

Memo on Security, Governance, and Development

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Civil conflict and economic growth, at present

1. Media and development sector conventional wisdom significantly overstates the present-day importance of violent conflict as a cause of bad economic performance in developing countries.
2. It is of course true that, in a few countries, major violent conflicts have had and are having devastatingly bad economic consequences. Currently, major violent conflicts and/or attendant state collapse cripple development prospects in three cases – Afghanistan, Somalia, and the DRC. And ongoing conflict poses major economic problems in a few others, such as Iraq and Pakistan.
3. It is also true that when we look across many country years, we find that on average countries grow more slowly in conflict years, and much more slowly for big conflicts. Table 1 shows estimates of the average effect on annual growth rate of three different levels of conflict, for country-year data for 1945 to 2009.¹ These come from regression models where the dependent variable is annual growth rate, and the independent variables are dummies for the levels of conflict. Country and decade fixed effects are included, so these are average differences in growth rates for war years versus peace years within countries. The three conflict levels are based on estimates of the total killed in all conflicts in the country year in question: “minor” if between 25 and 1000; “medium” if between 1000 and 10,000; and “major” if more than 10,000.

For all countries, and even within each region except Latin America, country years with major civil wars see 3.3 to five percent lower annual growth rates on average compared to peace years within the same country. Medium sized conflicts associate with 1.67% lower growth rates on average for the whole sample, and show more variability within regions (but still large differences for subSaharan Africa and Latin America). “Minor” conflicts, however – those estimated to have

¹The income data is primarily from Penn World Tables 6.3, extended where necessary by World Bank and Maddison growth rates.

Table 1: Average within country annual effect on growth rate for 3 levels of civil war

conflict level	all	SSA	Asia	NA/ME	LA/Ca
minor	-0.09	-1.18	0.21	-2.3	-0.01
medium	-1.67**	-2.38*	0.07	-0.07	-3.08**
major	-3.28**	-4.78**	-4.12*	-5.03***	-0.49

Coef's from regression of annual growth on war levels and country and decade dummies, 1945-2008.

** signif at .01, * at .05, with country country clustered se's.

no more than 1000 dead per year on average, which is actually a fairly high threshold – associate with a very small and statistically insignificant average reduction in growth rate.

4. *However*, the number of civil wars declined quite a bit since the high point in the mid-1990s, and conflicts in the last 10 years have tended to be much smaller scale as well.

Figure 1 shows the share of all independent countries with a civil war by year from 1945 through 2009, using an updated version of the civil war list of Fearon and Laitin (2003).² The three conflict magnitudes are broken out, with minor conflicts below the solid line, minor and medium below the dashed line, and all (minor, medium, major) below the dotted line. Similar to other civil war lists, the Figure shows a dramatic decline in civil war prevalence since the high point of the mid 1990s, so that at present about 12% of independent states have one ongoing.³ Note also that the severity of typical conflicts has changed. Minor conflicts are about as common as in the 1990s, but medium and major conflicts have fallen quite a bit; thus the conflicts of the 2000s have increasingly been small-scale.

Figure 2 is the same, but now limited to the poorest one-third of all countries in each year. Here we see an even more dramatic decline in the prevalence of violent conflict over the last 15 years, from a high point of 40% of the bottom third in 1994, to only about 14% in 2008-09! Further, almost all of the decline has been from the near disappearance of medium- and major-scale conflicts (in terms of intensity of killing). So now only about one in seven of the poorest countries have a significant violent armed conflict ongoing, and almost all of these are conflicts that are killing in the dozens or hundreds each year at this point.

Of course even this amount of violent conflict is not good. But it makes it hard to argue that actual,

²This counts as a civil war conflicts between armed groups vying for control of a central government or in a region, that killed at 1000 total, with an average of at least 100 per year, and with at least 100 on both (or all sides). More definitional details are given in Fearon and Laitin (2003).

³About 15% of non-micro-states, which are largely immune (despite being terribly governed in many cases, which is interesting).

large-scale violent conflict is currently a major barrier to economic development for the developing world. Among the poorest countries, only a handful are afflicted by major internal wars, and only a small number by low level armed conflict, which as we have seen does not appear to be strongly associated with much more negative growth effects in any event.

