(0.452)	(0.321)
pol instability _{t-1}	0.706**	0.746***	0.466**	0.714*	0.484**
	(0.261)	(0.205)	(0.170)	(0.293)	(0.184)
anocracy _{t-1}	0.258	0.482*	0.355*	0.281	0.636**
	(0.252)	(0.199)	(0.175)	(0.313)	(0.200)
democracy _{t-1}	-0.245	-0.466	0.080	0.232	0.276
	(0.408)	(0.329)	(0.215)	(0.451)	(0.263)
ELF	0.534	0.521	1.153***		
	(0.389)	(0.344)	(0.287)		
relig frac	0.597	-0.176	-0.228		
	(0.756)	(0.499)	(0.429)		
prior war	-0.519	-0.548**	0.204	-1.861***	-1.111***
	(0.320)	(0.185)	(0.228)	(0.400)	(0.203)
constant	-4.868**	-4.795***	-4.001***		
	(1.583)	(1.080)	(0.982)		
N	7929	7985	7873	2804 (55)	4805 (96)
Fixed effects ^a	No	No	No	Yes	Yes

Standard errors clustered by country in parentheses

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ ^a By country and decades.

armed conflict.

Table 3: Magnitude of “effect” estimates, Model 1

variable	level	pctile	% chance onset in 1 year	% chance onset over 5 years	% chance onset over 10 years	relative risk
baseline ^a		50	.57	2.84	5.6	
gdp/cap	\$1,665	25	0.83	4.06	7.96	2.03
	\$9,612	75	0.41	2.03	4.01	
population	3.3m	25	0.48	2.37	4.68	
	21.5m	75	0.7	3.43	6.74	1.46
% mountains	1.7	25	0.35	1.76	3.49	
	27.2	75	0.82	4.04	7.92	2.33
ELF	0.12	25	0.51	2.52	4.97	1.34
	0.66	75	0.68	3.35	6.59	
baseline ^a		50	0.57	2.84	5.6	
new state instability			3.59			6.44
oil			1.16	5.66	10.99	2.03
anocracy			1.7	8.21	15.75	2.99
			0.74	3.66	7.18	1.29

^aan autocracy that in the previous year was stable, at peace, a non-oil producer, with median income (\$4,117), population (8.3 mill.), mountains (9.4%), ELF (.35), and relig. fractionalization.

5 Other country characteristics of potential interest

5.1 Population growth and pressure and youth bulges

So-called “neo-Malthusians” fear that rapid population growth, or population per hectare of arable land, causes violent conflict by increasing competition for resources.¹¹ A more specific argument in this vein is that what matters is the ratio of young males to the rest of the population (or to just the adult population) (Huntington 1996; Urdal 2006). Fearon and Laitin (2003) and Urdal (2005) found little support for a strong relationship between either population growth rate or population density on arable land, but Urdal (2006) finds evidence in favor of a relationship between the share of the adult population under 25 and conflict risk. Fearon and Laitin (2003) and Collier and Hoeffler (2004) had found no relationship for the (very similar) measure of share of total population

¹¹Huntington (1996) sometimes stressed population pressures. See also Homer-Dixon (2001).

between 15 and 24.

Using our ACD-based measures for onset of major and all conflicts, I find that lagged population growth rates are positively associated with conflict risk, but the estimated coefficients are substantively very small and not statistically significant. When added to Model 1 in Table 1, lagged population growth rate gets an estimated coefficient of about .08 with a standard error of .11 ($p = .45$).¹² Somewhat smaller standard errors and slightly higher estimated coefficients can be found if one drops various other variables from the model. Population growth rates are moderately well correlated with several other variables, such as income (negative), oil producer (positive), anocracy (positive), democracy (negative), and ethnic diversity (positive). Overall, it tends to lose out in the “battle of the covariances” with these other variables, but not by much. Results are similar whether we look at major or all conflict onsets.

Results for a measure of arable land per capita (from the World Development Indicators) are similar, or even weaker. Arable land (in hectares) per capita is negatively related to civil war onset when added to Models 1 or 3, but not close to statistically significant. This measure of resource scarcity, if that is what it really is, shows no consistent association with higher civil war risk.

Urdal (2006) found that “youth bulge” as measured by the share of 15-to-24 year olds in the total population is not related to onset risk, which is what Fearon and Laitin (2003) and Collier and Hoeffer (2004) also found. However, Urdal reported that “youth bulge” as measured by the share of 15-to-24 year olds in the *adult* population *was* related to civil war onset in his data. He argued that “from a theoretical perspective” (p. 615) the latter indicator is to be preferred, but it is not clear what the theoretical argument is (to me, anyway). The two measures are highly correlated ($r = .85$), which increases the difficulty in saying what is different about them.

For the reanalysis undertaken here, I began by importing Urdal’s measure, which has “youth bulge” data for 1950 to 2000. Added to Model 1 the estimated coefficient is positive but substantively small and not close to statistically significant ($p = .78$). The estimated coefficient for income weakens slightly – low income and youthful populations are highly correlated – but remains close to significant at $p = .11$; otherwise nothing changes much. If we use all ACD onsets, “youth bulge” as a share of adult population is marginally significant at $p = .097$, taking away from income whose coefficient halves but keeps a p value of .078. It thus appears that “youth bulge” as Urdal formulates it performs less well as a predictor when the dependent variable is based on this ACD civil war list than in the formulation he used. It does slightly better, and can be “significant” at .10 under certain specifications, if we use Fearon and Laitin’s civil war onset variable.

I then updated the population estimates using the same source Urdal employed (U.N. Population

¹²I drop observations that have a population growth rate above the 99th percentile or below the 1st percentile, because changes in state form and some bizarre data points make for some extreme and influential outliers. If these are left in, results are highly erratic depending on specification.

Division data), which allowed an extension of the data to 2009. I also used the data for males between the ages of 15 and 25, rather than both males and females (although this almost surely would make no difference at all). I find that neither young males as a share of adult or total population is significantly related to civil war onset in these data. Added to the specifications in Model 1 or Model 3, or paired with income and other combinations of variables, it may take a positive or negative coefficient but the estimates are not close to being statistically or substantively significant. Once again, young males as a share of adult population (but not young males as a share of total population) performs somewhat better as a predictor if the dependent variable is from Fearon and Laitin's coding of civil war.

What to conclude about “youth bulge” and civil war onset? As Fearon and Laitin (2003) noted, “youth bulge” is strongly negatively correlated with per capita income, which makes it difficult to get stable or sharp estimates of the partial correlation with civil war onset controlling for other variables. There is a tendency for income per capita to “trump” youth bulge in these data, a tendency that is very strong when youth bulge is measured as youth over total population and fairly strong when it is measured as youth over adult population. Contrary to Urdal’s view, I find it difficult to come up with a plausible or clear theoretical rationale for why the results of these two different measures should be particularly different. So I am inclined to think that the evidence is not very good that population structure explains a big part of why poor countries are more civil war prone, rather than something else about poor countries explaining why countries with young populations happen to be more civil war prone.

“Opportunity cost” arguments about civil conflict hold that conflict is favored by anything that makes joining a rebel band financially more attractive for young men. Unemployment is sometimes suggested as one such factor, and one might hope that “youth bulge” could provide a reasonable cross-national proxy for youth unemployment rates. Direct cross-national data on unemployment (from the WDI) is not usable for this purpose, as it is self-reported according to definitions that vary greatly across countries, and the reporting is itself highly erratic across countries and years.

I considered whether youth bulge is related to the WDI unemployment rates for countries that might be expected to have similar reporting standards and where data is available. Unfortunately, there is no clear indication that higher unemployment tends to go along with many young males as a share of the adult or total population. As a result, the above results simply cannot speak to whether higher unemployment rates are correlated with higher risk of conflict.

5.2 Vertical and horizontal income inequality

The bivariate relationship between income inequality and civil war onset in these data is actually *negative* (more inequality, lower odds of conflict), although not statistically significant. The negative sign persists when we add the covariates in Table 1, Model 1, or subsets of them, and remains

insignificant ($p = .17$) in the full model. For all ACD onsets, the estimates are essentially zero. Nor is this a matter of inequality picking up regional “effects,” as results are the same when regional dummies are added.¹³

Not too much should be made of this in the absence of better inequality data, and a more theoretically informed model specification. Still, it is interesting: Contrary to some long-standing claims about the causes of civil conflict, not only is there no apparent positive correlation between income inequality and conflict, but if anything across countries those with more equal income distributions have been marginally more conflict prone.

Some have proposed that a more relevant form of income inequality is “horizontal,” meaning across groups within a country as defined by ethnicity or religion (Stewart 2002). Good measures of inequality across groups are hard to find and construct, however. Early efforts to use the Minorities at Risk data (Gurr 199x) to examine the relationship between economic disadvantage of “minorities at risk” and propensity to rebel found inconsistent or no evidence (Gurr 199x, Moore 199x, Fearon and Laitin 1999). More recently there have been some efforts to use the Demographic and Health Surveys (<http://www.measuredhs.com/>) to construct measures of the relative economic standing of different ethnic groups in sub-Saharan Africa. Results are inconsistent, though overall not very supportive of the “horizontal inequalities” proposition. Østby (2008) finds at best very faint evidence that ethnic horizontal inequalities are related to conflict onset at the country level. Østby, Nordås and Rød (2009) find positive but not significant coefficients on measures of the relative economic and social deprivation of a region (relative to the rest of the country) and conflict onset in Africa. Using similar measures of economic welfare but looking specifically at ethnic groups rather than administrative regions, Condra (2009) finds that, if anything, it is the relatively better off ethnic groups have been more prone to be involved in rebellions in sub-Saharan Africa.

Cederman and Girardin (2007) examine another possible interpretation of horizontal inequality, in the idea that ethnic groups whose members are excluded from political office will be more likely to rebel. They construct a measure that takes high values when the population share of the “ethnic groups in power” (EGIP) is small and the population share of “marginalized ethnic groups” (MEG) is high. They find that this measure is associated with higher onset probabilities, using data for Eurasia and North Africa (Latin America and Africa were not coded).

Fearon, Laitin and Kasara (2007) examined their data and found that the results are completely driven by the four observations where the coded EGIP is a minority. Unclear on what the coding rules were for EGIPs, Fearon, Kasara, and Laitin coded instead the ethnicity of the ruler for all country years in all regions. They find a positive but statistically insignificant relationship between

¹³I have used the WIDER inequality measure, which is based on an updated version of Deininger and Squire. The variable is constructed as the average of all observations within each country. Results are the same if we interpolate.

rule by a member of an ethnic minority and civil war onset.¹⁴ In their analysis (which, like Cederman and Girardin, used the Fearon and Laitin (2003) model and civil war list), ethnic minority rule has no association with civil war onset at all in sub-Saharan Africa or Latin American, but some signs of a positive relationship in the rest of the world. However, even in Eurasia ethnic minority rule is quite rare, so it is hard to establish any real pattern.¹⁵

More recently, Wimmer, Cederman and Min (2009) have analyzed the results of a more systematic effort they undertook to code EGIPs and MEGs. They used some process of expert surveys and consultations to assess first whether ethnicity was “politically relevant,” in a country year, meaning that their coders perceived discrimination or politicians mobilizing based on ethnic appeals. This leads to a number of ethnically diverse countries – such as Burkina Faso, Tanzania, and Papua New Guinea – being coded as having no “ethnopolitical groups” at all, and thus no possibility of ethnic exclusion.¹⁶ Second, for each “ethnopolitical group” in each country-year their experts coded whether the group has “monopoly power” or is “dominant,” or if it is an “excluded group” that has “regional autonomy,” is “powerless,” or is “discriminated” against. They find that the log of the share of what they call “excluded” ethnic groups in all “ethnopolitical” groups is robustly associated with civil war onset, and even more strongly related to the onset of ethnic civil wars.

Importing Wimmer et al.’s country-level codings into the data set considered here, I find that the log of the lagged share of “excluded groups” is positively related to major conflict onset odds when added to Model 1.¹⁷ The coefficient is .24 with a standard error of .08; substantively this implies that moving from a country with no excluded groups to one where 23% of the population of ethnopolitical groups are excluded (25th to 75th percentiles) approximately doubles the annual risk of civil war outbreak. The unlogged version of the variable is much more weakly related. With fixed effects the estimated coefficient is essentially zero, which suggests that “exclusion” is picking up enduring characteristics of countries more than a switch to exclusion of a particular group significantly increases the average risk of civil war.

When we include all ACD conflicts, minor through major, the estimated coefficient on the log of

¹⁴Related variables, like the size of the leader’s ethnic group, or the ratio to the second largest group, perform worse than a simple dummy for ethnic minority rule.

¹⁵There are also (as usual) concerns about endogeneity. Cross-sectional analysis could underestimate the conflict-generating effects of ethnic minority rule, if it is more likely in precisely those places where it is viewed as tolerable. On the other hand, ethnic minorities may in some cases work especially hard to attain and hold onto power where they would be highly threatened if they lost power (e.g., Syria, Iraq). Fixed effects models that try to control for country-specific characteristics like this yield unstable, though usually positive, estimates, because there are so few cases to go on.

¹⁶In all three cases politicians have mobilized along ethnic lines (in Papua New Guinea, this is essentially all there is), but the broader concern is whether this is coding on the dependent variable or not.

¹⁷I added .01 to avoid log of zero; I am not sure what Wimmer et al. do.

Wimmer et al.s' excluded groups variable is one third as large and is not statistically significant.

One could argue that the more relevant measure should be the share of population that is “discriminated against,” as “excluded” includes groups that could be content with their regional autonomy arrangements or are not particularly unhappy with being “powerless” (whatever exactly this means). Using as a predictor the share of ethnopolitical groups that are “discriminated against” by Wimmer et al.s' codings yields similar results to “excluded,” except that the (logged) variable is now also a significant predictor of all ACD onsets. In sum, Wimmer et al.'s study suggests that countries that raters judge to have bad ethnic relations and discrimination against relatively larger groups are more prone to civil conflict.¹⁸

Two caveats about these findings should be noted. The first mirrors a similar issue for the expert-ratings based governance indicators that are examined below. Namely, these measures of political exclusion and discrimination are based on the subjective judgements of diverse coders, trying to code somewhat impressionistic things. Countries where there has been no ethnic conflict and where ethnic relations have been calm are for that reason judged to have a low value on “exclusion” – thus, the dependent variable at least partly determines the coding of the independent variable. More generally, one can reasonably worry that a coder's knowledge that there was an ethnic conflict in a country increases the probability that he or she judges that, earlier on, groups were discriminated against or politically excluded. And codings of discrimination at time t may be based on earlier experiences of conflict, again making it hard to sort out causes and effects.

The second concern is that when we include a variable that tries to measure the extent of the population that is “excluded” or discriminated against by government policy, we are now running a *policy regression*. That is, we have put a variable that is a direct policy choice on the right-hand-side. Income per capita, and even ethnolinguistic fractionalization, can be viewed as the results of policy choices, as well. But they are produced by policy choices over longer periods of time, and arguably much more indirectly, than a variable that tries to measure current government policy with respect to an ethnic minority. If we have concerns about the endogeneity of income and ELF, which we should, then we must have them far more strongly about a direct policy.¹⁹

It is very important to understand that the endogeneity of the policy choice might lead us to over- or underestimate the average causal effect of introducing a more (or less) exclusionary policy in a typical country. For example, if governments tend to calibrate levels of exclusion to what they can get away with, then estimates from panel data may understate what would be the causal impact

¹⁸Cederman, Wimmer and Min (2010) use their EPR data to present a similar analysis at the level of ethnic groups in countries. They find similarly that “excluded” groups are more likely to have been engaged in rebellions than are groups judged by their expert raters to have some share of power in the central state. At the group level of analysis, they report that this relationship still obtains when they include country fixed effects.

