External Wars, Internal Conflict and State Capacity:

Panel Date Evidence

Mauricio Cárdenas, Marcela Eslava, Santiago Ramírez

EXTERNAL WARS, INTERNAL CONFLICT AND STATE CAPACITY: PANEL DATA EVIDENCE[1]

Mauricio Cárdenas[2]
Marcela Eslava[3]
Santiago Ramírez[4]

ABSTRACT

Relying on cross-country data, empirical studies have pointed at external wars as engines for the development of a state's capacity, and at internal conflicts as having the opposite effect. Concerns about possible reverse causality driving these results emerge, as the cross-sectional approach ignores the role of initial conditions and the persistence of state capacity. This paper re-examines the impact of external and internal conflict on state capacity using panel data to overcome these limitations. Two different data panels are analyzed, one covering countries, another covering Colombian municipalities. Beyond methodological differences with respect to previous work, we also add to the existing literature by looking at the impact of different attributes of conflicts: intensity, and types of conflict-related events. Large variability across municipalities allows us to zoom on multiple dimensions of conflict. We find that internal conflicts deteriorate state capacity both at the country and municipal level, and that more intense conflicts have a stronger negative impact. Moreover, episodes where civilians feel targeted affect the state's capacity to collect taxes, while those with more general reach affect the state's capacity to provide public goods. External conflicts, however, do not seem to affect on state capacity once initial conditions and endogeneity issues are taken into account.

[2] Senior Fellow, Brookings Institution. Email: mcardenas@brookings.edu
[3] Associate Professor of Economics, Universidad de los Andes. Email: meslava@uniandes.edu.co
[4] Graduate student, Universidad de Los Andes. Email: sa-ramir@uniandes.edu.co

1. INTRODUCTION

Despite its pedigree in various social sciences, state capacity is a relatively unknown concept in the economics literature in part because it is complex to define and measure. There are many interpretations in the political science and sociology literatures. In these contexts, state capacity is associated to military capacity, representing the state's ability to overcome rebellious actions with force, or to bureaucratic and administrative capacity, representing the ability of the state to conduct its business effectively and efficiently.

A recent interest for state capacity has emerged in economics. In the recent economics literature, a distinction is often made between "legal" and "fiscal" state capacity. Legal capacity refers to issues such as the availability of "contracting institutions" (i.e., institutions supporting private contracts) and "property rights institutions" (i.e., institutions constraining government expropriation), to use the terminology in Acemoglu and Johnson (2005). Fiscal state capacity deals with questions such as the ability to raise revenue from the society --typically measured by the GDP share of total taxes—and has been the focus of a number of contributions, including the forthcoming book by Besley and Persson (2011).

On the specific question of the relationship between conflict and state capacity, according to the "bellicist" approach to state building it is wars that make states. Military confrontations require increases in the level of taxation (the so-called ratchet effect) and demand greater state capacity. Fearing external domination, a consensus emerges around the idea of strengthening the state by increasing taxation. In this sense, wars are a rare moment of national unity, which is essential to build states. The work of Tilly (1990), among many others, reaches this conclusion based on the experiences of the U.S. and Western Europe. In fact, modern history is rich in examples of the association between wars and the introduction and development of the modern income tax systems. More broadly, Stubbs (1999) claims that war (or the threat of) has been an important factor in molding state institutions in the most successful economies of East Asia (namely, Japan, South Korea, Hong Kong, Malaysia, Singapore, Taiwan, and Thailand), while Desch (1996) looks into the cases of China, Cuba, Israel, and South Korea to conclude that their threatening external environments have resulted in stronger states.

Centeno (1997 and 2002) and López-Alves (2000) have explored the role of wars in state formation in Latin America. A major insight in their contributions is that external and internal wars are two distinct types of conflict with potentially opposing effects in the development of state capacity. While external wars are moments of unity and consensus, which facilitate the decision to invest in state capacity, internal wars are by definition divisive and destructive. The testable implication of this proposition is that external and internal have opposite effects on state capacity. In recent work, Besley and Persson (2008, 2009) provide empirical evidence that supports this view. Using cross-sectional data they show that the incidence of external wars is associated with stronger states, while the incidence of internal wars goes in the opposite direction.

These conclusions have been reached using cross-sectional data which has important limitations. Specifically, one of the main features of state capacity is that it persists over time, much like other institutional measures. This means that present state capacity is highly correlated with past state capacity. In addition, past state capacity may have been an important driver of a country's decision to engage in previous conflicts: stronger states may be more likely to fight wars with other states, while internal groups may be more likely to challenge the state if it is weak. This implies that ignoring the persistent nature of state capacity could lead to biased estimates of the effects of earlier conflicts. In other words, concerns about reverse causality arise when the effects of past state capacity are not properly acknowledged in econometric specifications. The nature of the cross-sectional data used in earlier work is such that earlier measures of state capacity cannot be taken into account, and so addressing these concerns requires a different empirical strategy.

The goal of this paper is to re-examine the impact of external and internal conflict on state capacity using panel data to overcome the above-mentioned limitations. Two different sets of data are analyzed: a panel covering cross-country information, and a panel of Colombian municipalities. The availability of panel data enables us to control for the persistence of state capacity and country fixed effects (including initial conditions), and use dynamic panel GMM estimation techniques to address concerns about the endogeneity of both conflict and other determinants of state capacity. This methodological approach also allows us to control for the level of development and other determinants of state capacity

that are not included in cross-sectional regressions due to concerns about them responding endogenously to state capacity.

Beyond these differences with respect to previous work, our analysis also contributes by examining how different types of conflicts affect state capacity. In particular, we analyze the effects of different levels of conflict intensity (measured in terms of numbers of casualties). We also take advantage of heterogeneity across Colombian municipalities in terms of conflict-related events to assess their differential impact on state capacity. One limitation of our databases is that they cover only a recent period, so we are unable to identify effects of conflict that may take place over the long horizons (which is the emphasis of the cross-sectional work mentioned above). However, if conflicts affect the incentives to invest in state capacity, then such relationship should show up in the data even in short horizons. Otherwise, the benefits of those investments would only be realized long after the war is over, calling into question the hypothesis that the capacity of the state was made stronger precisely to fight the conflict. Our results show that internal conflicts deteriorate state capacity both at the cross-country and within-country levels, and that more intense conflicts have a stronger negative impact. External conflicts, however, do not seem to have a clear effect on state capacity, once initial conditions and endogeneity issues are taken into account. We also find that some types of conflict-related events are negatively correlated with state capacity across Colombian municipalities. In particular, conflict manifestations that affect targeted civilians affect the state's capacity to collect taxes, while those with more general reach affect the state's capacity to provide public goods.

The paper is structured as follows. The next section presents the evidence related to the panel of countries. It starts by revisiting previous results, using standard cross-country OLS regressions, and then moves on to the dynamic estimations based on a GMM procedure. The following section focuses on the issue of internal conflict by using the panel of municipalities from Colombia. The final section provides a conclusion.

2. CROSS-COUNTRY EVIDENCE: THE PANEL DIMENSION

We begin by examining the relationship between the incidence of conflicts and state capacity for a panel of country-year observations covering the period 1975-2004. We first present the data, and then move on to the detailed empirical strategy and results.

Data

Our state capacity measures for the panel of countries relate to the fiscal and legal dimensions discussed above: we measure fiscal capacity with total tax revenue as a percentage of GDP and income tax revenue as a percentage of GDP (following Besley and Persson, 2009). Data on these variables comes from Baunsgaard and Keen (2010), who take total tax revenue data from the IMF's *Government Financial Statistics* (GFS) between 1975 and 2006 and improve it for countries outside the OECD. They do this with revenue information provided in the context of the IMF's periodic consultations with member countries, thus making the data more reliable.[5] We measure legal capacity through a summary indicator of the quality of government reported by the Quality of Government Institute (QOG), based on the International Country Risk Guide (ICRG).[6] This measure averages individual scores for three dimensions: law and order, corruption, and quality of bureaucracy. It takes values between zero and one, and increases with the assessed quality of government.[7] This variable is available for the period 1984-2008.

Turning to the explanatory variables, we use various measures of conflict from the *UCDP/PRIO Armed Conflict Dataset (version 3-2005),* also available in the QOG panel database. The data provides information on armed conflicts for the period 1946-2004. It records all armed conflicts following the definitions of the Uppsala Conflict Data Program

---

[5] Data for income tax as a percentage of GDP is only available until 2000.