5. In addition, even in the 1990s and before, the amount of variation in growth rates “explained by” civil conflict was quite small. The increase in R^2 from adding the three conflict dummies in the above model (over and above income, and the country and decade dummies) is minuscule, less than 1% (.109 to .118).⁴ The numbers are barely higher if we restrict attention to poor countries.

Part of the reason is that while conflict was quite common among the poorest countries in the 80s and 90s, a lot of it was fairly low level, often involving peripheral insurgencies fighting for greater autonomy for a small ethnic group. While these conflicts can be very bad for a region, they usually seem to have relatively little impact on the economy of the country as a whole.

But probably much more important is that measured growth rates are *highly* variable within countries, and at least given the present state of our knowledge, most of the variation looks like unpredictable noise. In other words, the unmeasured “other causes” of growth rates are very big, even relative to the impact of typical violent conflicts.

Using annual data – which is not recommended because it is thought that so much of the annual fluctuations are noise due to measurement error (Johnson, Larson, Papageorgiou and Subramanian 2009) – no more than 10% of the variation is accounted for by country and year dummies. If we average annual growth rates by decade, hoping to get a more meaningful indicator of performance, we still find that only about 32% of variation is accounted for by country and decade dummies. Put differently, while the growth rate in the previous decade is significantly related to the current growth rate, the amount of variation explained by lagged growth is very small, at less than 2%! In short, country growth rates bounce around a great deal from decade to decade, inexplicably,⁵ and certainly not mainly because of realized violent conflicts, even in the poorest countries.

6. As an aside, what explains the major decline in armed conflict since the mid 1990s? Of course, that high point was in part the result of a spike in conflicts related to the break up of the Soviet Union, many of which were quickly settled or became “frozen” stalemates. But even so, civil wars were quite common in the 1980s, and we have seen a substantial decline from those levels as well.

The rate at which new conflicts have broken out has declined a little bit in the last 15 years, but

⁴This doesn't change if we format the data by decades, using average growth for the whole decade as the dependent variable, which cuts the noise in annual growth rates quite a bit.

⁵So far as I know; I'm not a growth economist by a long shot. Still, I can't imagine that human capital (as measured by, say, education stocks) bounces around this much from decade to decade. Investment might, but if so then I suppose that's what we would like to explain.

not so much. The much larger change has been in the rate at which existing conflicts have ended, which has risen a great deal since the mid 1990s. Table 2 lists conflicts in progress in 1995 and gives an indication of what happened to them.

The conflicts from 1995 that are still ongoing are almost all quite small scale, and in many cases fit the pattern of long-running, low-level “sons of the soil” conflicts between peripheral ethnic minorities and a central state associated closely with a majority or plurality ethnic group (Fearon and Laitin 2010). A number of this type of conflict also appears in the set of “government victories” – Sri Lanka/LTTE, China/Xinjiang, Angola/Cabinda, Chad/FARF, Bangladesh/Chittagong. “Sons of the soil” conflicts are remarkably persistent and any of these may well return to violence.

One common argument for the decline in armed conflict is that the international community has gotten much more involved in mediation and the construction and implementation of peace settlements since the end of the Cold War (see the Human Security Report 2005 in particular). There is support for this view in Table 2, which shows ten of the 21 ended conflicts (as of 2009) involving a peace agreement, and almost all of these accompanied by an international peace-keeping operation (usually a UN operation). PKOs were rare before the 1990s, and usually deployed for interstate conflicts.⁶ We now have a fairly large literature investigating the question of whether PKO “treatment” causes longer peace duration, which pretty uniformly finds that it does. The treatment is not randomly assigned, but Fortna (2008) and Gilligan and Sergenti (2008) make compelling empirical cases that if anything this works against the hypothesis that PKOs help, rather than for it. So the evidence seems fairly good that peace-keeping at a minimum buys a post-conflict state valuable time to get “state building” off the ground.

There have also been quite a few government military victories – 12 of the 21 by my coding, four of which also involved a peace deal with some minor concessions to the losers. Government victories were the modal form of civil war outcome even before the 1990s, but the rate at which they have occurred in the last 15 years has been higher than in the period 1945-1994.