¹⁹See Rodrik (2005) for a nice discussion of this issue in the context of studies of economic growth.

of arbitrarily switching to more exclusionary policies (Fearon and Laitin 2010). Alternatively, to the extent that exclusionary policies are themselves driven by fear of rebellion for other reasons (such as rebel opportunity), then panel data estimates will tend to overestimate the causal impact of exclusion or discrimination.

Buhaug, Cederman and Rød (2008) consider determinants of ethnic conflict at the group (rather than the country) level. They find that Eurasian ethnic groups that are larger, live farther from the capital, and in more mountainous terrain are more likely to be involved in ACD minor or major level conflicts.²⁰ Curiously, they end up interpreting this as supporting an “exclusion” versus an “opportunity” explanation, although their three main variables (size, distance, and terrain) are usually considered to be at least as plausible as measures of capability to sustain rebellion as of motivation to rebel.²¹

5.3 Civil liberties and human rights abuses

Are countries whose governments abuse human rights and restrict civil liberties more prone to civil war onset? This certainly seems plausible and likely on its face. The question is motivated by an intuition similar to that behind studies of horizontal inequality, though here the focus is on whether repressive or restrictive government policies favor war even if they are not necessarily directed at any particular ethnic or religious group.

Once again, however, we need to be careful about the interpretation of results, since abuse of human rights and restriction of civil liberties are policy choices and thus almost surely are endogenous to other causes of conflict. Even worse, there is a major danger that these indicators may simply pick up onset of civil conflict before it happens to get coded by ACD or others. Is this government abuse that is causing a conflict, or government abuse that is already part of a conflict we are trying to explain? Still, it may be interesting to know how strong is the correlation between existing measures and civil war onset.

Freedom House provides a 1-to-7 scale of government observance of civil liberties for a large number of countries since 1972. Although procedures have varied over the years, for the most part the scale is constructed from expert responses to 15 questions grouped into four areas, concerning “Freedom of Expression and Belief, Associational and Organizational Rights, Rule of Law, and Personal Autonomy and Individual Rights.” Higher values on the scale indicate *fewer* civil liberties for citizens of the country. One problem with this measure for our purposes is that while government behavior is clearly the focus, the measure is not in principle limited to government behavior.

²⁰Condra (2009) reports similar findings for groups in Africa.

²¹All the groups in the sample are what they call “marginalized ethnic groups,” so that there is no opportunity to estimate the effect of “marginalization” by this design.

For example, a country may also be judged to have worse civil liberties if it has “groups opposed to the state [that] engage in political terror that undermines other freedoms.” Thus to some extent the scale may incorporate a measure of civil conflict.

The civil liberties measure proves to be highly correlated with other measures of democracy – for instance, $r = -.85$ with Polity – which we have already seen is not a significant predictor of civil war or conflict onset. When added to Model 1, the coefficient on lagged civil liberties is positive (worse civil liberties, higher conflict risk) and just statistically significant if the Polity measures of regime type are not included ($p = .083$). However, if lagged for two years, the estimate is close to zero and not at all significant. This suggests strongly that that the measure is picking up incipient civil war, at least as much as bad civil liberties performance causes greater civil war risk.

In addition, estimates are smaller and insignificant (even with the one year lag) if we include the variables for anocracy and democracy as measured by Polity; if we look at simpler specifications provided income is included; and if the dependent variable is all conflict (minor to major). Basically, observance of civil liberties behaves similar to democracy indicators. Indeed, if we add a squared term (and drop the Polity measures), we find evidence of the inverted U. Other things equal, civil war risk is highest for countries with a Freedom House civil liberties score of 5 out of 7, which they describe as countries “with a combination of high or medium scores for some questions and low or very low scores on other questions.”

Since the Freedom House civil liberties measure may be coded in part for civil conflict, one concern may be that including lagged civil liberties is like including an indicator for whether there was conflict in the previous period. I also tried running Model 1 (and variants) only for cases in which there was no civil war in the prior period. This leads to the coefficient on civil liberties doubling (or more), and marginally greater statistical significance. Overall, it is difficult to know whether these results suggest that democracy has a direct effect of lowering conflict risk, or if government abuse of civil liberties has a direct effect, or neither.

A potentially more focused measure of government abusiveness is the Political Terror Scale, an annual 1-to-5 index based on coding of Amnesty International and State Department human rights reports from 1976 to 2008 (Gibney, Cornett and Wood 2008). Mark Gibney writes that “Coders are instructed not to turn a blind eye towards violence by non-state actors, but that their primary goal is to measure levels of violence by the state.”²²

²²There are two indices, one based on Amnesty reports and the other based on State Department reports. They are well correlated at .8. As is common, I use the average of the two. The description of scale levels is: “(Level 5) Terror has expanded to the whole population. The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. (Level 4) Civil and political rights violations have expanded to large numbers of the population. Murders, disappearances, and torture are a common part of life. In spite of its generality, on this level terror affects those who interest themselves in politics or ideas. (Level 3) There is extensive political imprisonment, or a recent history

The main difficulty that this measure poses is that it is hard to separate out government abusiveness that is part of a civil war from government abusiveness that causes a civil war onset. Of the 774 country years coded as 4 or 5 on the PTS (the highest two levels of human rights violations), almost half are ACD major conflict years, and almost 70% have some kind of ACD conflict in progress. In other words, most of the worst human rights abuses, according to these data, are carried out by governments during civil wars. The problem is then that if lagged human rights performance predicts onset in a regression model, we don't know how much this is because the onset of civil war is coded with error, and how much it is because government abusiveness caused rebellion. A second difficulty is that the lagged PTS variable can act as an indicator for prior civil war(s) in progress.

Table 3 reports the results of adding one- and five-year lags of the PTS measure to Model 1, both for major conflicts and for all conflicts. With a one-year lag the scale appears to be *very* strongly related to subsequent civil war onset, both in terms of a substantively large estimated coefficient and statistical significance. However, for major wars the estimate for PTS when lagged five years is only one-fourth as large as for the one-year lag, which suggests that there may indeed be a big problem with the one-year lag picking up conflicts that have already started (this may also happen for the five-year lag, but hopefully less so). We also see this when the dependent variable is all ACD conflicts, although here there is still a statistically significant relationship between PTS lagged five years and conflict onset.

Substantively, the estimated association for the one-year-lag with major conflicts is simply enormous: the .88 estimate in Model 1 means that *each step up* the Political Terror Scale associates with a 2.4 times increase in the odds of civil war onset in the next year. This would imply that country years with a 5 on PTS have about an 80 times greater odds of onset in the next year than those with 1! If we use the one year lag estimate for all conflicts, each step on the scale associates with an 43% increase in odds, and the difference between 5 and 1 is a factor of 6, which is comparable to the effect of “new state.”

The extremely large estimate for the “effect” of lagged human rights abuses on civil war onset suggests that something may be going wrong in the estimation. Closer investigation reveals that this is the case: zero major conflict onsets are recorded for states with a 1 on the PTS scale, which means that the relative risk in going from 1 to 2 is infinitely large. The models in Table 3 implicitly treat PTS as a linear scale, estimating an average “effect” of moving from 1 to 2, 2 to 3, and so on. If we dummy out each of the five levels, we find that we cannot estimate a logit model because there were zero ACD civil war onsets for states at PTS level 1. (Level 1 states are described by

of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without a trial, for political views is accepted. (Level 2) There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beatings are exceptional. Political murder is rare. (Level 1) Countries under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional. Political murders are extremely rare.”

Gibney as countries “under a secure rule of law, [where] people are not imprisoned for their views, and torture is rare or exceptional.”)

Table 4 shows the frequency of major and all conflict onsets for each PTS level, by both one- or five-year lagged human rights abuses. We see that major conflicts almost never follow when prior PTS levels were less than 4, but there is a major jump at level 4. This is also the case for all conflicts, although when we include the minor conflicts there is also some evidence of increasing risk as abuses go from 1 to 2 and perhaps 2 to 3.²³

Table 4: Annual frequency of onsets by levels of lagged human rights abuses

PTS	major 1-yr lag	major 5-yr lag	all 1-yr lag	all 5-yr lag
1	0	0	.0059	.0044
2	.0046	.0094	.0249	.0335
3	.0092	.0045	.0408	.0309
4	.0202	.0181	.0927	.1069
5	.0387	.0127	.0718	.0506

According to Gibney, level 4 refers to countries in which “Civil and political rights violations have expanded to large numbers of the population. Murders, disappearances, and torture are a common part of life. In spite of its generality, on this level terror affects those who interest themselves in politics or ideas.” As compared to levels 1-3, this seems likely to pick up mainly countries with actual or incipient civil war.

Table 5 lists country years for which ACD codes the start of a civil war (a major conflict), and the PTS scale was 4 or greater in the previous year. There are a number of cases for which the high PTS score in the prior year is almost surely related to the fact that there was already a conflict in progress in the country. For a few of the others, it is plausible to see the conflict arising in some significant way from oppressive prior rule – for example, Cambodia 1978, Uganda 1978, and Yugoslavia 1998. But there are more cases where the lagged PTS coding is picking up incipient or residual civil war activity that is for different reasons missed by ACD – for example, Mozambique 1977, El Salvador 1979, Sri Lanka 1984, Pakistan 1995 and 2007, Rwanda 1997, Russia 1999, Iraq 2004, Israel 2006, and Somalia 2006.

²³These patterns hold up when we use a linear probability model (ordinary least squares with conflict onset as the dependent variable), and control for the other covariates of onset. Using robust (and country clustered) standard errors to try to deal with the heteroscedasticity that arises in the linear probability model, only levels 4 (and in a couple of case 5) are significantly different from level 1. In all of these models, I group 1.5 with 1, 2.5 with 2, and so on, for observations where the State Department and Amnesty disagreed so that there is an averaged value between integers.

Overall, the analysis suggests that very poor human rights performance is a *very* bad sign for a government: major civil conflict is then much more likely to begin, if it has not already started.

The causal, as opposed to diagnostic, role of government human rights abuses is less clear. It is plausible that government abuses would often encourage support for rebellion, but it is also possible that in some or many cases rebellion would be even more likely without the repression – else why is the government being so repressive? This is another “policy regression” problem. Cross-national data of the sort examined here cannot help us much in sorting this out.

There is, however, a growing literature on a closely related problem – what is the impact of indiscriminate counterinsurgency strategies on support and success of rebel movements? Much of this literature consists of case studies that suggest that indiscriminate brutality by government forces increased local support for (already existing) rebel groups. More recent work tries to identify natural experiments or to use fine-grained incident data from specific conflicts. Results so far are a bit mixed. Lyall (2009) finds, somewhat surprisingly, that random artillery shelling by Russian troops in Chechnya suppressed rebel activity. Lyall and Wilson (2009), who examine the effect of mechanized counterinsurgency on government success in a large sample of guerrilla wars and in a case study of Iraq, would seem to suggest the opposite, as does Condra and Shapiro’s (2010) incident study of Iraq.

Table 5: ACD war onsets in countries that had $\text{PTS}_{t-1} \geq 4$

country	year	$\text{war}_{t-1}?$	$\text{any conflict}_{t-1}?$	PTS_{t-1}	PTS_{t-5}
Mozambique	1977	0	0	4.0	
Uganda	1978	0	0	5.0	
Cambodia	1978	0	0	5.0	
El Salvador	1979	0	0	4.5	
Sri Lanka	1984	0	0	4.5	2.5
Iran	1986	1	1	5.0	4.5
India	1989	1	1	4.0	3.0
Iraq	1991	1	1	5.0	4.0
Bosnia	1993	1	1	5.0	
Pakistan	1995	0	0	4.0	3.5
DRC	1996	0	0	4.0	3.5
Rwanda	1997	0	0	5.0	4.0
Yugoslavia	1998	0	0	4.0	4.0
Angola	1998	0	1	4.0	5.0
Russia	1999	0	0	4.5	4.5
Iraq	2004	0	0	5.0	5.0
Somalia	2006	0	0	4.0	4.0
Israel	2006	0	1	4.0	4.5
Pakistan	2007	0	1	4.0	4.0

5.4 Political reform and civil war onset

The results in Table 1 show a strong relationship between *changes in governing institutions*, as measured by change in components of the Polity index, and an increased probability of civil war onset in subsequent years. The coefficient estimate is the same when we add country fixed effects, so it is not just a matter of more unstable countries having more civil wars. Rather, civil war somewhat reliably follows institutional changes in the direction of greater democracy or greater autocracy.

Does it matter whether the change is towards more democracy or autocracy? Previous work on civil war onset finds no significant differences, with a slight tendency for greater risk following autocratizing change (e.g., Fearon and Laitin 2003). With the formulation of the onset variables used here, we find a slightly stronger tendency for conflict to follow changes in an autocratic direction than in a democratic direction. This is shown in Table 6, where I have added to the core specification dummy variables marking whether there was a one-or-greater change in Polity from year $t - 2$ to $t - 1$ in the democratic direction and the autocratic direction (so that the excluded category is no change). Autocratizing change increases the odds of war onset in the next year by a factor of 2.54 on average, which is quite large. Democratizing change associates with increased odds of about 60%, though the estimate is not statistically distinguishable from zero. The results are similar though a bit smaller in magnitude for all conflicts.

The likelihood that democratic reform leads to civil conflict does not vary much across levels of development. The risk is slightly lower as one moves up the income scale, but not significantly so. Nor does it matter much what the initial level of democracy is. Civil war onsets have been somewhat more likely to occur following democratizing change in an autocracy or a democracy, as compared to an “anocracy” (a partial democracy). For all conflicts, minor and major, the risk of onset does not seem to vary much with initial regime type at all. One might have expected that democratic reform would be more likely to end in violent conflict the more authoritarian the reforming state. It is of course possible that there are more complicated paths between reform efforts and civil war risk. For instance, reform might lead to authoritarian retrenchment and higher risk of rebellion for that reason (e.g., Algeria in 1991, perhaps Thailand today). So these measures of democratizing and autocratizing change should be understood to be “short term,” and not likely to pick up more complicated political trajectories.

5.5 Rapid governance reform and conflict onset

Some case studies prepared for the WDR 2011 have suggested that rapid *administrative or political reforms* may actually heighten the risk of violent conflict in a country in the short run, even if the broader effects could be quite beneficial in the longer run. In this section I consider whether conflict onset is correlated with large positive changes in several governance indicators that might

Table 6: Change of political regime and conflict risk

	major wars	all conflicts
autoc. change	0.933** (0.348)	0.690** (0.265)
democ. change	0.481 (0.371)	0.324 (0.221)
$\log(gdp_{t-1})$	-0.454** (0.165)	-0.379*** (0.094)
$\log(pop_{t-1})$	0.205** (0.071)	0.245*** (0.060)
$\log(\% \text{ mountains})$	0.329*** (0.093)	0.149* (0.061)
new state	1.771*** (0.495)	1.429*** (0.356)
oil producer	1.089*** (0.310)	0.761*** (0.219)
anocracy_{t-1}	0.328 (0.277)	0.378* (0.187)
democracy_{t-1}	-0.199 (0.388)	0.146 (0.228)
ELF	0.475 (0.370)	1.080*** (0.296)
prior war		0.046 (0.242)
prior war	-0.533† (0.318)	
constant	-4.181** (1.370)	-3.898*** (0.908)
<i>N</i>	7836	7780

Robust standard errors in parentheses

Regime changes are measured from year $t - 2$ to $t - 1$.† significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

potentially pick up attempts at major administrative reform.