[6] *The Quality of Government Dataset (QOG) from the QOG Institute at the University of Gothenburg* compiles annual information for the period 1946–2008. The datasets can be freely downloaded at http://www.qog.pol.gu.se/. For details see Teorell et al. (2009). Matching the country classifications between the different data sources we use requires additional assumptions that are explained in the appendix. We use current countries only but to get historical data in cases of unification (division) we use the absorbing (original) country.

[7] The QOG measure is similar to the one constructed by Knack and Keefer (1995), and later used by Hall and Jones (1999), to quantify the quality of government. Knack and Keefer (1995) average 5 of the original 24 categories created by the ICRG to rank countries. These five categories are "law and order," "bureaucracy quality," "corruption," "risk of expropriation" and "government repudiation of contracts." QOG only uses the first three in its indicator of the quality of government because the latter two were discontinued in 1997.

(UCDP) at the Department of Peace and Conflict Research, Uppsala University, and the Centre for the Study of Civil War at the International Peace Research Institute in Oslo, Norway (PRIO). Conflicts are defined as such when there are at least 1,000 battle-related deaths over the full span of the episode. Internal conflicts are those that occur between the government of a state and internal opposition groups, without intervention from other states. External conflicts, meanwhile, are defined as those that occur between two or more states.[8]

Our control variables are year effects, real GDP per capita and a measure of democracy. Our dependent variables (in particular the fiscal ones) can be affected by global phenomena, such as economic crises, controlled for by the inclusion of year effects. Moreover, tax collections plausibly depend on income levels. (More generally, state capacity is a function of a country's level of development; we believe this statement applies more in the long run than for within-country variability, and in that sense country fixed effects should be the main way in which some of our regressions capture this specific mechanism). It has also been shown that inclusive political institutions are central to building state capacity (Besley and Persson, 2009; Cárdenas and Tuzemen, 2010). Country effects and lagged dependent variables are added in some of the specifications, in particular our preferred ones.

Real GDP per capita data comes from the QOG database, which in turn takes the information from Gleditch (2002).[9] As for democracy, we use a revised version of the Combined Polity Score from the Polity IV Project, named Polity2 (Marshall et al., 2009), which ranges from -10 (complete autocracy) to +10 (complete democracy). The index of democracy we use in our regressions is a dummy variable that takes the value of 1 if the Polity2 score, averaged over the five preceding years, is above 3.[10]

Table 1 reports descriptive statistics for our panel of countries. The sample covers 188 countries, with 140 of them having information on all variables, over 1975-2004. It is

---

[8] We use the UCDP/PRIO conflict dataset as opposed to the more conventional Correlates of War Dataset (COW) because it provides data up to 2004 while the latter only does so until 1997.
[9] Gleditsch (2002) fills gaps in the original data of the Penn World Tables using additional sources and extrapolation techniques.
[10] Results are robust to using a cutoff of zero rather than three.

worth noting that internal conflicts are much more frequent than external conflicts. In our sample of countries and years, 11 percent of the observations correspond to internal conflicts, while external conflicts represent only 2 percent of the observations. Regarding our state capacity measures, in the case of total tax revenues the sample average is 20.62 percent of GDP and 8.9 percent of GDP for income taxes. The average quality of government score is 0.55, in a 0-1 scale. Countries and years with a Polity2 score above 3 (which we define as democracies) represent 40 percent of the sample.

Baseline Estimations

We begin by revisiting the cross section evidence in Besley and Persson (2008 and 2009), who look at the relation between average state capacity between 1975 and 1997 and the previous occurrence of conflicts (either since 1945 or from the time of independence). Though we look at the effects of conflicts over a much shorter horizon, we take advantage of the time series variability offered by the panel structure of our data. This means that we cannot analyze effects that may take several decades to consolidate, but we can control for initial conditions and other country fixed effects that the pure cross section regressions ignore, and that may potentially bias the estimated coefficients.

As mentioned, if state capacity is persistent over time, the empirical relationship between past conflicts and current state capacity could be significant without there being a true causal relationship from the former to the latter. This would be the case, for instance, if countries with initially higher state capacity were more likely to engage in wars. Reverse causality is a clear possibility: In as much as wars make states it is also true that it is states that make wars. With persistent state capacity, this would show up in the data as a significant correlation between early conflicts and current capacity, unless initial conditions are properly controlled for.

To make our results more easily comparable to those by Besley and Persson, we initially explore a baseline specification that does not control for fixed effects, and does not take persistence into account. In particular, we estimate the following specification (without fixed effects):

$$SC_{it} = \beta_0 + \beta_1 IC_{it} + \beta_2 EC_{it} + \gamma' X_{it} + \delta' D_t + u_{it} \qquad (1)$$

where SC_{it} is a measure of state capacity in country i in year t; IC_{it} is 1 if country i had an internal conflict in year t, and 0 otherwise; EC_{it} is 1 if the country is part of an external conflict in that year, and 0 otherwise; X_{it} is a vector of controls: GDP per capita (in logs) and our index of democracy; and D_t is a vector of year dummies to control for global effects. We first estimate equation (1) including internal and external conflicts sequentially, and then include the two jointly.

The results from an OLS estimation of equation (1) using our panel of countries are presented in Table 2. The table shows a strong negative correlation between internal conflict on both fiscal and legal state capacity. Tax revenues as a percentage of GDP are close to 5 percentage points lower in countries and years with internal conflicts (and 2.5 percentage points lower in the case of income taxes). The negative effect on the quality of government is also large (it falls close to one half of a standard deviation during conflict years). These results for the correlation between state capacity and internal conflict hold whether or not we control for external conflicts. hold

The results for the relationship between external conflicts and state capacity are less robust. Besley and Persson (2008) find that total tax revenue (as a percentage of GDP) is higher in countries with greater average incidence of external war. We get the opposite result: countries and years with external conflict are associated with lower tax collection in our OLS regressions. In particular, in columns 2 and 3 we find that tax revenues as a percent of GDP fall in the presence of an external conflict; the drop is between 2 and 3 percentage points, depending on whether the incidence of an internal conflict is controlled for. , There is also a decrease of income taxes in the presence of external conflicts, of a magnitude close to 1.5 percent of GDP (columns 5 and 6.[11] In the case of legal state capacity, we find no statistically significant effects of conflicts, except when both types of conflicts are considered simultaneously (column 9). There is a positive correlation in the latter case. Overall, there is no consistent message across dependent variables in these OLS regressions: we find apparent negative relationships between any type of conflict and fiscal

---

[11] Excluding GDP from our list of controls, to mirror Besley and Persson's specification more closely, does not change the sign of the estimated coefficient measuring the impact of external conflict.

capacities, while legal capacity seems negatively correlated with internal conflict and ambiguously related to external conflict.

Both the results we present above and those from previous work should be taken with caution as they may be driven by the omission of initial state capacity conditions. These conditions are potential determinants of both contemporaneous state capacity and the probability that a country initially entered a conflict. To address the limitations of OLS regressions, we estimate the effect of conflicts on state capacity in a specification that takes into account country fixed effects and the potential persistence of state capacity over time. In particular, we use a dynamic panel data model, which allows us to capture the effect of past state capacity and country fixed effects on current state capacity, while addressing endogeneity problems. This approach also implies that we focus on relatively short run effects of conflicts on a state's capacity, as only within-country variability is taken advantage of.

Dynamic Panel GMM Estimations

Our basic dynamic panel model is of the following form:

$$SC_{it} = \beta_0 + \alpha SC_{it-1} + \beta_1 IC_{it} + \beta_2 EC_{it} + \delta' D_t + \gamma' X_{it} + \varepsilon_{it} \tag{2}$$

where

$$\varepsilon_{it} = \mu_i + v_{it} \tag{3}$$

and

$$E[\mu_i] = E[v_{it}] = E[\mu_i v_{it}] = 0 \tag{4}$$

where SC_{it-1} denotes the lagged state capacity variable to capture the persistent nature of state capacity; X_{it} is the same vector of controls as in (1); μ_i are country fixed effects and v_{it} are idiosyncratic shocks, which we assume are orthogonal to each other. As before, we introduce the two types of conflict sequentially first, and then simultaneously. We also run an alternative specification including more lags of the dependent variable as regressors. Our central results are robust to this change, but statistical tests show that the inclusion of

additional lags is necessary to support our choice of instruments in the case of legal capacity (not so for fiscal capacity).