Kalyvas, Balcells and Rohner (2008) observe that Marxist insurgencies of the classical sort have almost disappeared since the end of the Cold War (Nepal and the Naxalites in India being notable exceptions). They suggest this as a reason for the general decline in civil wars. The drop in magnitude of contemporary civil conflicts is probably closely related, since Marxist-vs-capitalist conflicts during the Cold War often had major financial or military intervention by the superpowers, and third party intervention is robustly associated with large-scale civil conflicts. As Kalyvas et al note, today we see a lot more militia-based conflicts (what they call “symmetric nonconventional” wars), which tend to be fairly low-level and disorganized.

⁶Fortna (2009) finds that there has been a major decline in the number of inter- and intra-state conflicts ended by military victories as opposed to “draws” and peace agreements, since World War II for inter-state wars and since the end of Cold War for civil wars. She argues that the rise of peace-keeping operations provides the best explanation.

To summarize, a plausible conjecture is that the end of the Cold War eventually led to a major reduction in both the number and scale of civil conflicts, by (a) facilitating the focus of major powers on peace-keeping operations for civil wars, and (b) removing or undermining an ideology that supported and funded guerrilla insurgencies, and which, with the help of US intervention on the other side, escalated a lot of conflicts during the Cold War.

Table 2: The class of 1995

country	conflict	years	outcome	PKO?
sri lanka	LTTE, etc.	1983-09	govt victory	
china	Xinjiang	1990-98	govt victory	
tajikistan	UTO	1992-97	govt victory	yes
croatia	Krajina	1992-95	govt victory	yes
peru	Sendero Luminoso	1981-95	govt victory	
angola	UNITA	1975-02	govt victory	
angola	FLEC (Cabinda)	1992-04	govt victory	
chad	FARF, other rebels in south	1992-98	govt victory	
cambodia	Khmer Rouge, FUNCINPEC, etc	1978-98	govt victory, peace agreement	yes
bangladesh	Chittagong Hills/Shanti Bahini	1976-97	govt victory, peace agreement	
guatemala	URNG, various	1968-96	govt victory, peace agreement	yes
sierra leone	RUF, AFRC, etc.	1991-00	govt victory, peace agreement	yes
papua n.g.	BRA (Bougainville)	1988-98	peace agreement	yes
bosnia	Rep. Srpska/Croats	1992-95	peace agreement	yes
burundi	Hutu groups v. govt	1993-06	peace agreement	yes
sudan	SPLA, etc.	1983-05	peace agreement	yes
uk	IRA	1969-98	peace agreement	
haiti	Mil. coup	1991-95	rebel victory	yes
rwanda	RPF, genocide	1990-02	rebel victory	
liberia	NPFL (Taylor), INPFL (Johnson)	1989-96	rebel victory, peace agreement	yes
indonesia	E. Timor	1975-99	secession, weak state	yes
afghanistan	v. Taliban	1992-01	ongoing, high level	yes
india	Naxalites	1988-	ongoing, high level	
somalia	post-Barre war	1991-	ongoing, high level	
philippines	MNLF, MILF	1970-	ongoing, low level	
burma	CPB, Karens, etc.	1948-	ongoing, low level	
philippines	NPA	1969-	ongoing, low level	
india	Kashmir	1989-	ongoing, low level	
india	N.East rebels	1956-	ongoing, low level	
pakistan	MQM:Sindhis v. Mohajirs	1993-99	ongoing, low level	
russia	Chechnya	1994-96	ongoing, low level	
colombia	FARC, ELN, etc	1963-	ongoing, low level	
algeria	FIS,GIA, GSPC	1992-	ongoing, low level	
turkey	PKK	1984-	ongoing, low level	
israel	Palestinian insurgents	1949-	ongoing, low level	
chad	FROLINAT, various ...	1965-	ongoing, low level	
senegal	MFDC (Casamance)	1989-	ongoing, low level	
uganda	LRA, West Nile, etc.	1989-	ongoing, low level	
ethiopia	Oromo Lib. Front	1992-	ongoing, low level	

Governance and growth

6. The above raises questions about whether we are focusing too much on “security” – in the sense of getting rid of violent internal conflicts – as a or the key development problem. This is obviously *the* key problem for a few countries, such as Afghanistan or Somalia, but arguably for very few others.⁷

7. Some of the interest in conflict from development policy and academic types comes from a broader notion, however. Perhaps overt, violent conflict is just a particularly striking indicator of *bad governance* or *bad institutions*, and it is these things that are the main obstacle to economic growth in many poor countries.