The “reform” indicators used here are drawn from two main sources, both of which are presented and discussed in more detail in the section below on governance and conflict. The first set come from the International Country Risk Guide (ICRG), a company that sells assessments of business conditions and political risk to investors. They produce a number of indices that attempt to tap various dimensions of political risk, and that appear to be derived primarily from expert surveys. I

try to identify major efforts at administrative reform by coding for large, fairly rapid changes in the ICRG's corruption index, "bureaucratic quality" index, and a variable called "military in politics" that is said to measure "the degree of military participation in politics" (higher numbers indicate less military involvement). The corruption and "military in politics" measures are scales from 1 to 6, while bureaucratic quality ranges from 1 to 4; higher values are better. We have annual data from 1984 to 2006 for between 110 and 139 countries. Unfortunately, relatively conflict-prone countries are more likely to be missing.

Table 7 lists the top 15 country years for two-year increases in the ICRG corruption, bureaucratic quality, and military in politics measures. That is, columns 3, 6, and 9 in the table show the change in the scale from year $t - 2$ to t , and these countries saw the largest such changes in a two-year period in our sample. Inspection suggests that a large number of these are cases of substantial reform during, or in the wake of, a successful episode of democratization. In some cases, it is reform following the end of a civil war as well.

Inspection also suggests that very few of these country years saw, or were soon followed by, the onset of violent conflict. Following major improvements in government corruption (the first three columns), only Liberia 2001 saw new armed conflict soon after, and this does not seem like a compelling example of an anti-corruption drive stirring up conflict. On bureaucratic quality, there is Angola 1996 (new war in 1998) and Algeria 1989-90; the usual narratives for these cases do not mention destabilizing impacts of administrative reform, though clearly blocked democratization in Algeria was a part of the sequence of events leading to the violence of the 1990s. The largest and most rapid reductions in the political role of the military (columns 7-9) clearly tend to occur after democratization and often civil war. The only contender here would be Yugoslavia 1997 (war in 1998).

Table 7: Top 15 reform episodes by ICRG measures

country	year	Δ corruption	country	year	Δ bur. qual.	country	year	Δ mil. in pol.
Bahamas	1994	4.00	Algeria	1989	2.00	Niger	2001	4.00
S. Korea	1992	3.00	Angola	1996	2.00	Algeria	2005	3.00
Paraguay	1992	3.00	Indonesia	1993	2.00	Chile	1991	3.00
Liberia	2001	2.50	Panama	1998	2.00	Guatemala	2005	3.00
Indonesia	1992	2.42	Poland	1991	2.00	Israel	1992	3.00
Bulgaria	1993	2.00	Paraguay	1993	1.75	Mali	1994	3.00
Chile	2006	2.00	Ghana	1990	1.67	Korea	1990	2.92
Egypt	1993	2.00	Uruguay	1997	1.67	Poland	1990	2.92
Ghana	1990	2.00	Malta	1996	1.50	Lebanon	1992	2.92
Guatemala	1997	2.00	Namibia	1992	1.46	Haiti	1996	2.75
Guyana	1995	2.00	Gabon	1986	1.45	Peru	2002	2.67
Lebanon	1994	2.00	Kenya	1986	1.36	Namibia	1993	2.50
Mali	1994	2.00	Kuwait	1993	1.33	Sierra Leone	2003	2.50
Nicaragua	1986	2.00	Philippines	1997	1.33	Yugoslavia	1997	2.50
Philippines	1987	2.00	Egypt	1986	1.09	Kuwait	1993	2.42

Numbers are change in governance index from $t - 2$ to t .

So there is no strong suggestion here that the most rapidly improving states on corruption, bureaucratic quality, and role of the military (as coded by the ICRG) have often been followed by increased civil conflict. Of course, we should really check the larger sample that has variation on the independent variables. For this purpose I construct dummy variables marking country years where the two-year increase or decrease in the relevant governance indicator is in the top and bottom 10% of all changes in the variable. The dummy marking large increases in the variable thus codes “reform” and the dummy marking a large decline marks “regression.” I then added lagged versions of these variables two at a time to the baseline cross-national model. So, for example, for corruption, I add a dummy marking country years such that the improvement in the corruption index from $t - 3$ to $t - 1$ was in the top 10% of all changes in this measure, and also a dummy marking country years where the corruption score fell so much from $t - 3$ to $t - 1$ that the change was among the 10% largest drops. This means that the coefficients mark the effect of a large change against a baseline of zero or little change in the last several years.²⁴

Table 8 shows the results, for models where the dependent variables are major conflicts and for all conflicts. There are no reliably significant results and no consistent patterns. There is a huge, negative estimated coefficient for positive change in the corruption index, which means that rapid improvement in the corruption index associates with *lower* odds of civil war onset. But not much weight should be put on this because there are only 18 major conflict onsets in this ICRG subsample, and this variable comes just one case shy of perfectly predicting zero major onsets.²⁵

²⁴I also checked results using unlagged versions of these variables; this made no consistent differences.

²⁵If we use a linear probability model instead of logit, there is little difference in estimated odds.

Apart from this instance, we do not see strong evidence that rapid improvement on these governance indicators associates with increased risks of instability. The estimated coefficients are weakly positive for improvements in bureaucratic quality and military in politics, but not close to significant (except for major conflict onset and military in politics).

Table 8: War/conflict onset and ICRG gov. reform and regression indicators

gov indicator	reform/regression	ACD wars		all ACD	
		coef.	se	coef.	se
corruption	big + change	-6.54	0.35	-0.33	0.36
	big - change	-0.74	1.06	-0.58	0.37
bur. qual.	big + change	0.37	0.57	0.13	0.31
	big - change	-0.74	0.98	-0.98	0.47
mil. in pol.	big + change	1.07	0.52	0.28	0.27
	big - change	0.61	0.64	0.32	0.26

Indicators are dummies for top and bottom 10% on lagged two yr change.

Coef's from model with controls for prior war, income, population, mountains, oil, instability, anocracy, democracy, and ELF.

I next consider four component variables that go into the World Bank’s Country Policy Institutional Assessment indicator: quality of public administration (qpa), quality of budget and financial management (qbfm), policies for gender equality (gender), and equity in public resource use (epru). These are scored on 1-to-6 scales, but we have the data only for 1998 through 2008, and only for countries that received or have received IDA financing. This is a short time series for variables that change little over time within countries (around 80% of the variance is across countries for these variables and these 11 years). We are also limited by the fact that the dependent variable, conflict, is quite rare for any given country in an 11-year window.

Table 9 repeats the exercise of Table 8 for these variables, using a logit model where the dependent variable is the onset of any level of ACD conflict. There are too few major conflict onsets to estimate a logit model for this sample, and even for all onsets it is not advisable to estimate a model with the full set of controls, due to the problem of overfitting. The model used here includes as controls prior war in progress, lagged log of per capita income, lagged log of country population, and ethnic fractionalization. In all the estimations, these take their usual signs (negative, negative, positive, positive), and are either statistically significant or close to it. This is despite the fact that the number of onsets in these models varies between 24 and 30, out of between 780 and 1000 country years.

The same cannot be said for the dummy variables marking positive or negative change in each of the four CPIA governance indicators. The thresholds used here are the top 15% for improvements and the bottom 15% for “regressions” in governance, over a two year period and lagged one year.

We see that a large positive change in quality of public administration and quality of budget and financial management is followed by an elevated average onset risk, but the “effects” are not statistically significant (though somewhat close for qbfm). Large improvement in gender policies is a nearly perfect predictor of *absence* of subsequent conflict onset, but little or nothing should made be of this (in my opinion) due to the small sample and risk of overfitting. Improvement in equity of public resource use associates with a slight decline in risk of conflict onset in the next year, but again this is not statistically distinguishable from zero effect. So no very consistent pattern emerges regarding the impact of a large, rapid positive change and conflict risk for these four indicators.²⁶

Table 9: All ACD conflict and CPIA gov. reform and regression indicators

gov indicator	reform/regression	coef.	se	p value
qual. pub. admin.	big + change	0.7	0.87	0.42
	big – change	-0.25	0.64	0.69
qual. budget/fin.	big + change	1.06	0.68	0.12
	big – change	0.58	1.14	0.61
gender	big + change	-6.07	0.4	0
	big – change	-1.01	0.99	0.31
equity resource use	big + change	-0.24	1.16	0.83
	big – change	-1.39	1.04	0.18

Coef's from logit model for all conflicts, 1998-2008.

Controls: prior war, log(income), log(pop), ELF.

So at least for these seven indicators (three from ICRG and four from CPIA), we find no indication that rapid administrative or military reform is consistently followed by an elevated risk of the outbreak of violent conflict. To the contrary, rapid reform often seems to accompany major and successful episodes of democratization, which might for a variety of reasons mark countries that are less likely to have a conflict begin. It is possible that case studies selected on certain conflicts might give a misleading impression of general patterns, even if it is possible that in some cases rapid reform does elevate conflict risk.

5.6 Natural resources

As seen in Table 2, oil producers are on average at much greater risk of war and conflict onset – the relative risk is about 3 for wars, and about 2 for all conflicts, using those estimates. The estimates are stable even with fixed effects for major conflicts, although statistical significance declines and

²⁶I note also the results are somewhat unstable if we construct the variables the same way but don't lag the dummy variables (so looking at the impact of change from $t - 2$ to t instead of $t - 3$ to $t - 1$), or if we vary the threshold window. This is not surprising given the sparsity of the onset data for an 11 year period.

there is less evidence for minor conflicts.

The measure used in Table 2 is from Fearon and Laitin (2003), and marks country years in which at least one third of GDP was from fuel exports, based primarily on World Bank numbers. This measure is open to various criticisms, and there are other plausible measures one could try. As to criticism, a number of researchers have worried that oil exports as a percentage of GDP brings the rest of the economy into the measure, so that it might be capturing an effect of prior conflict or the anticipation of future conflict on GDP rather than a causal effect of dependence on oil production on conflict. That is, conflict or its anticipation might cause greater resource dependence due to lack of investment and greater mobility of the non-oil sector.

Humphreys (2005) developed measures of oil production per capita and oil reserves per capita, which could lessen endogeneity concerns by removing GDP from the denominator. He again found support for a correlation between both oil production and oil reserves per capita and civil war risk, using panel data but no fixed effects.²⁷

Ross (2006) improved on Humphreys' data and also used data on fuel rents per capita produced originally by Hamilton and Clemens (1999). He again found a significant relationship between oil measures and conflict onset, in a large number of panel regressions that considered five different civil conflict codings and also whether the conflicts were separatist or over the center, and ethnic or ideological. In addition, he finds that fuel rents from off-shore oil deposits are unrelated to conflict risk.

Overall, the evidence for a correlation between oil and conflict thus appears fairly good, although this is not based on a lot of data and results with fixed effects are fragile and often non-existent. It is also not clear what the most important mechanisms are (if there is a causal relationship). Ross (2006) argues that the cross-national patterns and case-study evidence strongly support the idea that oil wealth sometimes encourage separatist and other regional rebellions to gain more local control. He observes, however, that the correlation still obtains when we restrict attention to center-seeking (non-separatist) civil wars, so the separatist mechanism is probably not the whole story. As mentioned in the section on causes below, state weakness (relative to per capita income) could be a factor, as could greater inequality (that is, higher prize value for capturing the central government).

Researchers have also spent quite a bit of time on the question of whether precious gems and minerals associate with higher conflict risk. Ross (2006) reviews and revisits this question as well, using updated and improved data to estimate annual gem rents per capita, both for primary

²⁷Brunnschweiler and Bulte (2009) try to instrument for resource dependence, and find when they do that they can eliminate a significant effect for resource dependence variables. In my view their set of instruments could not possibly satisfy the exclusion restriction necessary to allow us to infer that they are estimating the causal impact of resource dependence on conflict risk.

and secondary diamond sources. At least since Billon (2001) it has been conjectured that alluvial diamonds (secondary) are more conflict-causing than diamonds from deep mines (primary), which are more easily controlled by the state. However, contrary to some earlier studies (e.g., Lujala et al. 2005), Ross finds fairly strong evidence of association between primary diamonds and conflict risk; the evidence for secondary diamonds is much weaker. He notes, appropriately, that in both cases these are generalizations based on rather few countries, considerably fewer than even the case of oil.

The literature on natural resources and conflict was initiated and stimulated by early versions of Collier and Hoeffer (2004), the main argument of which was that the significance they found for a measure of primary commodity exports as a share of GDP indicated that civil wars were to be explained mainly by the financing opportunities available to rebel groups. This measure, which combines cash crops and oil but leaves out minerals, proved not to work very well as a predictor outside of the particular Correlates-of-War based civil war list used by Collier and Hoeffer. For example, when this measure (and/or its square) is added to the models in Table 2 above, the estimated coefficients are negative, usually with large standard errors.

5□ Geography

As seen in Table 2, a measure of rough terrain is associated with higher probabilities of civil conflict in the global sample. If one looks region by region, one finds that the measure takes a positive and fairly substantial coefficient in all regions (excluding the West, where there are few cases of conflict), and actually manages statistical significance in a number of them. The same pattern is evident for all ACD conflicts, though somewhat weaker.

The measure used here, as in most studies in the literature, is based on estimates of the percentage of each country that is “mountainous” as judged by geographer John Gerrard, who was commissioned by Paul Collier to construct the measure when Collier was at the World Bank.²⁸ Since that time, Geographic Information Systems tools have made it possible to construct measures of rough terrain based on satellite imagery. Surprisingly, there are no published studies that use this approach to reassess the rough terrain factor at the country level (as far as I know), although Buhaug, Gates and Lujala (2009) and ... have constructed GIS-based terrain measures at the level of “conflict areas” within countries.

Matthew Kocher (2004) developed a country-level GIS-based rough terrain measure, by computing the standard deviation in elevation of 1-by-1 kilometer cells and their neighbors for each country. His measure is the percentage of cells in a country with neighbor standard deviation of at least 50 meters. This measure is correlated at .81 with the Gerard measure (in logs). When used in place

²⁸Fearon and Laitin (2003) produced estimates for additional countries by using the difference between the highest and lowest elevation in a country, which turns out to be well correlated with Gerard’s numbers.

of the Gerard measure in the models in Table 2, the results are actually somewhat stronger overall: the coefficients and significance levels are slightly larger for models 1 and 2 (major and FL civil wars), and marginally smaller for all ACD wars.

As discussed more in the next section, there are multiple ways to interpret this association. It could be that it is easier for the state to deter or eliminate nascent rebel groups in a relatively flat state, because it is harder to hide. Case studies of *any* conflicts point to how guerrilla groups have survived by taking to the hills, or the jungle. Or it could be that, on average, central states have developed less administrative control of the periphery in places where the physical obstacles to extending government authority are relatively great. Or, it could be that because of the low presence of a strong central state, countries with rough terrain tend to have ethnic or religious groups with “cultures of honor” and/or a deep distrust and dislike of lowland authority. Or it could be a combination of these factors, or others still.

Gallup, Sachs and Mellinger (1999) have argued that being landlocked causes a country to be poorer and have a harder time developing economically, basically because of greater transportation costs. One might also wonder if landlocked countries are more likely to have civil conflicts. Landlocked countries tend to have several other risk factors associated with conflict, as shown in Table 10. On average they are poorer, more mountainous, and more ethnically fractionalized (however, they are also smaller and less likely to be oil producers on average).