By introducing country fixed effects and the lag of the dependent variable in the model, equation (2) takes into account the possible effects of initial conditions, a country's level of development, other sources of unobserved time-invariant heterogeneity, and persistence in state capacity. At the same time, the specification is subject to the problems of endogeneity for the lagged dependent variable that are standard in dynamic panel data models (e.g. Arellano and Bond, 1991; Blundell and Bond, 1998). Also, both state capacity and the probability of facing conflicts may be affected by third shocks that are unobserved by us (e.g. political reform). This would introduce additional endogeneity problems, directly related to our variables of interest. Reverse causality is also possible, since current state capacity may affect the probability that a conflict involving the state occurs.

We address the aforementioned problems by implementing a one-step "System" GMM estimator for equation (2) (Arellano and Bover, 1995; Blundell and Bond, 1998). We consider state capacity as a predetermined regressor in our model, and both of our conflict measures as endogenous variables, given the possibility of both reverse causality and simultaneity bias. Our instrument for the lagged dependent variable is its own first lag, while we instrument all other endogenous variables with their own second lags in the differenced equation.[12] The results we report correspond to a specification where GDP is considered exogenous, but the effect of conflict on state capacity is not altered if we declare GDP as an endogenous variable; however, in the latter case instrument proliferation impedes an appropriate evaluation of the join exogeneity of instruments.[13] It should be noted, in any case, that the causality from state capacity to GDP should materialize mainly in the long run; given that we control for country fixed effects, and thus focus on within-country variability, declaring GDP as exogenous in the present setting is not implausible.

---

[12] Our results are robust to using the more standard approach of instrumenting all endogenous and predetermined variables with their own first and second lags, but the Hansen test for joint instrument exogeneity has a p-value of 1, suggesting instrument proliferation does not permit judging on their exogeneity. We thus choose the most parsimonious specification supported by specification tests, which is the one we report, but point that our main results are robust to many other choices of instruments.

[13] In particular, Hansen tests show p-values of 1 when GDP is declared endogenous, for different designs of the instrument matrix.

In addition to instrumenting our endogenous variables with their own lags, a *physical integrity rights index* is used to instrument internal conflict, while a measure of *political globalization* is used to instrument external conflict.[14] Our physical integrity rights index is taken from the updated Cingranelli and Richards' (1999) Human Rights Dataset, and covers 189 countries and the period 1981-2004. It is an additive index summarizing the *torture*, *extrajudicial killing*, *political imprisonment*, and *disappearance* components of the dataset. It ranges from 0 (no government respect for these four rights) to 8 (full government respect for these four rights).[15] Political globalization, in turn, is measured by the number of embassies and high commissions in a country, the number of international organizations of which the country is a member, the number of U.N. peace missions the country participated in, and the number of international treaties the country has signed since 1945. The information comes from Dreher (2006) and Dreher, Gaston and Martens (2008), and covers 155 countries throughout the period 1970-2006. The index ranges between 0 and 100, where higher values indicate a higher degree of globalization.[16]

The choice of the physical integrity rights index as a relevant instrument for internal conflict is based on the argument that governments tend to be less respectful of human rights when engaged in internal conflicts. Most civil wars show human rights violations that would translate in a deterioration of our index score. Regarding exogeneity, we assume state capacity and the respect for physical integrity rights are only correlated through their respective relationships with the occurrence of internal conflicts, and that there is no additional channel connecting human rights violations and state capacity. While weaker governments may be less respectful of human rights even in absence of conflict, we believe this is true in particular in terms of long run relationships. Since we exploit only the within-country variation for our GMM estimations, our expectation is that this is a valid instrument. This assessment is indeed supported by the exogeneity tests we carry, except for some of the results on the relationship between conflict and legal capacity.

---

[14] Our results are similar if we instrument the conflict variables only with their lags, but the Hansen J tests for this alternative specification reject the exogeneity of the matrix of instruments for some of our dependent variables.

[15] Further details can be found in Cingranelli and Richards (1999). The data is available in the QOG database.

[16] The Political Globalization index is available in the QOG database.

As for the second instrument, we expect that more politically globalized countries are less likely to engage in external conflicts.[17] Deeper political globalization would reflect a preference toward the use of diplomatic means to solve disputes. We also expect political globalization to not affect state capacity other than through its effect on external conflict. The possible effects of political globalization on other determinants of state capacity, such as economic and political inequality (Cárdenas and Tuzemen, 2010; Cárdenas, 2010) are more likely to materialize in the long run than to be captured by the annual within country variation we focus on. Recent events of this decade, such as the intervention of the United States in Iraq and Afghanistan, are evidence that political equality (or more democracy) is not easily installed overnight by the international community and it is usually preceded by some form of conflict. Again, results of exogeneity tests support the inclusion of this instrument together with those described above.

It is worth mentioning that the System GMM estimator requires that the first-differenced instruments used for the variables in levels be uncorrelated with the unobserved country effects. We make this assumption in all our estimations. That is, we assume that the first differences of both our lagged values of state capacity and contemporaneous values of conflict are uncorrelated with any country-specific characteristics. While the levels of conflict and state capacity must be correlated with country fixed effects, it seems plausible to assume that <u>changes</u> in these dimensions do not reflect fixed characteristics of countries.

Table 3, panel 1, shows our estimates of equation (2) using our preferred specification of the "system" GMM methodology. In panel 1, columns 1-9 show a strong negative effect of internal conflict on state capacity, both fiscal and legal, which is in general statistically significant. The exception is column 6: an effect of internal conflict on income tax revenues as a percent of GDP cannot be identified when external conflicts are controlled for. Most importantly, we are also unable to uncover any effect of external conflicts on state capacity, independent on whether we focus on fiscal or legal capacity, and independent of whether internal conflict is controlled for or not.

---

[17] In fact, the occurrence, the duration and/or the intensity of internal conflict could also be affected by political globalization. International organizations and external countries tend to be involved in conflicts experiencing internal conflicts. In this sense, political globalization is also a relevant instrument for internal conflict.

Our point estimates show that the existence of an internal conflict in a country in a given year will reduce its total tax revenue and income tax revenue (as percentages of GDP) by 1.4 and 1.0 percentage points, respectively. This relationship holds when we also include external conflict in the equation (columns 3 and 6), although the magnitudes are reduced by close to 0.05 percentage points in each of the two above cases. Meanwhile, columns 7 and 9 show that, on average, if a country is involved in an internal conflict in a particular year, its quality of government score will drop about 0.03 points on a 0-1 scale, or 13 percent of the standard deviation of our legal capacity index.

Specification tests support our choice of instruments for the regressions in which fiscal capacity measures are the dependent variables. However, the same cannot be said about the regressions where effects on legal capacity are examined (columns 7-9.) When legal capacity is the dependent variable, there is second-order autocorrelation of the estimation error, and the set of instruments used are not exogenous as a group, according to the Hansen J test. This suggests that results regarding legal capacity in panel 1 should be taken with caution. We address these problems in Panel 2, by estimating a second version of (2) including additional lags of the dependent variable. Our results regarding the effect of internal conflict on legal state capacity are robust to this change, in terms of both sign and significance, and now the specification in columns 7-9 passes the tests on serial autocorrelation for the errors and on joint exogeneity of the instruments. The magnitude of the effect of internal conflict in columns 7-9, panel 2, is similar to that found in panel 1 and discussed above. That is, while the inclusion of additional lags of the dependent variable addresses concerns regarding the choice of instruments and specification, the central message that internal conflict reduces the state's legal capacity, while external conflict has no effect, remains unchanged. As for columns 1-6, additional lags of the dependent variable are generally not significant, suggesting panel 1 has the correct specification; this is also supported by specification tests, which show second-order autocorrelation and instrument proliferation in panel 2 for fiscal capacity. It is still worth mentioning that the message regarding the effects of internal and external conflicts on the fiscal dimension of state capacity also remains robust to using the specification in panel 2.

Three general points are important when comparing these system GMM results to our original OLS results. First, in terms of the effect of internal conflicts, the magnitude of the coefficients is reduced considerably, albeit the same statistically significant negative sign is observed. The important change in our point estimates suggests that dynamic bias and endogeneity were indeed biasing OLS results. Second, system GMM results show that, in addition to long-run effects identified in previous work (Besley and Persson, 2008 and 2009), there are also shorter-run effects of internal conflict on state capacity, both on the fiscal and legal dimensions. And third, once dynamic bias and endogeneity problems have been corrected, there seems to be neither a negative impact of external conflict on fiscal capacity which was an initial finding with our OLS estimation—nor a positive effect as found by Besley and Persson in their cross-section regressions. Although we are fully aware that the latter regressions possibly capture a long-run relationship not reflected in our regressions, it is also true that they are subject to potential reverse causality (not solved by the long lag introduced in their specification due to the persistence of state capacity).