The idea is that while more human capital and productively employed physical capital would be nice, you won’t get or sustain either without better political institutions and better governance (where “governance” refers variously to good policies and/or effective institutions). If this view is adopted, then the question for foreign aid and other external interventions is “how do you help to build a well-functioning state?” – that is, one that has good institutions and is well governed.

The U.S. faced this question in acute form, and not very successfully, in Iraq. The U.S. and NATO continue to face it in Afghanistan. United Nations PKOs have faced extreme state-building challenges in East Timor, Kosovo, Namibia, Cambodia, and other some other instances of “Transitional Administration” missions. Some UN personnel have acquired a lot of experience and practical skills in the area, which is perhaps under-realized and exploited by the development sector. Development aid routinely faces less extreme versions of the question, as more and more aid has gone into “governance” and “capacity building” projects (or, the addition of “capacity building” to all manner of aid projects).

8. Some minimum level of decent governance and political institutions are surely close to necessary conditions for economic development, and my own hunch would certainly be that these are important factors more generally.

But it is worth pointing out that the evidence that variation in governance and/or institutions explains a large amount of variation in decade-to-decade growth rates in poor countries is not very good. Below, I consider whether measures of different dimensions of “good governance” produced by the International Country Risk Guide (ICRG) and by Kaufmann, Kraay and Mastruzzi (2009) predict subsequent growth performance, controlling for initial level of income, prior growth rates, and other factors. Lagged ICRG measures – “investment profile,” “rule of law,” “corruption,” and “bureaucratic quality” – do not, in a country-decade panel with data for the 1990s and 2000s. Most of the Kaufmann and Kraay “Worldwide Governance Indicators” (WGI) do, in a cross-section

⁷I don’t think development implications are the only reason we should be concerned about violent conflicts, but take it that a focus of this meeting is security and economic development.

where growth from 1997 to 2008 is regressed on governance indicators in 1996, plus other covariates. But the amount of variation explained is still pretty small, and if one is worried about bias from omitted variables the concern would probably be that the bias is upwards.

The associations might be weak for any of several reasons. It could be that these measures of governance and institutions are just very bad measures. If so, then perhaps we should talk about what we think “good governance” and “good institutions” (the objectives of official third-party “state building” efforts) are and how they should be measured.

There are other possibilities. It could be that short of really horrific governance, this isn’t really the key thing for sustained economic growth. It could be that sustained growth does require good governance and institutions, but even with these it typically takes a very long time, perhaps many decades, for good governance to provide the foundation for sustained, rapid growth.⁸ Hypothetically, it could be that there is some omitted variable that is causing good governance but also low growth, so that estimates like those reported below are biased downwards.⁹

9. Kaufmann, Kraay and Mastruzzi (2009) have been producing a set of six governance indicators almost annually since 1996. Their procedure is to cluster a large number of mainly expert-ratings based indicators into six categories – which they term government effectiveness, voice (a democracy and accountability measure), political stability, rule of law, control of corruption, and something called regulatory quality – and then use a factor analysis-like method to extract a common dimension. All six measures prove to be highly correlated with per capita income, and also with each other. With governance measures, all good things go together.

Surprisingly, the WGI indicators have barely been used for studies of economic growth (as far as I know). Kurtz and Shrank (2007) found that the government effectiveness measure was not related to average growth over the next two years, looking separately at three different years of WGI measurements. But annual growth rates are so noisy that the null finding may not tell us much.

Table 3 reports models where the dependent variable is PWT 6.3 per capita annual income growth averaged from 1997 to 2008, regressed on the six WGI measures of governance quality measured

⁸On this argument, perhaps the residual variation in the governance indicators after effectively removing per capita income is just nothing compared to the long run summary of good institutions encapsulated in the income measure. If true, this would still be a concern for development policy, which can only aspire to help make incremental improvements in rule of law, government effectiveness, corruption, and so on.

⁹For instance, what if countries with particularly bad growth prospects for reasons of “economic fundamentals” tend to develop and/or choose good governance? This seems implausible as soon as it is stated – more likely the bias would be in the other direction, if there is one. The latter concern (that good economic prospects might cause or enable the development of good institutions and policies) motivates Acemoglu, Johnson and Robinson (2001) in an attempt to find instruments for “good institutions.”

in 1996, plus other covariates. The WGI measures have a standard deviation of about 1 by construction, so interpretation of “effect” magnitudes is straightforward. The covariates are per capita income in 1995, growth rate from 1985 to 1995, a dummy for oil producers in 1996, and dummies for five of six world regions (not shown).