Table 10: Average values for landlocked versus other countries

	Not landlocked	landlocked
per cap income	\$8,691	\$5,714
population	33m	7.1m
% mountains	16	30
oil producer	0.147	0.048
political instability	0.096	0.115
anocracy	0.235	0.255
ELF	0.366	0.515
share of major war years	0.092	0.088
freq of major onsets	0.011	0.009
share of all conflict years	0.195	0.132
freq of all onsets	0.033	0.028

Entries are averages over all country years with data.

It is perhaps surprising, then, that since 1946 landlocked countries have actually been somewhat *less* conflict prone, both in terms of numbers of major and minor conflict onsets, and in terms of number of years with major or minor conflict. This bivariate observation holds up when we add a dummy variable for “landlocked” the models in Table 2. The coefficients remain negative. For

example, landlocked countries are estimated to have had 40% lower annual odds of major conflict onset, with a p value of .12 in model 1, controlling for the other factors.

The absence of a positive relationship, and the possibility of a negative one, does not appear to result from outliers or region-specific effects. Partially excepting Latin America (where landlocked Paraguay and Bolivia had some violent conflicts in the late 40s and 50s, a long time ago), region by region there is a tendency for the landlocked countries to have experienced less violent conflict. In Eastern Europe and the FSU: Hungary, Czechoslovakia, Belarus, Armenia, all the “Stans” except Tajikistan. In Africa, the landlocked countries of the Sahel have been generally more peaceful than the coastal West Africa states, and the landlocked countries of southern Africa (Zambia, Malawi, Botswana, Lesotho) have been more peaceful than most of their neighbors with coasts. In Asia, Afghanistan and Nepal have seen a lot of conflict, but not so for Mongolia and Bhutan.

Whether there is any causal connection here is not clear. I have trouble even thinking of a plausible story for why being landlocked would by itself cause a country to be less prone to internal conflict. (On the other hand, it is not obvious why landlock would cause greater conflict risk either.)

One other geographic factor deserves mention: Evidence from multiple regions and time periods suggests that within countries, groups farther from the center of power are more likely to see armed conflict with the center than groups closer to the center. Tong (1988) found this to be strongly the case for rural rebellions by Chinese peasants in the Ming Dynasty. Brustein and Levi (1987) present very similar patterns for rebellion in early modern Europe. In the present day, using GIS data Condra (2009) and Buhaug, Cederman and Rød (2008) find that distance from the capitol predicts higher rebellion risks for countries in subSaharan Africa and in Eurasia, respectively.

5 □ Gender inequality and civil conflict

Caprioli (2005) and Melander (2005) find several cross-national measures of gender inequality to be related to civil war, in the context of incidence models where the dependent variable is whether there was conflict in a country year.²⁹ This subsection considers what happens when some similar measures are added to the onset models in Table 2.

The measures examined are total fertility rate (births per adult woman), share of parliament who are women, and the ratio of girls to boys completing primary school. The first two are drawn from the World Development Indicators. For the primary school ratio I use two different series: the education data in the WDI, and the data produced by Robert Barro and Jong-Wha Lee (2000). Coverage varies quite a bit. Fertility data is available for most countries at five-year intervals from 1960; I interpolate for the intervening years. The first estimates for women in parliaments are for

²⁹It is not clear how Caprioli handles the issue of duration dependence. Melander uses cubic splines; I have doubts about whether this method makes sense with incidence data.

1990, and then are provided each year from 1997 for most countries. The WDI primary education data essentially starts in 1975 for a reasonable cross-section, then coming in five-year periods till the 2000s. The Barro and Lee series, which has been very widely used in research on economic growth, has data for only about 104 of the countries in our sample, starting in 1960 and for five year periods to 2000-04. I interpolate estimates for the missing years in both cases, and extend the 2000 Barro-Lee estimates through 2009.

Table 11 shows correlations between these various indicators and income, both in raw form and after netting out income (that is, looking at the correlations of the residuals after regressing on log income). Not surprisingly, like so many aspects of “modernization,” gender equality is strongly related to per capita income. As with other variables like population growth or “youth bulge,” collinearity will make it difficult to estimate the conditional expectation of civil war onset with respect to gender equality measures. Share of women in parliament is the least well correlated with income, perhaps reflecting international influences on new constitutions. After netting out per capita income, the correlations are much weaker, and disturbingly so between the two measures of sex ratio of primary school students, which ostensibly measure the same thing. Still, the reasonable correlations between total fertility and the education measures after netting out income suggest that these indicators may be tapping something like a common dimension on gender policies and the standing of women.

Table 11: Correlations among gender inequality indicators

	correlations				corr. net of income		
	log(gdp)	iwmpri	wmpc	ifert	iwmpri	wmpc	ifert
iwmpri	53						
wmpc	53	64			44		
ifert	-68	-66	-53		-48	-21	
iwmparl	27	21	20	-29	6	5	-12

iwmpr = interpolated WDI sex ratio in primary school
 wmpc = interpolated Barro-Lee sex ratio, primary completion
 ifert = interpolated WDI fertility rate
 iwmpri = interpolated share women parliament

Tables 12 and 13 add these measures to the basic onset models of Table 2, for all conflicts and for major conflicts as the dependent variables.³⁰ The results are quite variable from indicator to indicator and depending on which level of conflict we look at, although overall there is some indication that higher levels of gender equality associate with a lower propensity for conflict.

³⁰It is important to keep in mind that because of variation in coverage, some of the variation in estimates is due to the samples changing from model to model.

Higher fertility rates are positively related to conflict and war onset, but the estimates are not statistically significant and are substantively close to zero. The share of women in the national legislature is negatively related to onset both for all and for major conflicts, but estimated coefficients are radically different in magnitude and only significant (statistically and substantively) when the dependent variable is all conflicts. The ratio of girls to boys in primary school is significantly related to lower odds of all and major conflicts when the measure is from the WDI, but not when the measure is from the Barro and Lee data.

Since per capita income, ethnic fractionalization, democracy, and each of the four measures of gender inequality are fairly well correlated, it is pushing the data very hard to include them in the same statistical model. Some of the instability of the results (and note also that the coefficients for income drop a lot in some cases when one of these gender measures are added) is due to overfitting the one or two onsets that may or may not show up in the “off diagonal” cells (e.g., low income, high gender equality), depending on the specific measure employed. For example, the WDI primary school ratio gains strength from the fact that ACD conflict are recorded for Iraq, Iran, Lebanon, Oman, and Saudi Arabia, all of which are relatively high income (for conflict countries) but don’t do well on the gender ratio in primary school; the Barro Lee measure is missing for some of these cases or is somewhat different.

Overall, however, the results are at least somewhat supportive of the proposition that even comparing countries at similar levels of economic development, those that have more women in parliament or more gender equality in school attendance tend to be somewhat less likely to have violent civil conflicts break out. If this is accepted, the next question is how to interpret the relationship. It seems clear that we cannot infer that if a government implements greater gender equality as a result of international pressure or inducements, this would lower conflict risk. For all we know, this could *increase* risk by increasing frictions between “traditional” and “modernist” factions.³¹ This relationship, if it is meaningful, is particularly likely to have an interpretation such as “societies that are okay with gender equality are the sort of societies that are less likely to have civil wars,” rather than that the one causes the other in some straightforward way.³²

6 Causes of civil war

Patterns of correlations like those presented above have been the basis for a new round of academic and policy-maker arguments about the causes of civil conflict that started after the end of the Cold

³¹Of course, it still might be worth doing, for the sake of improving the lot of women and girls in a society.

³²There is fairly little within country variation in these measures of gender equality, so that, not surprisingly, fixed effects models yield no significant coefficients.

Table 12: Gender inequality measures and all ACD conflicts

	Model 1	Model 2	Model 3	Model 4
fertility	0.05 (0.08)			
share women parl.		-5.00* (2.17)		
WDI prim. sch. ratio			-1.48* (0.64)	
BL prim. sch. ratio				-0.67 (0.47)
ln(gdp) _{t-1}	-0.26 [†] (0.14)	-0.47** (0.18)	-0.21 [†] (0.12)	-0.34* (0.16)
ln(pop) _{t-1}	0.27*** (0.07)	0.33** (0.10)	0.30*** (0.08)	0.30*** (0.07)
ln(% mountains)	0.16* (0.06)	0.08 (0.10)	0.14 [†] (0.07)	0.07 (0.07)
oil producer	0.71** (0.24)	1.21*** (0.35)	0.64* (0.29)	1.02*** (0.29)
new state	1.38*** (0.36)		2.30*** (0.52)	0.01 (0.72)
pol instab _{t-1}	0.36* (0.17)	0.11 (0.30)	0.41* (0.18)	0.28 (0.19)
anocracy _{t-1}	0.49* (0.20)	-0.27 (0.38)	0.29 (0.26)	0.36 (0.26)
democ _{t-1}	0.05 (0.26)	-0.04 (0.35)	-0.03 (0.30)	0.21 (0.28)
ELF	1.15*** (0.33)	1.99*** (0.54)	0.98** (0.35)	1.08** (0.40)
prior war	0.10 (0.24)	-0.12 (0.34)	0.04 (0.26)	0.06 (0.29)
constant	-5.33*** (1.58)	-3.42* (1.69)	-4.21*** (1.09)	-4.20** (1.29)
N	6429	2496	4854	4886

Standard errors clustered by country in parentheses

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ War.³³ In early versions of their influential paper “Greed and Grievance in Civil War,” Collier

³³There is of course a long history of theorizing about civil war and revolutions before this, mainly on the basis of particular cases (e.g., Moore 1966, Skocpol 1979), but including an earlier round of quantitative

Table 13: Gender inequality measures and ACD major conflict onsets

	Model 1	Model 2	Model 3	Model 4
fertility	0.10 (0.11)			
share women parl.		-0.58 (1.74)		
WDI prim. sch. ratio			-2.33** (0.88)	
BL prim. sch. ratio				-0.84 (1.01)
ln(gdp) _{t-1}	-0.14 (0.20)	-0.62* (0.27)	-0.11 (0.22)	-0.41† (0.23)
ln(pop) _{t-1}	0.24** (0.09)	0.27 (0.22)	0.27* (0.11)	0.22* (0.09)
ln(% mountains)	0.34*** (0.10)	0.19 (0.24)	0.35** (0.12)	0.27* (0.11)
oil producer	0.92** (0.33)	1.73*** (0.45)	0.94* (0.41)	1.44*** (0.33)
new state	2.30*** (0.48)		2.47*** (0.69)	0.36 (1.09)
pol instab _{t-1}	0.63* (0.32)	-0.20 (0.79)	0.48 (0.39)	0.27 (0.38)
anocracy _{t-1}	0.48 (0.30)	0.56 (0.72)	0.40 (0.45)	0.44 (0.33)
democ _{t-1}	-0.45 (0.43)	-0.78 (0.84)	-0.23 (0.47)	-0.09 (0.43)
ELF	0.92† (0.48)	-0.04 (0.74)	0.43 (0.55)	0.78 (0.51)
prior war	-0.64† (0.35)	-0.64 (0.85)	-0.64 (0.44)	-0.68† (0.37)
constant	-7.74*** (2.26)	-3.28 (3.70)	-5.59*** (1.59)	-4.16* (1.65)
N	6429	2496	4854	4886

Standard errors clustered by country in parentheses

† significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

and Hoeffler (2004) interpreted the strong association they found between civil war onset and measures of dependence on primary commodity exports and low education levels – versus the

cross-sectional analysis (e.g., Hibbs 1973).

weak association of onset with measures of ethnic diversity, income inequality, and democracy – as supporting the argument that rebel groups are primarily motivated by opportunities for profit rather than by a desire to right perceived wrongs. They suggested that aspiring “grievance-based” rebellions might face a more severe collective action problem than would “greed-based” or “loot-seeking” rebellions. In this initial formulation, the implicit assumption was that the causes of civil war would be located in the *motivations* of rebel groups.

Later versions of the paper converged (at least on this point) with the interpretation in Fearon and Laitin (2003), who argued that “Surely ethnic antagonisms, nationalist sentiments, and grievances often motivate rebels and their supporters. But such broad factors are too common to distinguish the cases where civil war breaks out” (p. 76). The idea is that grievances that could potentially motivate a rebellion are regrettably common (and reasonable) in much of the world, so that more of the “action” in terms of explaining cross-national variation in civil war propensities is likely to be found in variation in factors that affect the viability of, or *opportunity* for, rebellion.

Very few civil wars since 1945 have been, or have emerged out of, popular revolutions characterized by mass protests and mass action – the French revolution model. Instead, the vast majority of violent civil conflicts in this period have been fought as guerrilla wars or militia-based conflicts, typically by small rebel groups that often number in the hundreds, especially in their early years. Fearon and Laitin suggested that “because insurgency can be successfully practiced by small numbers of rebels under the right conditions, civil war may require only a small number with intense grievances to get going” (p. 76).

The prevalence of small-scale guerrilla or militia-based wars does not rule out the possibility that variation in the level of broad social grievances across countries could be an important explanation for civil war risk. In principle, it could be that social support from many sympathetic people is necessary for even a small rebel band to operate successfully. In practice, however, if this is the case it seems to apply mainly to rebellions in countries where the central state is relatively capable. Where states are less capable, rebel groups often appear to be able to operate without broad or deep social support, or they can coerce it.³⁴

Fearon and Laitin (2003) give mainly “opportunity” interpretations for the patterns of correlation described in sections 4 and 5 above. For example, when a colony receives independence, its central government receives a negative shock to its capability to deter and fight rebels, if and when the colonial army leaves. Likewise, political instability may signal weakness at the center, as may anocracy (partial democracy) (Hegre, Ellingsen, Gates and Gleditsch 2001). Large populations are suggested to make rebellion more feasible on average by making the governance problem harder

³⁴Cf. Weinstein (2007). Drawing on an exhaustive reading of “micro-level” accounts of particular civil wars, Kalyvas (2006) provides almost innumerable examples of how the relative local strength of rebel and government forces shapes local support for the rebels versus the government, more than the other way around.

for the center (harder to develop reliable chains of principals and agents to monitor what is going on at the local level; India is much harder to govern than, say, Mauritius). Rough terrain is argued to associate with better opportunities for rebel groups to hide, and, often, less historical development of central administrative structures throughout a region.

Along similar lines, Collier and Hoeffler (2004) found strong results for a variable measuring the share of primary commodity exports in GDP. In the 2004 version they interpreted this as an indicator of greater financing opportunities for would-be rebel groups, principally through extortion of producers. They also find some evidence that having a large minority diaspora in the US associates with greater likelihood of war renewal, which they interpret as evidence that diaspora funding provides opportunities for rebel groups to return to war.

As we saw above, lower per capita income is strongly related to a higher propensity to have civil wars and conflicts across countries (although there is not much evidence that economic growth or decline within countries over time strongly predicts change in conflict risk). Two main causal interpretations of this pattern have been advanced in the literature. Collier and Hoeffler (2004) stressed a *labor market* explanation: low income, they hypothesize, means lots of underemployed youth who find the opportunity costs of joining a rebel band to be small. Fearon and Laitin (2003), by contrast, stressed a *state capabilities* explanation. In this view, per capita income is largely a proxy for state administrative, military, and police competence, and thus the ability to deter or defeat nascent insurgencies.³⁵

The state capabilities explanation is arguably more consistent with the lack of a robust within-country relationship between income and civil war: one wouldn't expect a lock-step relationship between state capabilities and income, whereas short-run income changes should affect labor market conditions. The labor market explanation might be more consistent with Miguel, Satyanath and Sergenti (2004), who found that exogenous variation in rainfall is related to civil war onset in sub-Saharan Africa. They argue that a direct causal effect of income on civil war propensity can be identified by using the variation in income related to variation in rainfall, on the assumption that the only way rainfall variation affects civil war propensity is through income. As they note, it is possible that heavy rainfall might provide a negative shock to state capabilities by making roads less passable. But it is perhaps more likely that the effects would go through economic channels.