Conflict Intensity

One concern with our previous OLS and GMM estimations (as with previous work) is that they neglect the possibility that the level of intensity of a conflict may determine the magnitude and even the sign of its impact on state capacity. It can be argued that internal conflicts also generate incentives for the government to invest in state capacity in order to build-up the ability to defeat opposing groups, so that the effect of internal conflict on state capacity could even be positive. Moreover, these incentives may vary with the level of intensity of the conflict. Conflict-led investments in state capacity, one could argue, are particularly likely if either internal conflict is weak enough that internal division is not important, or if it is intense enough that popular discontent with rebel groups pushes the government to invest in building up its capacity.[18] To test this proposition we look at the possibility that the effects of conflict intensity on state capacity may not be monotonic. Though these hypotheses about the effect of conflict intensity refer more naturally to the

---

[18] In fact, popular wisdom in Colombia is that the recent success of the government in fighting the guerrillas is the result of investments (in particular in strengthening the military) made possible by growing popular discontent around the conflict.

effects of internal conflicts, for completeness we also look for effects that are non-linear, with respect to intensity, for external conflicts.

We classify conflicts into minor, intermediate, and war-scale according to the number of battle deaths involved. "Minor" conflicts correspond to those with at least 25 battle-related deaths per year for every year in the period of conflict. "Intermediate" conflicts are those with more than 25 battle-related deaths per year and a total conflict history of more than 1000 battle-related deaths, but fewer than 1000 per year. "Wars" are those conflicts with 1,000 or more battle-related deaths per year.[19] The thresholds that we use to divide these categories follow the ranges reported in the UCDP/PRIO database.

Table 4 shows descriptive statistics for these additional conflict measures. Throughout the period 1975-2004, 5 percent of country-year observations indicate the presence of minor conflict. The corresponding shares for intermediate conflicts and wars are 6 percent and 5 percent, respectively. As for external conflicts, 2% of observations in our sample are external wars, while minor and intermediate conflicts correspond to 1% of our observations each.

Table 5 shows our results when several levels of conflict intensity are used. Panel 1 presents OLS estimates, while panel 2 presents their GMM counterparts. For each dependent variable, the first column looks at internal conflict in isolation, allowing for heterogeneous effects of minor, intermediate, and major conflicts, while the second column does the same for external conflict. The third and final column looks at the heterogeneous effects of internal conflicts of different intensities, while controlling for the occurrence of external conflict (as defined in the previous section). The latter column is our preferred specification, because it explores the central issue of heterogeneity in the effect of internal conflict, keeping the occurrence of external conflicts constant. There are two reasons to not allow for heterogeneity in the effect of external conflict in this specification. First, we want to keep a plausibly parsimonious specification to avoid losing too much precision in the

---

[19] Note that only "intermediate" and "major" conflicts are considered in our dummy of conflict occurrence in Tables 2 and 3. This is for consistency with the more generally used definition of conflict, which considers events involving 1,000 or more casualties.

estimation of the effects we are most interested on. Second, as we will see, we do not find clear evidence of heterogeneity in the effect of external conflict (in the "middle" columns).

Focusing on our preferred GMM estimates, we do not find clear evidence of non-linearities in the effects of internal conflict. On the contrary, conflicts of a higher intensity always have a stronger negative effect on state capacity than internal conflicts of a lower intensity. The impact of minor conflicts, although negative, is in general statistically insignificant and of much lower magnitude than that of the other two types of conflict. This evidence runs counter to the idea that very intense internal conflicts can trigger investment in state capacity. If anything, the opposite may actually hold: only when the magnitude of conflict is low investment in state capacity offsets the predatory effects associated with civil unrest. As for potential differential effects of different levels of external conflict, columns 2, 5 and 8 in general show non significant effects of external conflicts, at any level of intensity. It is important to point that second-order autocorrelation tests again reject our choice of instruments for the case of legal capacities. Given the consistent message we get across dependent variables, however, we interpret our results in this section as strongly suggesting that the negative effect of internal conflict on state capacity increases with the intensity of conflict. But, we point out that we have been so far unable to find a combination of instruments that supports our specifications when legal capacity is our dependent variable.

3. STATE CAPACITY IN A PANEL OF COLOMBIAN MUNICIPALITIES

We now delve deeper into the relationship between state capacity and internal conflict, using data regarding the Colombian conflict. We take advantage of the fact that the country has been immerse in a long-lasting internal conflict, with conflict-related events that vary in type and intensity both over time and across regions. This variability offers a unique opportunity to investigate several unexplored dimensions of the relationship between internal conflicts and state capacity. First, potentially differential effects of different types of violent events (homicides, displacement, kidnappings, attacks, military confrontations)

can be examined. Second, the effects of the intensity with which these events are observed can also be evaluated. Finally, we can focus on local investments in state capacity.

Studies that focus on regions within a single country have the advantage of eliminating much of the heterogeneity that cannot be controlled for in cross-country analyses. Colombia is a legally centralized country, so local governments of 1,104 municipalities at least in theory share the same basic legal capacity. In contrast, given the high degree of fiscal decentralization, Colombian municipalities differ greatly in terms of their fiscal abilities. Specifically, there is significant dispersion in terms of the municipalities' ability to raise local taxes or to invest in infrastructure with their own funds. We will refer to state capacity in the sub-national simply as the ability by the local government to collect its own tax revenues and invest in public works. Specifically, we use data on tax revenues and the total expenditure in roads as measures of fiscal capacity, the latter being a proxy for the ability of the government to deliver public goods. Tax revenue is a plausible measure of state capacity at the municipal level to the extent that local governments have legal authority to raise their own taxes, which is the case for municipalities in Colombia. There are some constraints on the type of taxes Colombian municipal governments can adopt. Taxes on production and sales, and on property, are the two major sources of municipal tax revenue.

The second measure of state capacity is public spending on roads, which captures the ability to provide services and promote development. First, the construction and maintenance of roads corresponds closely to the textbook concept of a public good. Second, regional governments decide how much to spend on roads with a high degree of autonomy. This comes in contrast to spending on other types of public services, such as education and health, where regional governments receive earmarked resources from the central government that cover much, if not most, of their spending. This is not the case in the infrastructure sectors. [20] Since we are interested in the effect of conflict on state capacity, and the destruction of productive capital is characteristic of internal conflicts (see Blattman and Miguel, 2009), focusing on one type of such capital (public roads) seems natural. In

---

[20] Drazen and Eslava (2010) have found that despite the inflexibilities introduced by earmarked revenues, local governments have some leeway to decide over spending in health and education. However, municipal governments are much less legally constrained in determining what to do with transport infrastructure.

particular, we examine whether active governments engage in expanded investments to counteract this effect, or on the contrary the capacity of the state to provide these services is also negatively affected by conflict. Finally, from a measurement standpoint, spending on roads is clearly separated in the municipality fiscal accounts. We must note, however, that even though local expenditures on road construction are decided with a relatively high degree of autonomy, the fraction of total public road funding that comes from local sources is relatively small, compared to national sources.

Data

Data on the two measures of fiscal capacity (tax revenues and expenditure in roads) come from Drazen and Eslava (2010) and are available annually from 1984 until 2002.[21] Income revenues and expenditures in roads are measured in constant 1998 pesos deflated with the national CPI. We create two variables, both at the municipal level: tax revenue as a percentage of total fiscal revenue (which includes capital income) and expenditure in roads as a percentage of total expenditure. Expenditure in roads is constructed as the sum of expenses on roads using resources from different possible sources: royalties and co-finance funds, current revenue, and other resources.

Data on internal conflict measures comes from various sources. Because of the intensity and pervasiveness of conflict in Colombia, conflict-related events have been well measured and registered. A key aspect of this is the availability of data at the municipal level for various manifestations of conflict since the 1990s. We use a database constructed by the Human Rights Observatory of the Office of the Vice President of Colombia. It contains data on internal conflict measures per year for 1,104 municipalities throughout the period 1993-2008. We construct our own five conflict intensity measures based on the information available. First, total offensive actions undertaken by the ELN and FARC (guerilla) and AUC (paramilitary) illegal groups. Second, total massacres perpetrated by these groups (a massacre is considered as such if it involves four or more deaths). Third, total confrontations between the three previously mentioned armed groups and Colombia's

---

[21]Drazen and Eslava (2010), in turn, use data from the office of the Comptroller General (*Contraloría General de la República*). The data corresponds to the figures in the financial report each municipality files annually. Unfortunately, data for years after 2002 is not fully comparable with earlier data, restricting the use of recent years in our sample.