This set up is motivated by the desire to get the longest possible growth period after a measurement of governance. There is no point in regressing current growth on current governance indicators, because observation of good economic performance is quite likely to cause experts to rate governance in a country more favorably.¹⁰ And there is little point to trying country fixed effects, because there is very little over time variance in the governance indicators for the 12 years for which we have data.

With initial income in the models, we are in effect asking about the relationship between growth and the part of the governance measure that is not related to per capita income. You could call this “surprisingly good governance” given income. Identification of a causal effect depends on the extent to which surprisingly good governance arises for reasons that are uncorrelated with other determinants of growth rates in the next ten years. This is far from iron-clad, but it seems reasonable to me that quality of governance varies over time within countries to some extent for reasons having to do with vagaries of politics and leadership. Also, to the extent one is worried about upward bias caused by an omitted variable that drives surprisingly good governance and subsequent growth (but not through governance), if the estimates are small this would be discouraging.

The coefficient estimates for the WGI measures are all positive and significant except for “voice,” which appears to be basically a democracy measure. The substantive magnitude of the effects is large – a country that was one standard deviation higher on a governance indicator (except voice) averaged one or more percent faster growth, annually, for 1997-2008.

Regarding the other variables, we see some convergence effects for initial income when we control for governance quality. Oil producers did very well in the last 12 years, and it is important to include this variable because oil producers tend to be rated as having bad governance, other things equal. Growth rate in the previous decade is basically unrelated to subsequent growth for this sample.¹¹

(I have also considered adding various measures of civil conflict, both for 1997-2008 and for the

¹⁰I have also used the average of the governance indicator for 1997-2008, instrumenting with the indicator in 1996. Results are similar except that the coefficients on the governance indicators are typically around two times larger.

¹¹Results are similar without the regional dummies, but a bit weaker for the governance measures. I suspect that this may be partially because the ratings on which the measures are ultimately based are often implicitly regional, in the sense that, for instance, Botswana gets generally better marks than it would if it were in Europe.

earlier period. Either way this has almost no effect on the governance coefficients, which suggests that poor governance is not mainly associated with lower growth because it is associated with prior or contemporary violent conflict.)

Table 3: WGI governance and growth, 1997-2008

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
log(income)96	-0.29 (0.29)	-1.09** (0.34)	-0.30 (0.30)	-0.67* (0.33)	-0.90* (0.35)	-0.80* (0.34)	-0.84* (0.33)
grwth8595	-0.07 (0.06)	-0.13* (0.06)	-0.07 (0.06)	-0.08 (0.06)	-0.11 [†] (0.06)	-0.10 (0.06)	-0.11 [†] (0.06)
oil96	1.80* (0.71)	2.66*** (0.71)	1.81* (0.76)	2.05** (0.71)	2.52*** (0.74)	2.55*** (0.76)	2.59*** (0.73)
ge96		1.59*** (0.41)					
voice96			0.01 (0.34)				
polstab96				0.64* (0.30)			
rol96					1.19** (0.41)		
corrupt96						1.05* (0.41)	
regqual96							1.19** (0.37)
constant	5.60* (2.38)	12.73*** (2.92)	5.63* (2.60)	8.87** (2.81)	10.96*** (2.98)	10.29*** (2.97)	10.70*** (2.81)
<i>N</i>	183	183	183	183	183	183	183
<i>R</i> ²	0.18	0.25	0.18	0.20	0.22	0.21	0.23
adj. <i>R</i> ²	0.14	0.21	0.14	0.16	0.18	0.17	0.19

Standard errors in parentheses. Region dummies included but not shown.

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

10. So far, that's encouraging. Note however that the amount of variation in growth rates explained remains quite small, with governance indicators accounting for only 2 to 7 percent.