³⁵Both papers mention the alternative stressed by the other as a possibility. Fearon (2008) develops a model of individual decisions to join a rebel movement, noting that the standard opportunity cost argument neglects that while there may be less to lose by joining a rebel band in a poor country, there is also less to gain. Unless one assumes a somewhat unusual form of utility function (increasing relative risk aversion), the propensity to join a rebel group will be independent of per capita income in the country. An alternative explanation, stressed in that paper, is that in more modern economies it may be more difficult for rebel groups to extract wealth using typical tactics, given that much of it is virtual wealth held in bank accounts, human capital, and businesses that are more mobile than are farmers in poor countries.

As they are typically stated in the empirical literature, “opportunity” arguments are incomplete and not coherent as explanations for civil war. Simply noting that some factor makes rebellion more likely to succeed or be self-sustaining does not explain why it would occur. If this factor – such as a weak central state, or lots of unemployed youth, or rough terrain, etc. – makes would-be or actual rebel groups stronger relative to the central government, then why doesn’t this just change the terms of implicit or explicit deal that the government offers to avoid a costly conflict?

Explanations for civil war that fully address this question (which also arise for “grievance” stories) remain relatively rare in the literature; this is an area where further empirical progress probably depends a lot on theoretical progress.³⁶ The most developed answer in the academic literature proposes that shocks to the relative capabilities of rebel groups and governments can create a commitment problem that renders bargained solutions hard to reach and implement (Acemoglu and Robinson 2001; Fearon 1994; Fearon 2004; Walter 1997). The idea is that shocks to the relative capability of actual or potential rebel groups create “windows of opportunity” that cannot be bargained away because the government would have an incentive to renege on the deal after the window closes and the rebel group’s opportunity declines. This problem is most acute during civil wars, since disarming in a peace deal may leave one or both sides highly vulnerable to reneging by the other (Walter 1997; Walter 2002). But it can also explain civil war onset. For example, armed groups may have a window of opportunity to rebel successfully (either to take the center or gain some regional autonomy) in the first few years of independence; the center may not be able to credibly commit to a long-term policy of favoring the region or satisfying the rebel’s demands, if a peace deal would in the long run strengthen the central government. More generally, the idea is that in poor countries with relatively weak central governments it will take less of a shock to the relative strength of center versus opposition to get a would-be rebel group over the threshold that makes fighting a worthwhile gamble (Fearon and Laitin 2007).

A second possible approach to explaining the puzzle of why governments aren’t always successful at deterring and buying off violent challenges would put the stress on incomplete information about what it will take. Perhaps governments sometimes just guess wrong about what sort of redistributive and other policies will be enough to prevent the mobilization of armed opposition. There is then a further puzzle, in that if a government guesses wrong and armed conflict begins, why doesn’t it then revise its beliefs and propose or implement a better deal, so that civil “conflict” would be so short as to barely qualify? Arguments (and questions) of this sort are more developed in the analysis of interstate conflict,³⁷ and some of them may transfer straightforwardly to the domestic setting (e.g., Cetinyan 2002). But this is not so clear, and it is also not clear how this approach would explain the pattern of empirical findings. One possibility is that bargaining is more likely to fail due to private information in poor countries because governments are less well informed about popular or minority group preferences. Another is that weaker central states imply

³⁶Blattman and Miguel (2010) similarly note the underdevelopment of theory in this area.

³⁷For example, Fearon (1995, 2007), Powell (1999), and Wagner (2000).

that misjudgments of relative strength by rebel groups are more likely. Or perhaps the “prize” value of capture of government (or regional autonomy) versus living in the regular economy is greater on average in less developed countries, so that the bargainers are willing to make aggressive offers that run a greater risk of costly conflict.

Theoretical arguments about “grievances” as causes of civil conflict are even more underdeveloped than arguments about opportunities. As we saw above, a number of the variables that come to mind first as possible measures of “societal grievance” – such as lack of democracy, income inequality, and various formulations of ethnic or religious demography – do not show a strong or consistent relationship with a country’s propensity for civil war onset. But it is hard to know how to interpret this. One possibility, of course, is that these are not good measures of “average level of grievance,” and if we keep trying, we will find something better that “works.” Research discussed above by Østby, Cederman, and others has pushed in this direction, by attempting to come up with better measures for “horizontal inequalities” and “ethnic exclusion” from political power.

Another possibility is that the enterprise of trying to correlate measures of societal grievance with civil war risk is misguided, because “grievance” is a function of government policies, which are themselves chosen partly in light of civil war risk. Thus we can’t learn about the importance of “grievances” relative to “opportunities” by looking at cross-national data, or from time-series within countries unless we have some plausibly exogenous variation in grievance (which is hard to imagine). For example, as noted above, the exercise of correlating “ethnic exclusion” with civil war risk might either underestimate the causal effect of policies of exclusion, if governments choose what they can get away with, or overestimate it, if governments tend to exclude those minorities that are more threatening based on “opportunity” variables such as relative strength. A more developed theoretical analysis is probably necessary to allow us to see how to conceptualize and understand the relationship between “grievance” and relative power and cost factors, rather than posing them as distinct.

7 Governance indicators and civil conflict

As discussed above, several of the most striking cross-national patterns in civil war onset might be explained by an interpretation that puts “state capabilities” at the center of the story. In this view, low per capita income is strongly related to conflict onset because it is a proxy for the central state’s capability to deter and suppress armed challengers, and possibly also to provide public services. A set of alternative interpretations argues that there is a direct effect of low income on civil war propensity through some labor market channel. For instance, it is argued that poverty makes joining a rebel group relatively more attractive for young men.

This section uses three sets of governance indicators – the Worldwide Governance Indicators produced by Kaufmann, Kraay and Mastruzzi (2009), the International Country Risk Guide (ICRG)

indicators, and the World Bank's aggregate Country Policy Institutional Assessment (CPIA) – to try to better assess the “state capabilities” argument about civil war onset.

The ICRG series, which starts in 1984, is produced and sold by the company Political Risk Services; the variables are derived from expert surveys of business and political conditions in about 140 countries. The WGI project began in the 1990s at the World Bank. Kaufmann and Kraay assembled a large set of expert-based governance ratings produced each year by think tanks, academic research groups, NGOs, international organizations, and businesses, and divided them into sets that they argue correspond to six dimensions of governance: “government effectiveness,” “voice,” “political instability,” “rule of law,” “corruption,” and “regulatory quality.”³⁸ They then use techniques akin to factor analysis to extract a common dimension in each area. This has yielded a panel for 212 countries and territories for 1996 to the present (but not including 1997, 1999, and 2001).

Since 1977, each year World Bank staff have coded Bank client countries on 16 or more dimensions concerning the quality of policies and institutions. These codings are then aggregated to a summary measure called the Country Policy and Institutional Assessment, which is used for various purposes including decisions about aid allocation. The aggregate index varies from 1 to 6, with higher scores indicating a better policy and governance environment from the Bank’s perspective. Unfortunately the CPIA index is produced only for aid recipient countries, so we have nearly complete series for only 85 countries.

To my knowledge this paper is the first to exploit these data for an analysis of civil war onset. An earlier version for the ICRG data, for 1982 only, has been used as a measure of “governance” or “good institutions” in a number of studies of the determinants of economic growth, including influential papers by Acemoglu, Johnson and Robinson (2001), Knack and Keefer (1995), and

³⁸Kaufmann, Kraay, and Mastruzzi (p. 6) describe these six areas as follows: “(1) Voice and Accountability (VA) capturing perceptions of the extent to which a country’s citizens are able to participate in selecting their government, as well as freedom of expression, freedom of association, and a free media. (2) Political Stability and Absence of Violence (PV) capturing perceptions of the likelihood that the government will be destabilized or overthrown by unconstitutional or violent means, including politically-motivated violence and terrorism. (3) Government Effectiveness (GE) capturing perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government’s commitment to such policies. (4) Regulatory Quality (RQ) capturing perceptions of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development. (5) Rule of Law (RL) capturing perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence. (6) Control of Corruption (CC) capturing perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as “capture” of the state by elites and private interests.”

Mauro (1995). But the longer time series employed here appears not to have been used even in that much larger literature on growth.

Whether the thing to be explained is growth or civil war onset, expert-survey based measures of “good institutions” or “good governance” face a number of problems. In the first place, it is not completely clear what the expert ratings are measuring. This is partly due to lack of clarity about what we are trying to measure. Just what are “good governance” and “good institutions”? Many people have strong intuitions here, based on experiencing the relative efficiency, competence, and corruption of public services and officials in different countries. In more theoretical terms, the tradition associated with North and Thomas (1973) identifies “good institutions” with formal and informal political institutions that render unlikely the expropriation of private wealth and investments by political elites. In work on state capabilities and civil war, the focus tends to be on the efficiency and competence of the police, armed forces, and judiciary (“rule of law,” in part).

But neither the competence of public management, expropriation risk and contract enforcement, nor “rule of law” is easily observed and measured. Ideally, we would like to have objective indicators for these constructs, but even if we did, concepts like “efficiency,” “competence,” and “expropriation risk” seem inherently to be latent variables that would have to be inferred from diverse observations of different things.

This fact makes expert surveys a natural approach for measuring the quality of “governance” and “good institutions,” but it is also makes it hard to know exactly what the experts are doing. For example, are they really making judgements about the quality of governance and particular institutions, or implicitly are they answering the general question “How do you think things are going these days in country X (perhaps implicitly as compared to other countries in the region)?” Answers to the latter might partly measure quality of governance or institutions, but could also include many considerations that we would not associate conceptually with governance and institutions. In sum, there are reasons to be concerned about both the validity and reliability of the expert-survey based measures of governance, but it is also not obvious what a better approach would be.³⁹

The second major problem one faces when trying to use governance indicators to assess the causes of economic growth or civil war onset is endogeneity. If an indicator is well correlated with contemporaneous growth or civil war onset, we cannot infer causality, because it could be that the observation of growth is leading the experts to think that governance is good, or that the observation of civil war leads them to infer that governance or institutions are bad.

When one has only a single observation of governance quality for a set of countries, and a single observation of level or growth of income, or conflict performance, the only feasible solution is to find an instrument for governance – an exogenous variable that affects growth or conflict *only*

³⁹A signal advantage of Kaufmann and Kraay’s approach is that by drawing on a large number of different expert-based measures, their measures may have greater reliability than any one source.

through its effect on governance. Such variables are very hard to find and the exclusion restriction is not testable.⁴⁰

With data from time t on governance and from time $t + 1$ on growth or conflict, we can ask whether the former predicts the latter, controlling for other possible determinants of growth or conflict. An important advantage of this design is that it cannot be that observation of the outcome (growth or conflict in time $t + 1$) caused the experts to code better governance or institutions in time t , because of course it had not happened yet when they made those judgments. So if we have enough years of data on governance and growth or conflict, then we can ask whether expert assessments of governance actually *predict* subsequent conflict or growth experience.

If the answer is yes, this still does not settle the question of causality, since it could be that omitted variables are causing both expert assessments of quality of governance at time t and conflict or growth performance subsequently. In particular, as we will see below, all of the WGI and ICRG indicators are *highly* correlated with per capita income. This is as it should be, if it is true that income is a proxy for state capabilities and that good governance and good institutions cause economic growth over the long term. But it raises the problem of how to separate out the causal impact of governance on conflict or growth, versus that of other determinants of high income.

The basic approach taken below will be simply to control for prior income levels, thus asking about the relationship between what we might call “surprisingly good governance” and civil war onset. A country has surprisingly good governance when experts gave it high ratings as compared to other countries *at the same level of per capita income*. The attempt to identify the causal impact of governance quality on conflict then comes from seeing whether surprisingly good or bad governance in one period of time predicts conflict onset subsequently. The strategy will be effective to the extent that whatever determines surprisingly good/bad governance in one time period influences subsequent conflict risk primarily through governance and institutions, rather than via some other path.

With the ICRG and CPIA indicators, we have long enough time series and enough variation over time within countries to go further. We can consider models with country fixed effects, so controlling for all manner of unobserved time-invariant country characteristics.

The core strategy here is potentially subject to the concerns noted above concerning “policy regressions,” where the researcher tries to infer something about the causal effect of a policy choice by

⁴⁰For studies of institutions and governance as causes of economic growth, Mauro (1995) used ethnic fractionalization as an instrument for corruption as measured by expert surveys; however, it is highly implausible that the only path through which ethnic fractionalization would be related to economic growth is corruption. Acemoglu, Johnson and Robinson (2001) famously used settler mortality in colonies hundreds of years ago as an instrument for 1983 expropriation risk (ICRG). Knack and Keefer (1995) did not really address the endogeneity issue.

measuring different policies across cases and putting them on the right-hand-side in a regression model. “Surprisingly good governance” (SSG) is of course at least partly a policy choice by a leadership or political regime. Then we will *underestimate* the positive impact of good governance if on average leaderships tend to choose better governance when, for other unmeasured reasons, they expect that the risk of civil conflict is high. In that case, SSG will be partly correlated with unobserved factors that favor conflict, so that our estimates of the pacifying effects of SSG will be biased downwards. But, on the other hand, what if leaderships are more able to implement SSG in country years when, for reasons completely unrelated to governance, conflict is unlikely? To this extent, we would tend to *overestimate* the causal impact of good governance on conflict risk. I find it difficult to think of plausible stories here – perhaps there are cultural trends that arise independent of governance but can enable better governance, and these directly determine civil war propensity? But in principle the risk is there.

The efficacy of the approach depends in the end on what explains variation in surprisingly good governance. If, or to the extent that, leaderships and state bureaucracies “get their acts together” for reasons that are largely independent of other, independent causes of civil strife, then the results below suggest that good governance and institutions are indeed important factors in reducing a country’s conflict risk. For instance, governance and institutions may improve when an old leader dies and the new one is just more capable, or is politically situated so that he or she can implement better policies and develop better institutions. Or governance may be fairly steady, but income varies due to international shocks and other vagaries of economic growth, in which case “surprisingly good governance” will appropriately estimate a causal effect. I find these possibilities more plausible than alternative arguments that would imply that this approach leads to overestimation of the “governance effect,” but clearly more work on the determinants of surprisingly good governance is needed.

7.1 The WGI and ICRG governance indicators

As noted, the WGI project produces indicators for six dimensions of “governance,” labeled government effectiveness, voice and accountability, political instability, rule of law, corruption, and regulatory quality. ICRG produces a large set of indicators, which have varied somewhat over the years. In this paper I consider four ICRG indicators that have the longest history and that correspond most closely to the WGI categories. These are called “investment profile,” “corruption,” “rule of law” (or “law and order”), and “bureaucratic quality.” The correspondence with the WGI indicators is clear except for investment profile. ICRG intends this measure as a general indicator of business climate and political risks to business in a country year. It is the successor to the “expropriation risk” and “observance of contracts” variables from the 1982 ICRG data used by a number of growth studies.

Because they are derived from a factor-analysis-like technique, the WGI indicators all have mean

zero and standard deviation of 1, with higher values indicating better quality governance on that dimension.⁴¹ For ICRG, corruption and rule of law are on 1-to-6 scales. Investment profile varies from 1 to 12, and bureaucratic quality from 1 to 4. Higher values are better.