Armed Forces. Fourth, total number of kidnappings (civil kidnappings, political kidnappings and kidnappings of members of the army) perpetrated by FARC, ELN or AUC. Finally, total number of deaths in each municipality caused by FARC, ELN and AUC in a given municipality in a specific year. The sources of deaths are civil homicide, political homicide and a homicide of a member of the army. Also, from *Acción Social* (Executive Office of the President of Colombia) we have counts of numbers of people forced into exile from the municipality (expulsion) for the period 1997-2009.

Data on control variables and instruments come from different sources. GDP per capita at the department level (available for the period 1984-2005), and municipal population are taken from DANE data. Royalties and cash transfers paid by the central government to the municipal governments are taken from the National Planning Department-DNP. Finally, we use as instrument in some of our estimations a dummy variable indicating the presence of one or more military bases in a municipality for a given year. This dummy variable takes the value of 1 if there is presence of one or more military bases, and 0 otherwise. We take this variable directly from previous work by Dube and Naidu (2010).[22]

Table 6 shows descriptive statistics of our state-capacity measures. Both tax revenue and expenditure in roads average around one tenth of total revenue and total expenditure, respectively, and show standard deviations of similar magnitude.

Municipality Panel Evidence

We now present our empirical strategy for analyzing the panel of municipalities.

---

[22] Dube and Naidu (2010) eliminate from their sample three military bases that, according to their source (www.globalsecurity.org), were created during their period of estimation (their choice is a precaution against the possibility of an endogenous response from conflict to military bases). After consultation with the Colombian National Army, Navy and Air Force we correct the date of creation of the Tres Esquinas base and include it and the other two in our database, acknowledging potential endogeneity both in Dube and Naidu's (2010) and in this paper. We address this concern by looking at standard tests on the exogeneity of our vectors of instruments.

As in the case of countries, we start by analyzing the relationship between conflict and state capacity in a model that takes advantage of the cross-sectional variability by not including fixed effects. Our baseline model specification is of the following form:

$$SC_{jt} = \varsigma + \lambda conflict_{jt} + \sigma' X_{jt} + k' D_t + \varepsilon_{jt} \qquad (5)$$

where $conflict_{jt}$ can be any of the conflict measures described above for municipality j and year t, X_{jt} is a vector of controls, and D_t denotes a vector of time dummies. For controls, we include the log of GDP per capita, the log of population, royalties and transfers from the national to the municipal government. Per capita income and population capture the possibility that larger and more developed municipalities may face greater incentives to organize complex local governments, with the ability to raise taxes and begin large infrastructure projects. In turn royalties and transfers from the national government are two main sources of income for municipal governments. Major recipients of these resources may face lower incentives to raise taxes. There is also some correlation between receipts of royalties and transfers and the presence of conflict-related events, as illegal groups become particularly strong in localities that are major rent recipients, where they seek to appropriate those resources. To that extent, royalty revenue would be an important source of omitted variable bias if not controlled for. It is important to mention that our data on royalties is available only for close to 50% of the municipalities (Table 6), so the inclusion of this control reduces considerably the number of observations in the regression analysis.[23]

OLS estimations of equation (5) are prey to the endogeneity problems discussed above for our cross-country analysis. In particular, a negative correlation picked up by our regressions would have to be interpreted with extreme caution, since both reverse causality and simultaneity are potential problems in this context. It is possible that municipalities with weak local governments attract illegal groups, and that isolation and other initial conditions may jointly determine the intensity of conflict and local state capacity. We address these issues by estimating a dynamic panel model, which controls for fixed effects

---

[23] However, our results about the effects of conflict are robust to excluding royalties from the sample and to using a dummy variable that takes the value of 1 when royalties are above zero and 0 when they are equal to zero.

and path dependence, and is carried out with the same GMM procedure discussed in the previous section. In particular, we estimate equations of the following form:

$$SC_{jt} = \varsigma + \eta SC_{jt-1} + \theta conflict_{jt} + \sigma' X_{jt} + \tau' D_t + \varepsilon_{jt} \tag{6}$$

where

$$\varepsilon_{jt} = \mu_j + v_{jt} \tag{7}$$

and

$$E[\mu_j] = E[v_{jt}] = E[\mu_j v_{jt}] = 0 \tag{8}$$

where the same previous assumptions are made regarding the disturbance, ε: the fixed effect remains orthogonal to the idiosyncratic component of the error term, as specified by (8). The vector of controls, X_{jt}, is the same as in (5), and by introducing fixed municipality effects, we focus on the within municipality effects of conflict variability. The specification also includes time dummies to control for shocks that are common to all municipalities. Finally, we instrument the lagged state capacity regressors with their first lags in the differenced equation and with their first differences in the levels equation. Our conflict variables are always instrumented with their second lags in the differenced equation and with the first lags of their first differences in the levels equation.[24]

One word of caution is in place before describing our results. The variability of our conflict measures is more limited across Colombian municipalities than across countries. More specifically, at the municipality level, conflict-related events are quite rare even in a country that is typically regarded as violent. As Table 6 shows, the number of events that affect municipalities is typically zero. Out of our six measures of conflict, four of them show that there are no conflict events per municipality per year for over 75 percent of the observations (massacres, military confrontations, kidnappings per 100,000 inhabitants and deaths per 100,000 inhabitants). The two exceptions are not much different: even in the top quartile of total attacks and displacement (forced expulsion), the numbers do not go above one attack per municipality per year and five displaced individuals per 1,000 inhabitants per municipality per year, respectively. The large number of zeroes in our conflict measures

---

[24] This choice of specification mirrors that for our cross-country analysis, and is supported by specification tests.

makes identification more difficult when using municipal data mainly because we have to rely on the variability across a small fraction of observations, while the rest of municipalities share zero events. In other words, the nature of the data subjects us to attenuation bias. Our narrative thus focuses on the results that are estimated precisely, and avoids inferring that conflict has no effect when we estimate coefficients that are statistically not significant.

Panel 1 of Table 7 shows our OLS estimates of equation (5), while panel 2 shows Blundell and Bond (1998) one-step system GMM baseline estimations of equation (6). One striking feature of the results that becomes immediately evident is that, while OLS regressions show widespread negative and significant correlations between conflict-related events and tax collection, these findings disappear when we focus on within-municipality variation, control for the persistence of state capacity, and instrument endogenous regressors. Moreover, all lagged dependent variables in the GMM estimations have large and statistically significant effects. One possible explanation for these results is that much of what is being picked up by our OLS regressions reflects reverse causation and the simultaneous effects of initial conditions on state capacity and conflict (as suggested by the evidence of high persistence in state capacity). This raises a word of caution about purely cross-sectional approaches that cannot address endogeneity issues in the context of the conflict-state capacity relationship. We must mention, however, that these results may also reflect that good part of the consequences of conflict-related events on state capacity materialize only in the long run, and are thus not identified by our strategy (and with the available data).

Focusing now on the GMM results reported in Table 7, which correspond to our preferred specification, other interesting features appear. First, different conflict-related events seem to affect state capacity differently. Tax revenue is negatively affected by kidnappings and displacement. Interestingly, among our measures of conflict, these capture more directly targeted damage mainly inflicted to civilians than the rest of measures. Meanwhile, we are unable to identify any significant effect of events of a less targeted nature, and that arguably affect members of the military and the illegal armed groups more than civilians: attacks, military confrontations and conflict-related deaths. Though the

coefficients for these types of events are, in general, negative, none is statistically significant. If our estimates, as we expect, are indeed a lower bound to the true effects of conflict, what we can state is that we find that kidnappings and displacement have a stronger negative effect on fiscal capacity than other conflict-related events. These findings seem consistent with the view that the confidence of the population on the state, and thus the willingness to pay taxes, can be undermined in societies where conflict makes civilians perceive the state as unable to protect them. Moreover, government agencies are frequently captured by illegal groups in the Colombian municipalities most affected by the conflict. Civilians perceive this reality and are likely less willing to pay taxes to governments they perceive as illegitimate.

Our point estimates suggest that these effects on tax collections are large. A one standard deviation increase in kidnappings (5.5 kidnappings per 100,000 people) reduces tax revenue as a share of total revenue by 14.4 percent, while one standard deviation increase in the number of displaced individuals per 1,000 inhabitants (34.5) is associated with a reduction in tax revenues (as a share of total revenues) by 27.6 percent.