The results when we use the ICRG indicators are much less encouraging. These are produced by the company Political Risk Services – apparently on the basis of expert surveys – for sale to businesses and investors interested in the business climate in different countries. An early version of these indicators (measures for “contract enforcement” and “expropriation risk” from 1982) have been used some well known papers on institutions and growth (Acemoglu, Johnson and Robinson 2001; Knack and Keefer 1995; Mauro 1995). However, PRS has continued to pro-

duce the indicators, in slightly different format, so that one can now get a time series back to 1984. As far as I know this longer series has not been used in the growth literature.

The ICRG measures are “investment profile,” which seems to have subsumed “contract enforcement” and “expropriation risk” and varies from 1 to 12; “law and order,” which varies from 1 to 6; “corruption,” which varies from 1 to 6 with higher values being less corruption; and “bureaucratic quality,” which varies from 1 to 4.

I construct a country-decade panel for the 1980s, 90s, and 00s; the dependent variable is average annual growth rate in each decade. Using lagged variables, this makes for two waves, the 90s and 00s.

Here there is not much of a relationship between governance measured in the previous decade and growth in the next decade.¹² Only “investment profile” is statistically significant, and in terms of magnitude these “effects” work out to less than half the size as those for the WGI measures, even in the best cases (looking at a one standard deviation difference).

Governance and conflict

I recently wrote a long background paper for the World Development Report 2011, which is focused on conflict and development (Fearon 2010). The paper reviews and tries to reproduce findings from a fairly large cross-national statistical literature that has developed in the last 10 years or so, focused on identifying covariates of civil war and lower-level conflict onset. Here is a very brief summary (for references to the literature, see the paper).

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

¹²Results are similar if we use the governance measure from 1989 and 1999 instead of the average over the whole decade.

Table 4: ICRG governance and growth, 90s and 00s

	Model 1	Model 2	Model 3	Model 4
$\log(\text{income})_{t-1}$	-1.07** (0.33)	-0.94** (0.33)	-0.89** (0.33)	-0.87* (0.35)
growth_{t-1}	0.06 (0.10)	0.09 (0.10)	0.09 (0.10)	0.10 (0.10)
oil	1.70** (0.61)	1.69** (0.64)	1.76** (0.66)	1.69* (0.65)
year = 2000	1.92*** (0.36)	1.80*** (0.37)	1.99*** (0.37)	1.95*** (0.36)
ip_{t-1}	0.36* (0.18)			
rol_{t-1}		0.30 (0.20)		
corrupt_{t-1}			0.26 (0.28)	
bq_{t-1}				0.16 (0.24)
constant	9.04*** (2.55)	9.13*** (2.67)	8.81** (2.68)	9.15** (2.85)
N	247	247	247	247
R^2	0.28	0.28	0.27	0.27
adj. R^2	0.25	0.24	0.24	0.24

Country clustered se's in parentheses

Region dummies included by not shown.

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

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 diversity, again leading to spurious correlations. So it is important to keep in mind that much caution is called for in interpreting many of these associations.

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 (“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 in the background paper, and so may have a stronger claim on being (or marking) causal effects.

The paper also examines 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 the background 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.¹³

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, which 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 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

¹³But see Fearon (2005) and papers in the May 2010 special issue of the *Journal of Conflict Resolution*, which focuses on state capacity.

“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.

However, as seen in the previous section, it’s not clear just how big a kick to growth would result from better governance and institutions in most poor countries. It may be that there is a handful of countries where it is obvious that solving a major institutions/governance/conflict problem is necessary for economic progress, and that for these a major focus on state-building and what domestic and regional agreements and programs would help with this is absolutely critical. But it may be that for most poor countries, at least at present, even better governance and institutions won’t help all that much, except perhaps over the long run.