The World Bank's CPIA indicator varies from 1 to 6, with higher values indicating better governance. The scale is an average of a large number of components, which since 1997 have been grouped into four equally weighted clusters, described as “economic management,” “structural policies,” “policies for social inclusion/equity,” and “public sector management and institutions.”

For our purposes, a major liability of the CPIA index is that it is only coded for countries that receive IDA loans, and that countries can “graduate” out of and enter into this category depending on economic and government performance. As a result, the CPIA sample is already truncated by having relatively poor countries, and, even worse, there is a built-in selection bias that will work against identifying the impact of governance on conflict (or growth) outcomes. Namely, countries that perform well are more likely to exit the CPIA sample, and countries that perform poorly may enter it.

Figures 6 and 7 show scatterplots that illustrate how closely related are the different ICRG and WGI governance indicators to each other and to (the log of) per capita income. The data are for 2005, but the picture of course looks very similar for other years. The correlations are given in Table 14, which also shows that associations among the WGI, ICRG, and CPIA indicators are strong as well.⁴² There is not much indication that correlations across the ICRG and WGI indicators are higher within what should be the same dimension – for example, “rule of law” in the two different data sets.

Table 15 shows the correlations between the residuals of the WGI, ICRG, and CPIA indicators after regressing each of them on log per capita income. They remain substantial, which is encouraging in that it suggests that raters' perceptions of quality of governance or institutions are not completely determined by level of economic development. Instead, there appears to be some level of agreement about surprisingly good or bad governance. However, there is not much indication that agreement is markedly higher within categories (e.g., corruption, rule of law, etc.) than across them. This suggests either that these various dimensions of governance quality tend in practice to go together very closely, or that the expert raters really have in mind some general notion of “country has its act together” rather than being able to separate out dimensions of performance clearly.

One other descriptive statistic about these indicators is worth presenting before moving to the

⁴¹One problem with this approach is that a country's rating may change from one year to the next not because anything changed in the country, but because other countries changed; these measures have more validity as a ranking within a given year than as a time series measure.

⁴²This is partly mechanical, since ICRG indicators are one of the many inputs into the WGI indicators.

Table 14: Income and governance indicator correlations

	income	ge	voice	pol. stab.	corr.	rol	reg. qual.	WGI	ICRG		
								ip	corr.	rol	bq
govt eff.	79										
voice	58	75									
pol. stab.	67	79	71								
corruption	74	94	72	77							
rule of law	77	95	79	83	94						
reg. qual.	75	94	79	75	88	91					
inv. prof.	72	82	73	73	79	83	88				
corruption	61	85	74	67	88	84	78	65			
rule of law	69	73	50	72	75	78	66	61	65		
bur. qual.	77	89	77	66	82	85	83	72	76	63	
cpi	51	78	58	47	62	67	82	73	51	39	62

Table 15: Correlation between WGI and ICRG, netting out income

ICRG	WGI						
	ge	voice	pol. stab.	rol	corruption	reg. qual.	
ip	38	36	33	40	35	47	
corruption	60	51	41	61	66	52	
rol	42	14	42	55	46	33	
bq	67	54	27	58	53	57	
cpi	61	41	24	51	47	64	

analysis. Table 16 shows the percentage of variation for each indicator that is due to variation across countries as opposed to over time within countries. Almost of all of the variation in the WGI indicators is across countries, which makes sense given that the time period is just over a decade and no one could think that “state capabilities” would change a great deal from year to year. There is much more within-country variation for the longer ICRG and CPIA series, and especially for the ICRG “investment profile” indicator. This will allow us to consider a fixed effects model with the ICRG and CPIA data.

7.2 ICRG governance measures and civil war onset

Table 17 reports our baseline model (Model 1 of Table 2), for major conflicts, adding each of the four ICRG governance indicators in turn. The indicators are lagged two years to try to lessen the risk that their values are being caused by knowledge of a civil conflict already in progress.

Table 16: Percentage within vs. between country variation in governance indicators

variable	between %	within %
log(income)	85	15
ACD war onset	6	94
WGI: 1996-2008		
ge	96	4
voice	97	3
pol. stab.	91	9
corruption	96	4
rol	96	4
reg. qual.	94	6
ICRG: 1984-2006		
ip	41	59
corruption	71	29
rol	72	28
bq	82	18
CPIA: 1977-2008		
cpia	58	42

We find that all of them have estimated coefficients that correspond to very large substantive effects, with all but “bureaucratic quality” statistically significant (investment profile and rule of law strongly so). For investment profile, moving from the 75th to the 25th percentile (8.5 to 5) is estimated to associate with an increase in annual civil war odds of a factor of 5.9. Going from 12 to 1, the full range of the scale, increases the estimated odds by a factor of about 400. Moving from the 75th to the 25th percentile on rule of law and corruption associates increasing the annual onset odds of 4.6 and 1.9, respectively. Note that the estimates for investment profile and rule of law are remarkably large, in substantive terms.

Model 1 of Table 17 is Model 1 of Table 2 but restricted to the sample used when we add ICRG indicators, which is limited to 1984-2006 and countries with ICRG data. It shows that in this subsample the standard errors for several of the other covariates increase and some of the estimated coefficients diminish. One reason is that the ICRG measures are systematically more missing for low income and high conflict countries – presumably investors don’t need to be told that Afghanistan has a poor investment profile. It is plausible that the results would be generally stronger if we had more complete data for the lowest income countries.

Even so, given the data we have we find that the estimated coefficients for per capita income actually turn positive (though not significantly different from zero) when we add the governance indicators in Models 2-4. This is consistent with the hypothesis that income “matters” because it

proxies for state capabilities, or governance.⁴³

Table 17: ICRG governance and civil war onset (major conflicts)

	Model 1	Model 2	Model 3	Model 4	Model 5
ip _{t-2}		-0.50*** (0.12)			
corrupt _{t-2}			-0.32 [†] (0.19)		
rol _{t-2}				-0.59*** (0.17)	
bq _{t-2}					-0.23 (0.31)
log(gdp _{t-1})	-0.13 (0.23)	0.33 (0.24)	0.04 (0.25)	0.23 (0.24)	0.04 (0.34)
log(pop _{t-1})	0.23 (0.20)	0.30 (0.19)	0.21 (0.20)	0.27 (0.21)	0.25 (0.20)
log(% mountains)	0.11 (0.17)	0.04 (0.17)	0.14 (0.18)	0.13 (0.18)	0.10 (0.17)
oil producer	1.34** (0.46)	1.09* (0.46)	1.18* (0.51)	1.14* (0.47)	1.23* (0.49)
pol instability _{t-1}	-0.02 (0.58)	-0.06 (0.54)	0.02 (0.58)	-0.11 (0.59)	-0.03 (0.59)
anocracy _{t-1}	1.08* (0.50)	1.05* (0.45)	1.06* (0.49)	1.08* (0.47)	1.08* (0.49)
ELF	0.92 (0.87)	1.55 [†] (0.88)	0.99 (0.87)	0.93 (0.85)	1.07 (0.88)
prior war	-0.06 (0.70)	-0.52 (0.76)	-0.13 (0.69)	-0.45 (0.69)	-0.13 (0.68)
constant	-7.55** (2.83)	-9.25*** (2.62)	-7.92** (2.86)	-9.10** (3.07)	-8.73** (3.12)
N	2777	2777	2776	2776	2776

SE's clustered by country.

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

Table 18 is the same as Table 17, except for all ACD conflicts (including minor, medium and small ones). Here all four ICRG governance indicators are significantly negatively related to conflict onset, with the strongest effects again being for investment profile and rule of law. Low income is significantly negatively related to conflict onset in this subsample (Model 1), and while the

⁴³This effect is stronger if we consider models that drop other covariates, for example leaving just prior war, income, population, and ethnic fractionalization.

signs remain negative when governance indicators are added, the estimated coefficients for income shrink towards zero and in several cases are no longer statistically significant. This again favors the interpretation that income is proxying in a large part for state capabilities and administrative or policy competence.

In principle, it could happen that when a country gets better ICRG (or WGI) ratings, foreign investment increases as a result, which then increases subsequent growth, which then has a direct economic effect on the odds of civil conflict. Governance or institutions are still causally efficacious for lower conflict risk in this argument, but the pathway is through the economy. A first-order check of the hypothesis can be done by adding the current economic growth rate to the models in Tables 17 and 18; a better approach would be to check whether improvements in ICRG ratings are strongly related to subsequent FDI. When we do this, the estimated coefficients on the governance variables change very little, although in some cases significance drops slightly. This again argues against the main effect of better governance indicators on conflict risk working primarily through the economy.

Table 19 repeats the exercise for all conflicts, but using conditional fixed effects logit and thus controlling for all unmeasured (but temporally stable) country characteristics. Remarkably, given the small number of countries in the sample now and the relative lack of temporal variation in governance quality, all four of the ICRG indicators get negative coefficients (though corruption is essentially zero), and the estimates for investment profile and rule of law are statistically significant ($p = .002$ and $.091$ respectively). As in Table 2, Model 5 (fixed effects on the larger sample), per capita income takes “the wrong sign” and is never close to significant.

Thus, within countries over time, civil war onset has been somewhat more likely when investment profile, corruption, and rule of law were judged worse in recently preceding years. This result supports a causal interpretation of the relationship between governance quality and conflict onset more than the previous models, because here identification is based on within-country comparisons (and because it is somewhat remarkable to find anything given the lack of within-country variation in governance indicators).⁴⁴

One might still worry that a two-year lag is not enough to rule out the possibility that expert raters are coding based on indications of incipient civil war, more than on the quality of governance or institutions. I have constructed a panel with three waves, for the 80s, 90s, and 00s, asking if average ICRG ratings in one decade forecast conflict in next decade, controlling for prior conflict experience and lagged income levels. I find that the results are quite similar: ICRG indicators forecast conflict outbreak even in next decade.

⁴⁴Looking only at major conflicts, there are too few countries with a major onset in the ICRG subsample (only 17) to get anything reliable out of fixed effects. However, the coefficient for investment profile is negative and very close to “significant” at .10.

Table 18: ICRG governance and conflict onset (all ACD conflicts)

	Model 1	Model 2	Model 3	Model 4	Model 5
ip _{t-2}		-0.27*** (0.07)			
corrupt _{t-2}			-0.18 [†] (0.10)		
rol _{t-2}				-0.20* (0.09)	
bq _{t-2}					-0.29 [†] (0.16)
log(gdp _{t-1})	-0.45** (0.17)	-0.17 (0.15)	-0.37* (0.18)	-0.33 [†] (0.20)	-0.23 (0.19)
log(pop _{t-1})	0.30** (0.10)	0.35*** (0.10)	0.28** (0.10)	0.32** (0.11)	0.34** (0.11)
log(% mountains)	0.12 (0.12)	0.09 (0.12)	0.13 (0.13)	0.13 (0.13)	0.10 (0.12)
oil producer	0.91** (0.33)	0.72* (0.32)	0.83* (0.35)	0.81* (0.34)	0.75* (0.34)
pol instability _{t-1}	0.12 (0.25)	0.10 (0.26)	0.12 (0.26)	0.07 (0.26)	0.11 (0.26)
anocracy _{t-1}	-0.04 (0.31)	0.03 (0.30)	-0.06 (0.31)	-0.01 (0.30)	-0.03 (0.31)
ELF	1.23* (0.52)	1.58*** (0.47)	1.24* (0.52)	1.24* (0.52)	1.40** (0.51)
prior war	0.53 (0.34)	0.27 (0.34)	0.49 (0.33)	0.40 (0.34)	0.44 (0.36)
constant	-3.71* (1.75)	-5.02** (1.59)	-3.77* (1.81)	-4.19* (1.91)	-5.45** (1.84)
N	2777	2777	2776	2776	2776

SE's clustered by country.

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

7.3 GI indicators and civil war onset

The WGI series is only for 1996 to 2008, with some of the early years missing. It also has very little over time variation within countries, and the method of its construction raises some questions about whether and how best to treat it as panel data in any event.

However, because 14 years have passed since the first set of WGI indicators were constructed, we can ask whether expert-based assessments of different dimensions of governance quality in

Table 19: ICRG governance and all conflict onsets, country fixed effects

	Model 1	Model 2	Model 3	Model 4
ip _{t-2}	−0.228** (0.075)			
corruption _{t-2}		−0.038 (0.153)		
rule of law _{t-2}			−0.226 [†] (0.134)	
bq _{t-2}				−0.236 (0.215)
log(gdp _{t-1})	0.287 (0.541)	−0.166 (0.505)	0.015 (0.512)	−0.024 (0.517)
log(pop _{t-1})	0.517 (0.948)	−0.352 (0.901)	0.104 (0.923)	−0.429 (0.894)
oil	0.528 (0.964)	0.441 (0.976)	0.383 (0.965)	0.239 (0.982)
pol. instab.	0.184 (0.324)	0.163 (0.325)	0.155 (0.326)	0.161 (0.325)
anocracy _{t-1}	−0.088 (0.398)	−0.131 (0.396)	−0.169 (0.399)	−0.126 (0.398)
democracy _{t-1}	−0.587 (0.478)	−0.721 (0.488)	−0.746 (0.487)	−0.687 (0.487)
prior war	−1.414** (0.359)	−1.271** (0.355)	−1.342** (0.362)	−1.295** (0.357)
N (N countries)	956(44)	955(44)	955(44)	955(44)
Country fixed effects	yes	yes	yes	yes

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$

1996 or 1998 actually forecast conflict experience in the next decade, controlling for initial level of income and prior conflict experience. Using the ACD civil war variable, only 10 countries had onsets between 1997 and 2009. Using all ACD conflicts, 37 countries had a total of 63 onsets. We control for income level in 1996, along with prior conflict experience and ethnic fractionalization. Thus, we are asking if perceptions of “surprisingly good governance” relative to income level and conflict history can still forecast civil peace.

Table 20 shows that the perceptions of “government effectiveness,” “political stability,” and “rule of law” in 1996 are indeed significantly related to major conflict outbreak over the next 13 years. This might not be too surprising for “political stability,” which is based on expert surveys intended to capture “perceptions of the likelihood that the government will be destabilized or overthrown by unconstitutional or violent means” (Kaufmann, Kraay and Mastruzzi 2009). But the results are

also present for “government effectiveness” and “rule of law,” and the estimated coefficients are negative and substantively large for corruption and regulatory quality as well. It is also evident that adding governance measures tends to turn the sign on the estimate for income slightly positive or close to zero, again consistent with the hypothesis that income normally “stands in for” governance or state capabilities. The pattern continues to hold if we add region fixed effects (although the significance of government effectiveness weakens a bit), or measures of oil production or population in 1996.

Table 21 repeats the exercise with the dependent variable of all ACD conflicts.⁴⁵

I have run the same models using the WGI indicators from 1998, and the dependent variable as onsets after 1998. The results are quite similar, though marginally weaker. This lowers the likelihood that the results are a fluke from one year of WGI data (which as we have seen are highly stable over time anyway). I have also added average growth rate of GDP per capita after 1996, again finding little change for the governance indicators.

There is not much evidence that different dimensions of governance as measured by WGI show notably stronger or weaker relationships to subsequent conflict risk. “Political stability,” which is supposed to be an expert appraisal of conflict risk, is indeed the most strongly related, while “voice and accountability,” which is based on assessments of democracy, is the weakest. (This is consistent with our earlier findings of little link between democracy and conflict risk in poor countries.) Rule of law and “government effectiveness” – which KKZ describe as “capturing perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government’s commitment to such policies” – appear to be most predictive after political stability. But overall, just as we saw that there are quite high correlations among these different dimensions, no strong conclusions can be drawn about what dimension of governance is most important for increasing the odds of civil peace.