Moving to our estimations of the effect of conflict on the provision of public goods (roads, in particular), we find a negative and significant impact of one specific type of event: attacks by illegal armed groups. Column 4 of Table 7 shows that a one standard deviation increase in the number of attacks by illegal armed groups (3.60) would reduce this public spending on roads by 13 percent in a typical municipality. It is possible that these events, or rather a perceived high probability of their occurrence, reduce incentives to invest in public infrastructure. This is a plausible implication of the fact that infrastructure is directly damaged by attacks (in fact, it is frequently the target of such attacks). It is also possible that the role of attacks by armed groups is acting here as a proxy for the role of a strong presence of illegal groups: attacks are most likely in regions where the illegal organizations have gained enough strength to mobilize large groups of their members to attack towns, military posts, and mobile military groups, and to destroy infrastructure. One possible interpretation of the finding that attacks affect governmental capacity to provide public goods is, thus, that it is the reflection of the link between local politics and conflict in regions where armed illegal groups are particularly strong. These groups traditionally

intervene in local politics in the regions where they exert most influence, frequently capturing the local elites. In many cases, these groups have been able to deviate local public resources to their own pockets, for instance by becoming part in consortia that then are designated to provide a given public service or construct infrastructure works.[25] Again, given our expectation that we are measuring lower bounds for the effects of conflict on state capacity, our ability to identify significant effects for only attacks by illegal groups should be interpreted as signaling a particularly acute role for this type of event, rather than showing that other events play no role.

Table 8 shows that the findings discussed above are robust to different specifications of the instruments matrix. Panel 1 shows estimates of equation (6) when we include military bases as an external instrument in our system GMM model, while panel 2 excludes military bases but includes additional lags of the endogenous and dependent variables as instruments for the differenced equation. More specifically, in panel 2 the lagged regressors of state capacity are instrumented with their own first and second lags in the differenced equation and with their first differences in the levels equation, whereas the conflict variables are instrumented with their second and third lags in the differenced equation and with the first lags of their first differences in the levels equation.

Our results for the effects of conflict are robust to the modifications introduced in Table 8, not only in terms of sign and significance, but also in terms of magnitudes. There is one interesting exception. In panel 1 of Table 8 not only kidnappings and displacement reduce tax revenue, but also massacres and conflict-related deaths have a negative and significant effect. Taking this at face-value would reinforce our interpretation that the capacity to collect taxes is most affected by phenomena that touch the population directly, as civilian deaths and massacres could also be included in this category. Using the presence of military bases as an additional instrument allows us to uncover other conflict-state

---

[25] One strategy used by illegal groups to control public resources is to promote the creation of associations that are apparently legal providers of services (in health, public works, etc), and that then underwrite contracts with local governments. The ability illegal armed groups have to pressure or capture governments makes them much more likely to win those contracts, at higher costs than normal. One of many interesting examples took place in Sucre, in the eastern coastal region. A well known paramilitary leader, Edgar Cobo, launched a cooperative that provided health services, and then promoted a partnership with several municipalities (Tolú, Coveñas, San Onofre, Palmito and Sincé). This partnership, gave the cooperative a large advantage in getting public contracts in those municipalities, which were in turn recipients of very large amounts of royalties from oil production activities.

capacity relationships that are not evident in Table 7. We do not use this as our baseline specification given doubts about the precision and coverage of data on existing military bases, and the fact that the exogeneity of the presence of military bases may be controversial ex-ante (though joint the exogeneity Hansen test supports the set of instruments). We still find the results of this specification reassuring about the interpretation that events with direct effects on civilians undermine the confidence they place on the state, and thus their willingness to pay taxes.

CONCLUSIONS

The main message of this paper is that internal conflicts are a source of destruction of state capacity, even after controlling for initial state capacity conditions and addressing potential endogeneity. However, the effect is smaller than previously estimated in the cross-country literature, mainly because the persistence of state capacity over time and the probable reverse causality between state capacity and conflict had not been properly considered. Although there is evidence of high persistence in fiscal and legal capacity measures, our estimates also show that the effect of internal conflict on state capacity is strong even in the short run. Conversely, once controlling for such persistence and endogeneity, our estimates suggest that the presence of an external conflict does not raise state capacity within countries. This last result contrasts with much of the existing literature on the relation between external war and state capacity, underscoring the role that major international wars played in the construction of the modern state. External conflict can have a positive effect on state capacity across countries, but it would be over a much longer time horizon than the one considered in the present work. In other words, wars are not a shortcut to development in modern times, at least based on the experience of a large number of countries during the last three decades or so.

Both our cross-country OLS and our within-country System GMM estimations are consistent with the hypothesis that internal conflict does matter greatly for state capacity. On average, countries and years involved in an internal conflict have less capacity to collect taxes and govern efficiently than countries and years not involved. The relationship remains

strong when analyzed within countries and across time: a country in the midst of an internal conflict will be less capable of collecting taxes and governing efficiently compared to a situation where there is no conflict. When searching for the manifestations of conflict that matter most , we find that in the particular case of Colombia conflict-related events that affects the civilian population (kidnappings and forced displacement) reduce the state's capacity to collect taxes. Probable factors behind this relationship include the deterioration of the tax administration system, the impediment to tax collectors to go about their business, and a reduction in the willingness to pay taxes of a citizenry that does not feel protected or feels that the local government has been captured by illegal groups. In turn, attacks perpetrated by illegal armed groups undermine the governments provision of public goods (in particular expenditure in roads). It is likely that in municipalities where such armed groups are present, governments are either unable or unwilling to spend in public goods.

Finally, we find that the more intense the internal conflict is, the larger in magnitude is its negative impact on state capacity. This empirical fact goes in line with the hypothesis that internal conflicts divide societies and make it more difficult for it to reach a consensus to invest in state capacity. In turn, weaker state capacity likely exacerbates conflict, generating a negative spiral. Breaking that vicious circle early on is a fundamental policy implication of this paper.

REFERENCES

Acemoglu, D., and Johnson, S. (2005). Unbundling Institutions. *Journal of Political Economy , 113* (5), 949-95.

Arellano, M., and Bond, S. (1991). Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations. *Review of Economic Studies , 58*, 277-297.

Arellano, M., and Bover, O. (1995). Another Look at the Instrumental variables Estimation of Error Components Models. *Journal of Econometrics , 68*, 29-51.

Baunsgaard, T., and Keen, M. (2010). Tax Revenue and (or?) Trade Liberalization. *Journal of Public Economics , 94* (9-10), 563-577.

Beseley, T., and Persson, T. (2011). *Pillars of Prosperity: The Political Economics of Development Clusters.* mimeo.

Beseley, T., and Persson, T. (2009). The Origins of State Capacity: Property Rights, Taxation, and Politics. *American Economic Review , 99* (4), 1218-44.

Beseley, T., and Persson, T. (2008). Wars and State Capacity. *Journal of the European Economic Association , 6* (2-3), 522-30.

Blattman, C., and Miguel, E. (2009). Civil War. *Journal of Economic Literature* , forthcoming.

Blundell, R., and Bond, S. (1998). Initial Conditions and Moment Restrictions in Dynamic Panel Data Models. *Journal of Econometrics , 87*, 11-143.

Cárdenas, M. (2010). State Capacity in Latin America. *Economía , 10* (2).

Cárdenas, M., and Tuzemen, D. (2010). Why do Governments Under-Invest in State Capacity? *Working Paper, Brookings Institution* , Washington D.C.

Centeno, M. A. (1997). Blood and Debt: War and Taxation in Nineteenth-Century Latin America. *American Journal of Sociology , 102* (6), 1565-605.

Centeno, M. A. (2002). *Blood and Debt: War and the Nation-State in Latin America.* University Park: Pennsylvania State University Press.

Cingranelli, D., and Richards, D. (1999). Measuring the Level, Pattern, and Sequence of Government Respect for Physical Integrity Rights. *International Studies Quarterly , 43* (2), 407-418.

Desch, M. (1996). War and Strong States, Peace and Weak States? *International Organization , 50* (2), 237-68.

Drazen, A., and Eslava, M. (2010). Electoral Manipulation via Voter-Friendly Spending: Theory and Evidence. *Journal of Development Economics , 92*, 39-52.

Dreher, A. (2006). Does Globalization Affect Growth? Evidence from a New Index of Globalization. *Applied Economics , 38* (10), 1091-1110.

Dreher, A., Gaston, N., and Martens, P. (2008). *Measuring Globalisation-Gauging its Consequences.* New York: Springer.

Dube, O., and Naidu, S. (2010). Bases, Bullets and Ballots: The Effect of U.S. Military Aid on Political Conflict in Colombia. *Working Paper 197* , Center for Global Development.

Gleditsch, K. (2002). Expanded Trade and GDP Data. *Journal of Conflict Resolution , 46*, 712-724.