Figure 1. Civil war prevalence

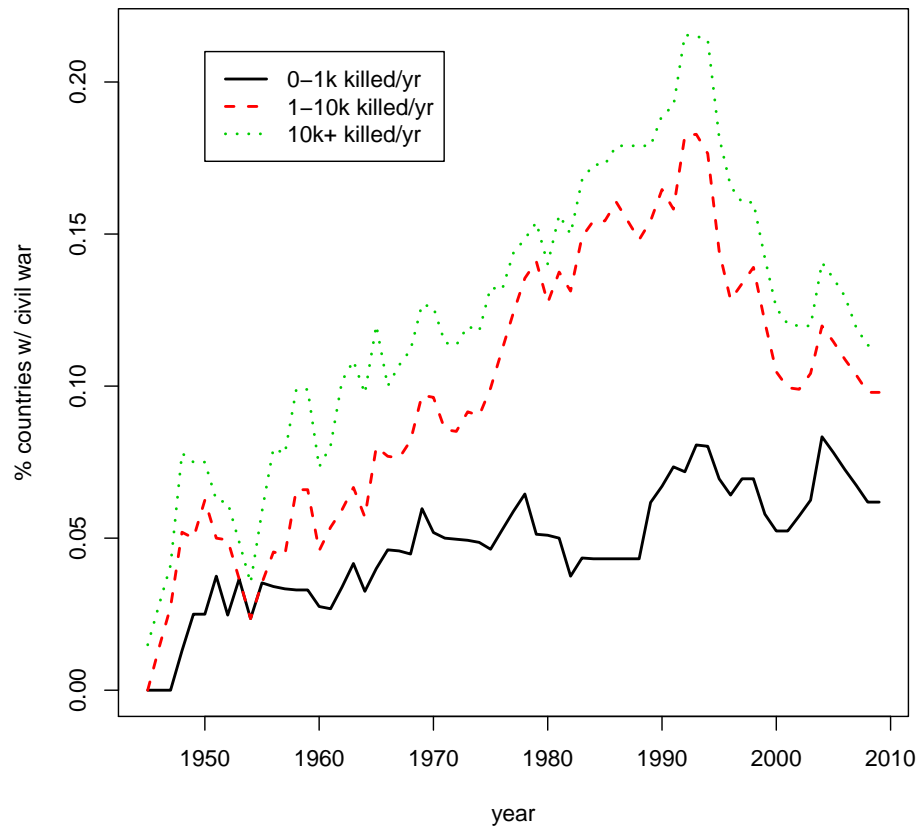
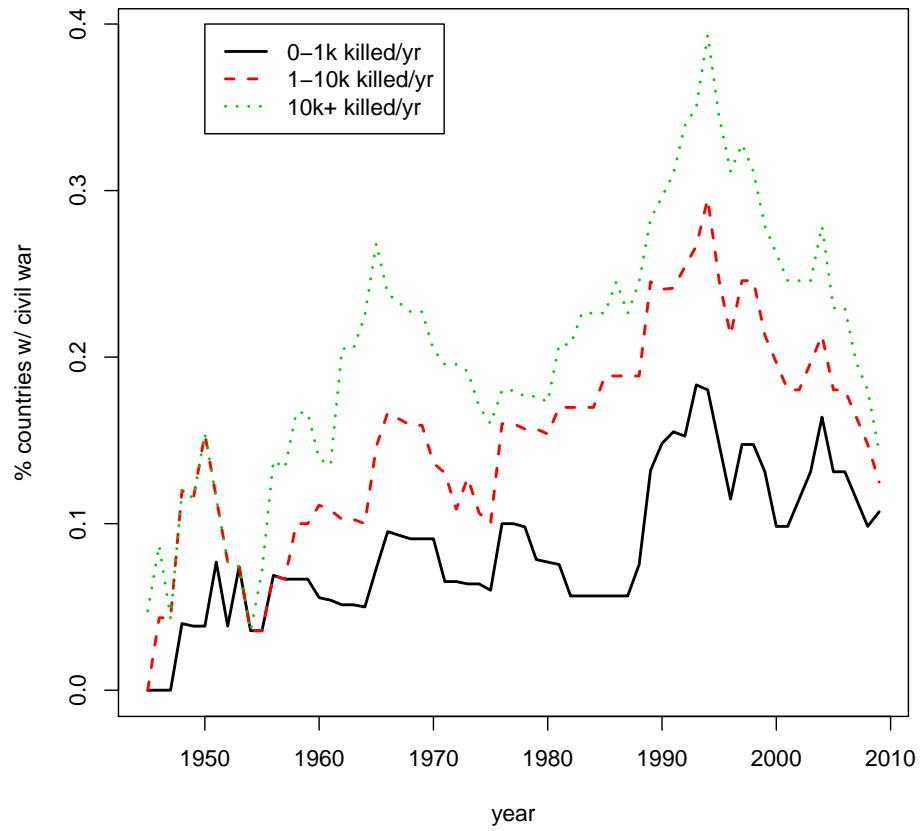


Figure 2. Civil war prevalence in poorest 1/3 countries



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