To recall, the identification strategy here is plausible in so far as the following argument is plausible: once we control for 1996 income, prior conflict experience, and other factors, variation in countries’ quality of governance as measured by expert ratings in 1996 is essentially random with respect to unmeasured other determinants of subsequent civil war risk. I find it difficult to think of omitted variables distinct from “governance” or “institutions” that would plausible cause both rater perceptions of governance quality and conflict performance over the subsequent ten years. But the possibility still exists.

⁴⁵I log the number of prior onsets (plus one) because some countries have many prior low-level onsets and the distribution is quite skewed. Also, note that because some countries have multiple onsets after 1996, a negative binomial model could be used instead of logit with whether a country had at least one onset as the dependent variable; results are quite similar.

The identification strategy here will not do a good job of estimating the long run impact of good governance or state capabilities, since some part of these may be incorporating into per capita income in 1996. Depending on how we think about what causal effect we are trying to estimate – relatively short run or long run impact of governance on civil war odds – this would tend to make for underestimates rather than overestimates of the importance of governance and state capabilities.

The next step in this part of the investigation should be to examine the determinants of surprisingly good/bad governance – what explains variation in expert perceptions once we have netted out income level? In preliminary work, I find that, interestingly, ethnic fractionalization is almost completely unrelated to surprisingly good governance. This is surprising in light of the literature in economics finding and arguing that ethnic diversity directly causes low public good provision and corruption. Second, I find that oil producers and autocracies are consistently judged to have worse governance, even controlling for income.⁴⁶

7.4 CPI indicators and civil war onset

Table 22 is essentially our basic model for all ACD conflicts using the set of countries and years with CPIA estimates (Model 1), adding one- and two-year lags of the CPIA index (Models 2 and 3), and then Model 2 with country fixed effects. The estimated coefficients for the lagged CPIA index are similar in magnitude to those for the other governance indicators, despite the sample truncated sample and the selection bias issues. However, they are barely statistically significant for all conflicts, not at all with fixed effects, and not at all if we look only at major conflicts. The estimate hardly changes when we use the two- instead of the one-year lag, which suggests that there probably isn't very much “coding of CPIA on civil war in progress” going on. There is less indication here that the CPIA measure successfully competes with income.

⁴⁶As noted, the results in Tables 17 and 18 are not changed if we control also for oil or democracy.

Table 20: WGI governance in 1996 and ACD civil war onset, 1997-2008

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
gov't effec. 1996	-0.97 [†] (0.58)						
voice 1996		-0.52 (0.44)					
pol. stab. 1996			-0.97* (0.44)				
rule of law 1996				-1.40* (0.60)			
corruption 1996					-0.96 (0.66)		
reg. qual. 1996						-0.73 (0.45)	
onsets pre 1997	0.50* (0.22)	0.46* (0.23)	0.43 [†] (0.23)	0.24 (0.27)	0.38 (0.23)	0.40 [†] (0.23)	0.45* (0.23)
log(income) 1996	-0.42 (0.34)	0.11 (0.47)	-0.20 (0.38)	0.08 (0.42)	0.24 (0.45)	-0.02 (0.45)	-0.02 (0.41)
ELF	-0.71 (1.29)	-0.63 (1.28)	-0.46 (1.32)	-0.59 (1.30)	-0.60 (1.32)	-0.62 (1.28)	-0.60 (1.30)
constant	0.60 (2.98)	-4.16 (4.17)	-1.51 (3.48)	-3.95 (3.81)	-5.57 (4.09)	-3.11 (4.02)	-2.95 (3.69)
<i>N</i>	156	156	156	156	156	156	156

DV = ACD civil war onset after 1996. Logit, with standard errors in parentheses

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

Table 21: WGI governance in 1996 and all ACD conflict onset, 1997-2008

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
govt effect. 1996		-0.62 (0.41)					
voice 1996			-0.47 (0.30)				
pol. stab. 1996				-0.93** (0.32)			
rule of law 1996					-0.67† (0.38)		
corruption 1996						-0.12 (0.41)	
reg. qual. 1996							-0.45 (0.33)
log(# onsets pre 1997 + 1)	1.26*** (0.35)	1.19*** (0.35)	1.15*** (0.35)	0.72† (0.39)	1.12** (0.35)	1.22*** (0.35)	1.17*** (0.35)
log(income) 1996	-0.55* (0.23)	-0.24 (0.31)	-0.40 (0.25)	-0.23 (0.27)	-0.27 (0.28)	-0.51† (0.29)	-0.34 (0.28)
ELF	1.30 (0.87)	1.39 (0.88)	1.53† (0.89)	1.58† (0.91)	1.41 (0.88)	1.30 (0.87)	1.43 (0.88)
constant	1.72 (2.07)	-1.13 (2.76)	0.18 (2.29)	-1.18 (2.37)	-0.89 (2.56)	1.29 (2.54)	-0.22 (2.54)
<i>N</i>	156	156	156	156	156	156	156

DV = at least one ACD conflict onsets after 1996. Logit, with standard errors in parentheses
 * significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

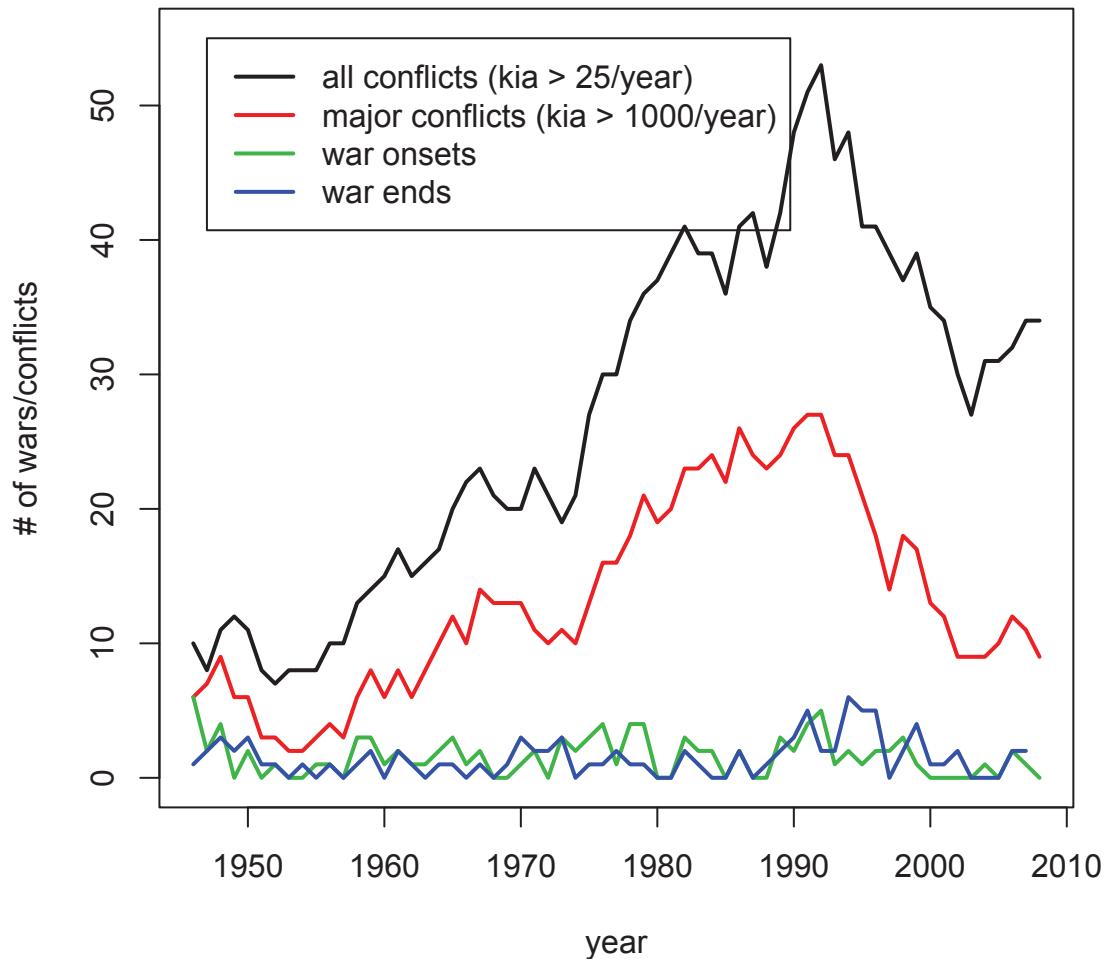
Table 22: CPIA governance and all ACD conflict onset

	Model 1	Model 2	Model 3	Model 4
prior war	-0.16 (0.29)	-0.26 (0.27)	-0.14 (0.29)	-1.89*** (0.35)
log(gdp _{t-1})	-0.48** (0.16)	-0.37* (0.16)	-0.33* (0.16)	0.01 (0.42)
log(pop _{t-1})	0.27* (0.11)	0.31** (0.11)	0.31** (0.10)	0.27 (0.65)
log(% mountains)	0.17* (0.08)	0.18* (0.08)	0.18* (0.09)	
oil producer	0.69† (0.37)	0.58 (0.37)	0.65† (0.37)	0.91 (0.77)
pol instability _{t-1}	0.03 (0.25)	-0.01 (0.25)	-0.00 (0.27)	0.11 (0.29)
anocracy _{t-1}	-0.04 (0.27)	-0.06 (0.27)	-0.08 (0.28)	0.11 (0.37)
democracy _{t-1}	-0.01 (0.34)	0.04 (0.35)	-0.04 (0.35)	0.09 (0.43)
ELF	1.22* (0.49)	1.23** (0.47)	1.32** (0.45)	
cpiat ₋₁		-0.31† (0.16)		-0.08 (0.17)
cpiat ₋₂			-0.31† (0.18)	
constant	-3.09* (1.55)	-3.26* (1.54)	-3.62* (1.45)	
<i>N</i>	3120	3120	3063	1652(61)
Country fixed effects?	No	No	No	Yes