Hall, R. E., and Jones, C. I. (1999). Why Do Some Countries Produce so Much More Output per Worker than Others? *Quarterly Journal of Economics , 114* (1), 83-116.

Knack, S., and Keefer, P. (1995). Institutions and Economic Performance: Cross-Country Tests using Alternative Institutional Measures. *Economics and Politics , VII*, 207-227.

López-Alves, F. (2000). *State Formation and Democracy in Latin America 1810-1900.* Durham: Duke University Press.

Marshall, M. G., Gurr, T. R., and Jaggers, K. (2009). Polity IV Project, Political Regime Characteristics and Transitions: Data Users' Manual. *Center for Systemic Peace* .

Stubbs, R. (1999). War and Economic Development: Expert-Oriented Industrialization in East and South East Asia. *Comparative Politics , 31* (3), 337-55.

Teorrell, J., Charron, N., Samanni, M., Soren, H., and Rothstein, B. (2009). The Quality of Government Dataset, version 17June09. University of Gothenburg: The Quality of Government Institute, http://www.qog.pol.gu.se.

Tilly, C. (1990). *Coercion, Capital and European States, AD 990-1990.* Cambridge, MA: Blackwell.

Appendix

Countries

GDP per capita: Gleditsch (2002) fills in gaps in the Penn World Tables' mark 5.6 and 6.2, by imputing missing data through the use of an alternative source (the CIA *World Fact Book*), and through extrapolation beyond available time-series. Based on this imputation technique, he first estimates GDP per capita in U.S. dollars at current year international prices and then in constant U.S. dollars at base year 2000. This last version is our measure of real GDP per capita. The data is originally available for 205 countries throughout the period 1950-2004.

Democracy score: The Polity2 version (Marshall et al., 2009) applies a simple treatment to the original Polity measure, converting instances of "standardized authority scores" (i.e., -66, -77, and -88) to conventional polity scores (i.e., within the range, -10 to +10). The change is made to facilitate time series analyses. The values have been converted according to the following rule set: -66 (cases of foreign interruption) are treated as "system missing"; -77 (cases of interregnum or anarchy) are converted to a neutral Polity score of "0"; and -88 (cases of transition) are prorated across the span of the transition. For example, if country X has a Polity score of -7 in 1957, followed by three years of -88 and, finally, a score of +5 in 1961, the change (+12) would be prorated over the intervening three years at a rate of 3 per year, so that the converted scores would be as follows: 1957 -7; 1958 -4; 1959 -1; 1960 +2; and 1961 +5. The Polity2 score thus captures the nature of the political regime on a scale ranging from -10 (hereditary monarchy) to +10 (consolidated democracy) after modifying standardized authority scores[26].

Country codes: As explained in the text, we re-codify countries in the QOG and Polity IV databases to take maximum advantage of existing historical information regarding the countries that currently exist. We explain here how we proceeded regarding these changes

---

[26] The original Combined Polity score is computed by subtracting the autocracy score from the democracy score. The Democracy score uses a 0-10 scale and combines measures of competitiveness of executive recruitment, openness of executive recruitment, constraints on the executive, and competitiveness of political participation. The Autocracy score also uses a 0-10 scale to measure the degree of restriction or suppression of competitive political participation. Its components are competitiveness of executive recruitment, openness of the executive recruitment, constraints on the executive, regulation of participation and competitiveness of political participation. For more details see Marshall et al. (2009).

in country codes, and note that they refer solely to information obtained from the QOG and Polity IV databases. The fiscal information we use in fact exists only for the countries that exist today, and in general only for the years in which those countries existed.

For data from QOG and Polity IV, when the current country is the result of the unification of several countries, the data that is used prior to the unification corresponds to the absorbing country (e.g., West Germany in the case of today's Germany or North Vietnam in the case of today's Vietnam). If the current country is the result of a division, then the historical data from the original country is used (prior to the date of creation of a new country). For example, Czech Republic and Slovakia are both assigned the value of Czechoslovakia up to 1992. Following Teorrell et al. (2009), to determine where to put the data for the year of the merger/split, we have relied on the "July 1st-principle". If the merger or split occurred *after* July 1st, the data for this year will belong to the historical country. This applies to Pakistan in 1971, Vietnam in 1975, Germany in 1990, and the USSR in 1991. For mergers/splits *before* July 1st, the data for this year is recorded as belonging to the new country. This applies to Yemen in 1990, Yugoslavia in 1992, Ethiopia in 1993, and Czechoslovakia in 1993. The only exception to this rule occurs if there are missing values on the year of the merge/split for the countries being modified. For example, there is a missing value for the USSR in the year 1991 for real GDP per capita but non-missing for Russia in the same year. In this case we keep the 1991 observation that belongs to Russia and not the USSR, even when the split between the two took place after July 1st. The motive behind this exception is to retain as much information as possible in the process of rearranging countries. Below we show a detailed description of the changes made for each individual country that was either the result of unification or of a division.

Countries that resulted from unification:

Germany: Data corresponds to West Germany up to and including the year 1990. East Germany is not considered in our dataset.

Yemen: We only have data for Yemen after unification (since 1990), and only for the case in which our legal capacity measure is the dependent variable.

Countries that resulted from a division:

Czech Republic and Slovakia: For both of these countries, data corresponds to Czechoslovakia up to and including the year 1992. For the measure of globalization we have no data for Czechoslovakia, so no pre-92 information is included regarding this variable. There is no data on fiscal capacity for Czechoslovakia, the Czech Republic or Slovakia.

Ethiopia and Eritrea: For both of these countries, data corresponds to Ethiopia up to and including the year 1992. No change was made with respect to the measure of globalization because there was no data for Ethiopia for the pre 1992 period. There is no data on fiscal capacity for Eritrea.

Fifteen ex-soviet nations (Armenia, Azerbaijan, Belarus, Estonia, Georgia, Kazakhstan, Kyrgyzstan, Latvia, Moldova, Lithuania, Russia, Tajikistan, Turkmenistan, Ukraine and Uzbekistan)[27]: For these 15 nations, data on QOG variables (conflict measures, the ICRG indicator of quality of government, real GDP per capita and the physical integrity index) corresponds to the USSR up to and including the year 1991. With respect to the measure of political globalization, there is individual information for ten of these countries for the pre-1991 period: Armenia, Azerbaijan, Belarus, Georgia, Kazakhstan, Kyrgyzstan, Moldova, Tajikistan, Turkmenistan and Uzbekistan. We use this information for these countries. For the remaining five countries we have missing values on political globalization for the pre-1991 period, because there is no data for either them or the USSR.[28] For the polity2 variable, information up to and including 1991 corresponds to the USSR for all fifteen nations. There is no data on fiscal capacity for any of the latter.

Five ex-Yugoslavian nations (Bosnia and Herzegovina, Croatia, Macedonia, Serbia and Montenegro, and Slovenia): For Bosnia and Herzegovina, Serbia and Montenegro, and Macedonia, the "July 1st principle" applies with no exception to all but one of the variables

---

[27] We consider the USSR's final dissolution date as the general date in which all 15 Republic acquired independence. This facilitates the treatment of this large group of countries. The independence date is December 26, 1991.

[28] Although this implies considering these ten countries as independent from the USSR before the year of the division, the data on political globalization is different across them throughout the pre-division period. This treatment constitutes an exception to the July 1st principle and allows us to retain more information on these countries.

from the QOG dataset (the ICRG indicator of quality of government, real GDP per capita, the conflict measures and the physical integrity index). For the first two countries, independence occurred between June, 1991 and March 1992 (*before* July 1st, 1992) and thus information corresponds to Yugoslavia until 1991. The same is true for Macedonia, which acquired independence on 8 September, 1991 (*after* July 1st). However, the exception does apply to Croatia and Slovenia regarding the variables of the ICRG indicator of quality of government and the physical integrity index because there are missing values for both countries in 1991, whereas Yugoslavia has non-missing values on this year. Considering that both countries acquired independence on June 25, 1991 (*before* July 1st), the exception implies that their information corresponds to Yugoslavia until1991. Having said this, the "July 1st principle" applies to all other available QOG variables for Slovenia and Croatia (the conflict variables and GDP per capita), which means information from Yugoslavia is only used for them up to and including 1990.

Regarding the measure of political globalization, there is individual information before1991 for Bosnia and Herzegovina, Serbia and Montenegro, and Macedonia. We use this information for these countries.[29] For the remaining two countries (Croatia and Slovenia) we have missing values on political globalization before-1991, because there is no information either for them or for Yugoslavia. In the case of the Polity IV dataset, we treat Bosnia and Herzegovina, Serbia and Montenegro, and Macedonia in exactly the same way as described above in the case of the QOG dataset. However, for Croatia and Slovenia the exception does no longer apply as before because there is no polity2 information missing for these two countries in 1991. Thus, information on polity2 for these countries corresponds to Yugoslavia only up to and including 1990.