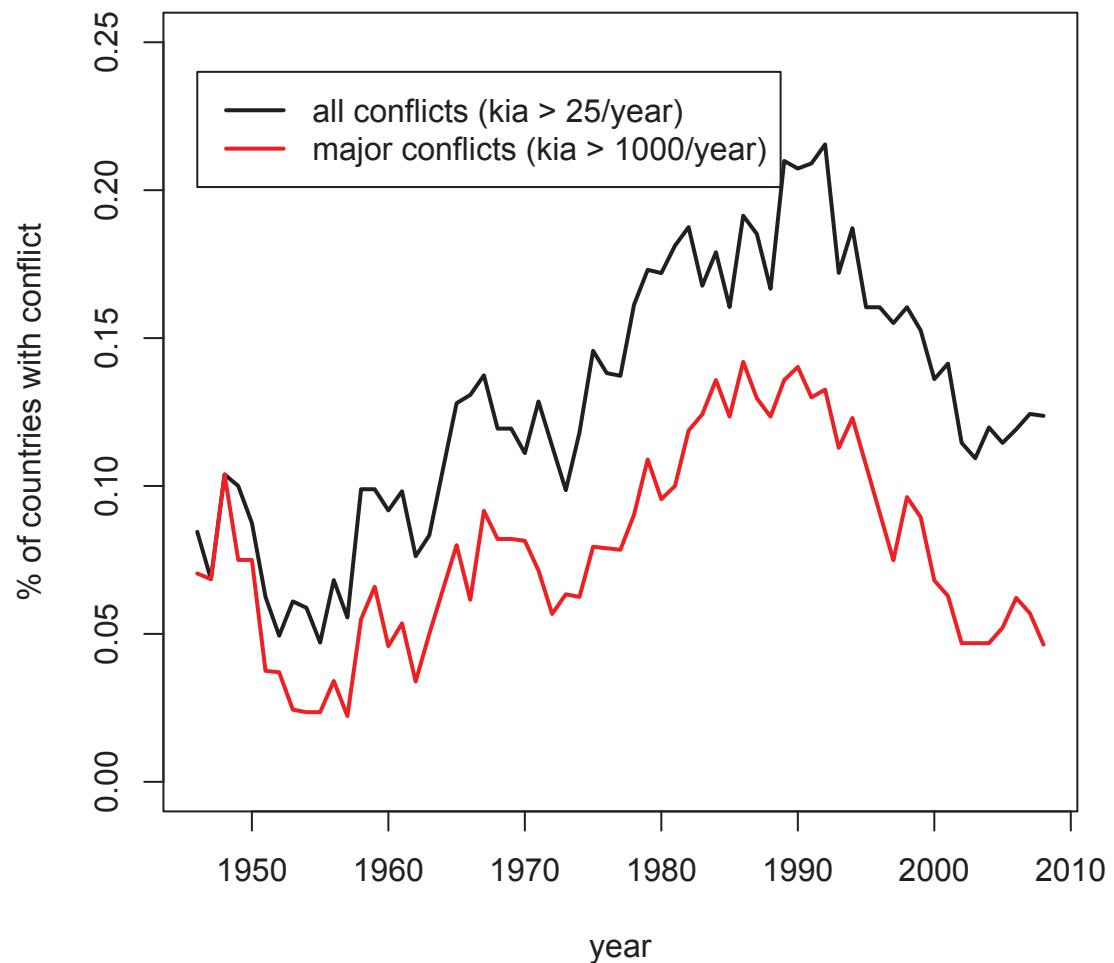
Standard errors in parentheses

† significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

Figure 1. # of civil conflicts and wars, 1946–2008



% countries with conflict or war, 1946–2008



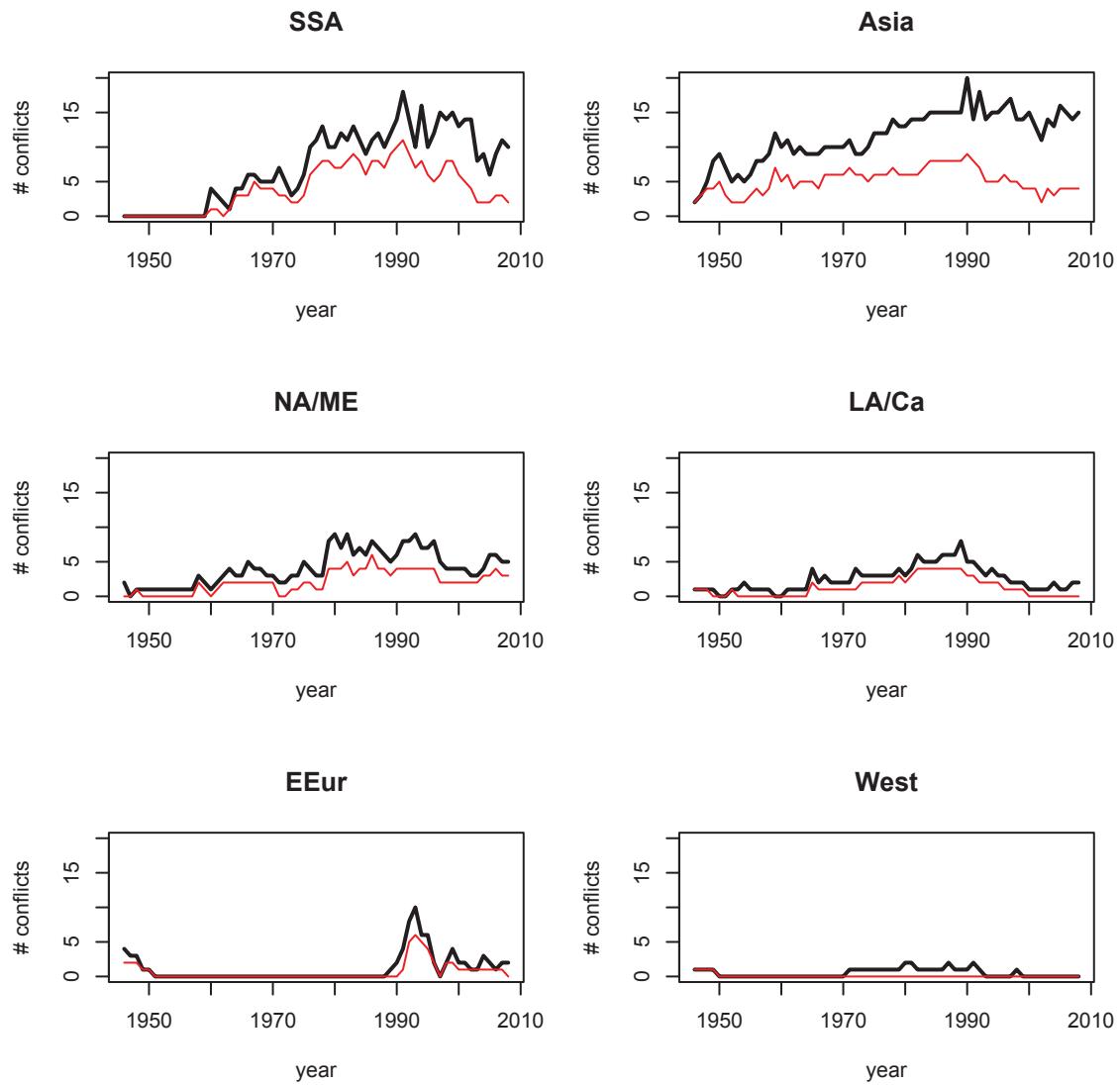


Figure 4. % of world population in conflict countries

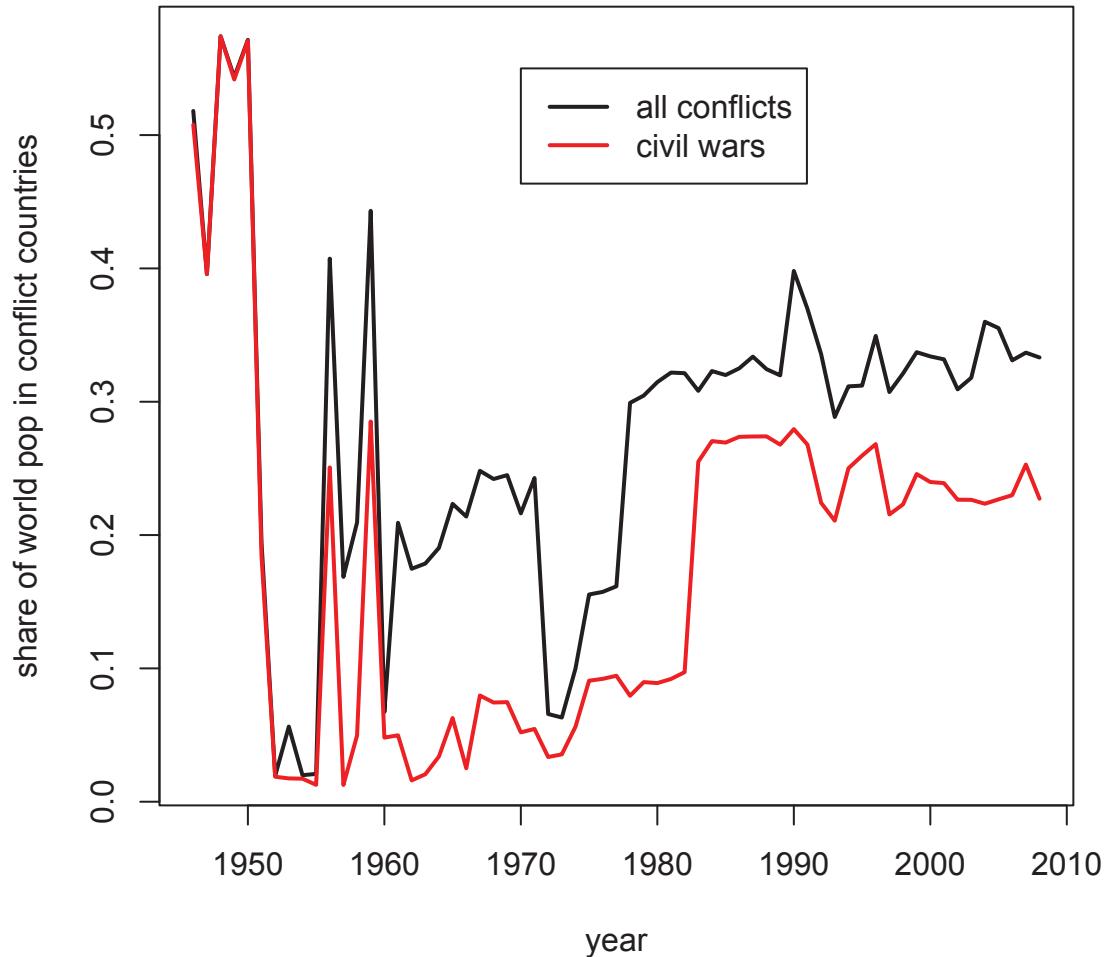
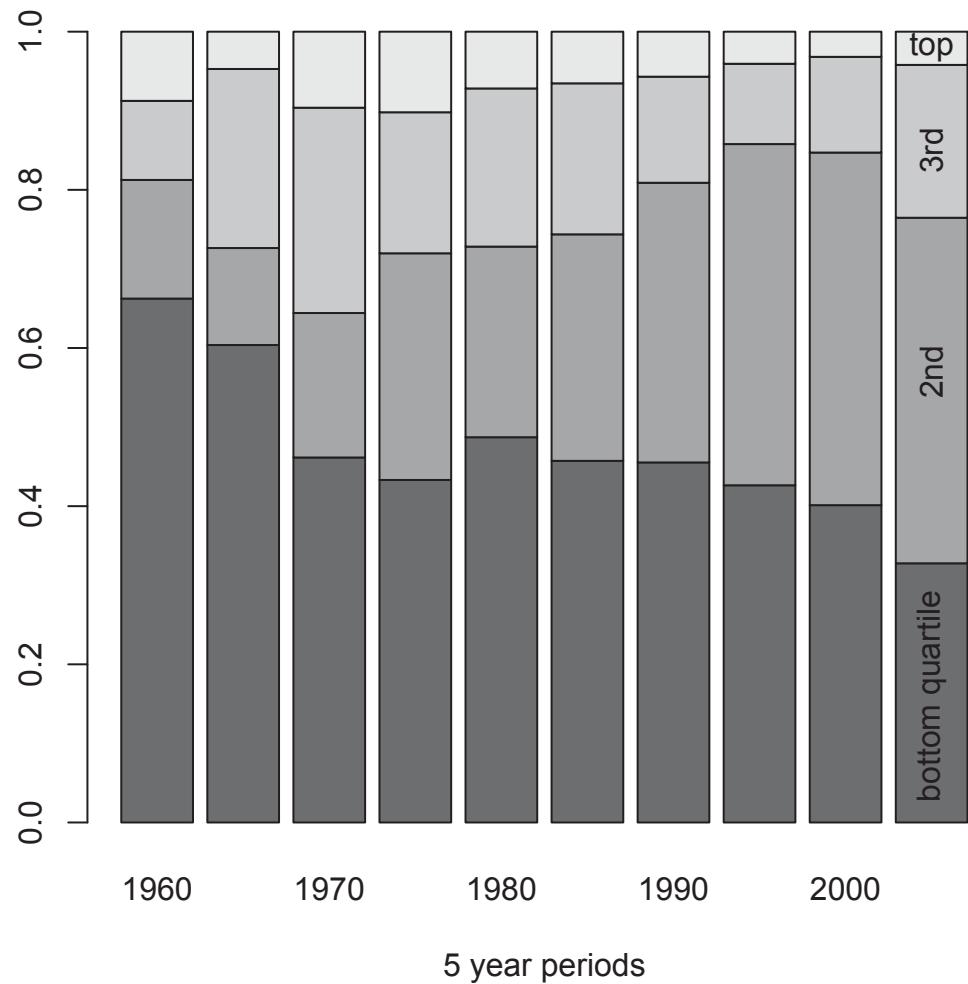
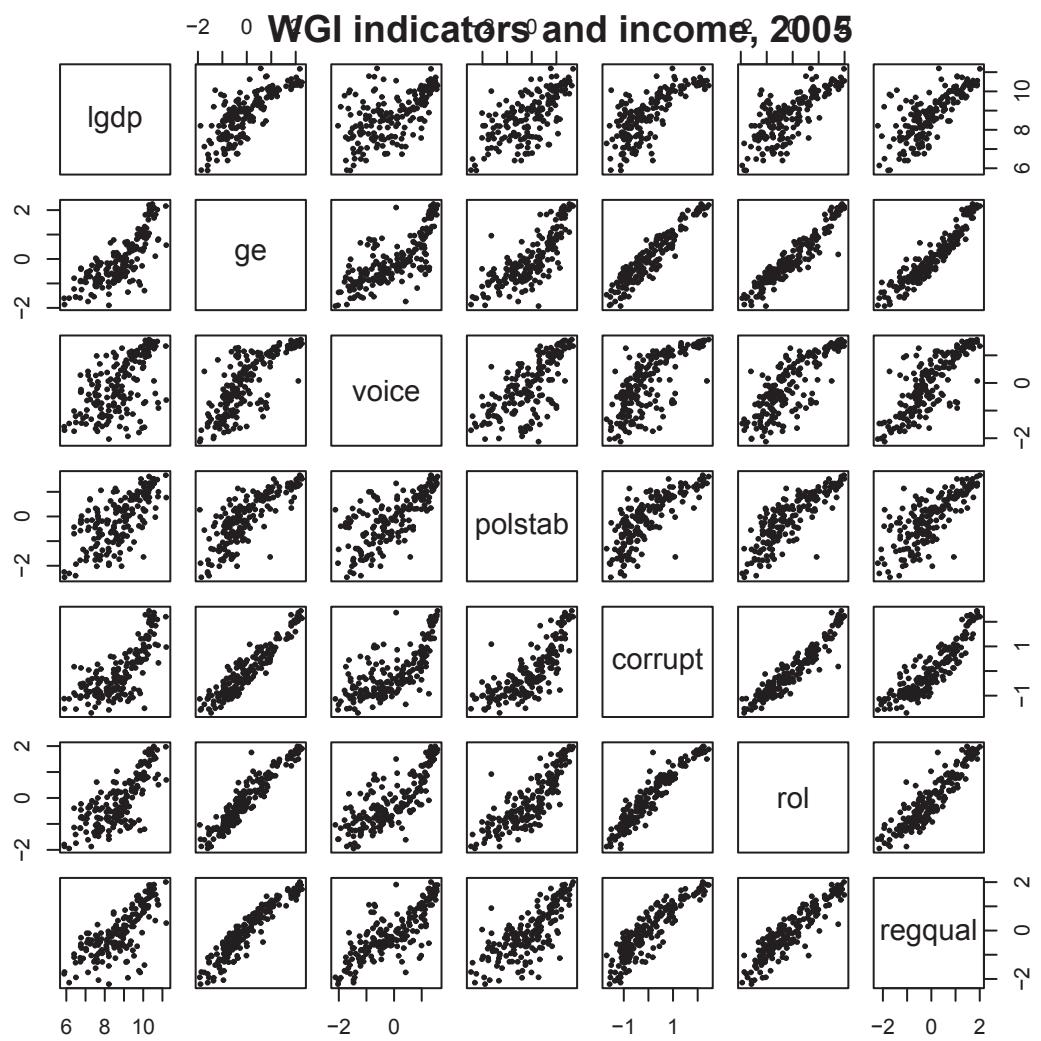
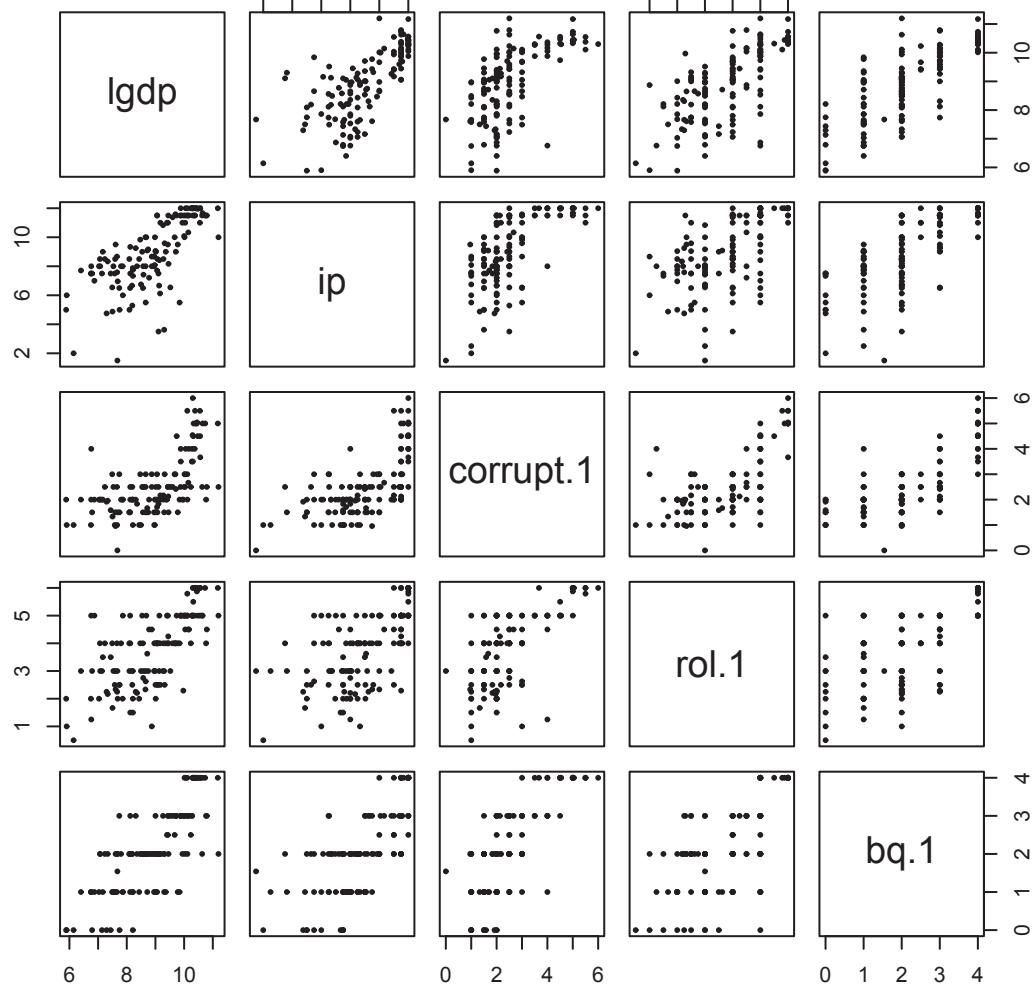


Figure 5. Distribution of conflicts by income quartiles and years





ICRG indicators and income⁵, 2005



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