Although the Polity IV dataset treats Serbia and Montenegro as two separate countries since 2006, it assigns the same values of the polity score to both countries, so we simply take this same value for Serbia and Montenegro, which we treat as a single country throughout.

---

[29] Information on political globalization varies across these three nations prior the separation of former Yugoslavia.

Table 1. Country Panel Descriptive Statistics

	Obs.	Number of Countries	Mean	Standard deviation	Minimum	Maximum	Percentile 25	Percentile 50	Percentile 75	Years Covered
State Capacity										
Total Tax Revenue (% of GDP)	3,552	125	20.62	10.64	0.09	53.38	12.35	18.43	26.89	1975-2006
Income Tax Revenue (% of GDP)	2,997	125	8.85	8.82	0.00	40.07	2.50	5.18	12.25	1975-2000
ICRG Indicator of Quality of Government (0-1)	3,425	147	0.55	0.23	0.04	1.00	0.39	0.53	0.67	1984-2008
Conflict										
Internal Conflict	5,068	171	0.11	0.31	0.00	1.00	0.00	0.00	0.00	1975-2004
External Conflict	5,068	171	0.02	0.15	0.00	1.00	0.00	0.00	0.00	1975-2004
Controls and Instruments										
Dummy of average polity2>3 in previous five years	5,079	161	0.40	0.49	0.00	1.00	0.00	0.00	1.00	1975-2007
Real GDP per capita	5,521	188	7,822	8,085	170.55	67,188	1,768	4,767	10,600	1975-2004
Physical Integrity Index	3,783	188	4.79	2.36	0.00	8.00	3.00	5.00	7.00	1981-2004
Political Globalization	5,823	187	50.20	24.28	1.56	98.78	31.90	48.18	68.81	1975-2006

Table 2- State capacitiy and conflict : Baseline OLS estimations

Dependent Variable	Total Tax Revenue as a % of GDP (t)			Income Tax Revenue as a % of GDP (t)			ICRG Indicator of Quality of Government (t)		
	(1975-2004)	(1975-2004)	(1975-2004)	(1975-2000)	(1975-2000)	(1975-2000)	(1984-2004)	(1984-2004)	(1984-2004)
	1	2	3	4	5	6	7	8	9
Internal Conflict (t)	-5.049***		-4.939***	-2.570***		-2.505***	-0.091***		-0.093***
	(0.378)		(0.377)	(0.319)		(0.321)	(0.009)		(0.009)
External Conflict (t)		-3.068***	-2.034***		-1.763***	-1.254**		0.021	0.038**
		(0.844)	(0.747)		(0.649)	(0.613)		(0.016)	(0.016)
Dummy of average polity2>3 in previous five years	5.100***	5.065***	5.116***	4.581***	4.566***	4.592***	0.096***	0.090***	0.096***
	(0.425)	(0.431)	(0.425)	(0.324)	(0.326)	(0.324)	(0.007)	(0.007)	(0.007)
log of GDP per capita (t)	4.284***	4.492***	4.283***	4.090***	4.193***	4.089***	0.115***	0.121***	0.115***
	(0.189)	(0.188)	(0.188)	(0.143)	(0.143)	(0.142)	(0.003)	(0.003)	(0.003)
Observations	2882	2882	2882	2515	2515	2515	2706	2706	2706
R-squared	0.426	0.407	0.427	0.515	0.507	0.515	0.569	0.551	0.570

Note: The regressions in these tables include year dummies. All equations are estimated by OLS. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3- State capacitiy and conflict: GMM estimations

	Panel 1: Baseline estimations								
Dependent Variable	Total Tax Revenue as a % of GDP (t)			Income Tax Revenue as a % of GDP (t)			ICRG Indicator of Quality of Government (t)		
	(1981-2004)	(1976-2004)	(1981-2004)	(1981-2000)	(1976-2000)	(1981-2000)	(1985-2004)	(1985-2004)	(1985-2004)
	1	2	3	4	5	6	7	8	9
Internal Conflict (t)	-1.454**		-0.924**	-0.985**		-0.527	-0.032***		-0.029***
	(0.614)		(0.455)	(0.436)		(0.345)	(0.010)		(0.009)
External Conflict (t)		-0.144	0.042		0.128	0.276		0.007	0.011
		(0.293)	(0.305)		(0.128)	(0.188)		(0.005)	(0.009)
Dependent variable (t-1)	0.912***	0.940***	0.958***	0.913***	0.973***	0.966***	0.918***	0.924***	0.923***
	(0.032)	(0.023)	(0.019)	(0.035)	(0.019)	(0.021)	(0.027)	(0.024)	(0.023)
Dummy of average polity2>3 in previous five years	0.550***	0.461***	0.358***	0.355**	0.194**	0.167	-0.000	-0.001	-0.000
	(0.206)	(0.133)	(0.116)	(0.164)	(0.097)	(0.108)	(0.004)	(0.003)	(0.003)
log of GDP per capita (t)	0.336**	0.243**	0.137	0.357**	0.118	0.141	0.009***	0.010***	0.009***
	(0.157)	(0.123)	(0.091)	(0.158)	(0.086)	(0.088)	(0.003)	(0.003)	(0.003)
AR(2) test	[0.202]	[0.178]	[0.206]	[0.282]	[0.174]	[0.285]	[0.000]	[0.000]	[0.000]
Hansen J test	[0.657]	[0.836]	[1.000]	[0.911]	[0.679]	[0.988]	[0.002]	[0.003]	[0.223]
Observations	2329	2777	2329	1964	2412	1964	2487	2481	2410
	Panel 2: Robustness exercises								
Dependent Variable	Total Tax Revenue as a % of GDP (t)			Income Tax Revenue as a % of GDP (t)			ICRG Indicator of Quality of Government (t)		
	(1981-2004)	(1977-2004)	(1981-2004)	(1981-2000)	(1977-2004)	(1981-2000)	(1987-2004)	(1987-2004)	(1987-2004)
	1	2	3	4	5	6	7	8	9
Internal Conflict (t)	-0.883***		-0.739**	-0.562**		-0.354	-0.029***		-0.027***
	(0.319)		(0.292)	(0.271)		(0.235)	(0.009)		(0.008)
External Conflict (t)		-0.032	0.152		0.006	0.103		0.007	0.012
		(0.269)	(0.304)		(0.176)	(0.150)		(0.006)	(0.010)
Dependent variable (t-1)	0.894***	0.944***	0.907***	0.965***	1.006***	0.981***	1.210***	1.222***	1.190***
	(0.056)	(0.062)	(0.048)	(0.072)	(0.061)	(0.066)	(0.068)	(0.069)	(0.064)
Dependent variable (t-2)	0.005	-0.029	-0.000	-0.012	-0.042	-0.017	-0.338***	-0.334***	-0.316***
	(0.060)	(0.068)	(0.056)	(0.070)	(0.068)	(0.069)	(0.078)	(0.080)	(0.073)
Dependent variable (t-3)	0.060*	0.057**	0.065**	-0.011	0.003	0.003	0.051**	0.050**	0.049**
	(0.032)	(0.029)	(0.032)	(0.044)	(0.044)	(0.046)	(0.024)	(0.023)	(0.022)
Dummy of average polity2>3 in previous five years	0.370***	0.277***	0.311***	0.265**	0.183	0.173	0.001	-0.001	0.000
	(0.124)	(0.099)	(0.104)	(0.121)	(0.131)	(0.114)	(0.003)	(0.002)	(0.003)
log of GDP per capita (t)	0.130	0.123	0.076	0.240***	0.157	0.137	0.009***	0.009***	0.009***
	(0.102)	(0.080)	(0.081)	(0.093)	(0.106)	(0.089)	(0.002)	(0.002)	(0.002)
AR(2) test	[0.017]	[0.010]	[0.005]	[0.172]	[0.013]	[0.101]	[0.341]	[0.525]	[0.284]
Hansen J test	[1.000]	[1.000]	[1.000]	[0.999]	[1.000]	[1.000]	[0.196]	[0.278]	[0.730]
Observations	2298	2568	2298	1936	2205	1936	2199	2203	2142

Note: All equations are estimated using the Arellano & Bover (1995)/Blundell & Bond (1998) one step system GMM estimator. In panel 1, the lagged state capacity variables are always instrumented with their own first lags in the differenced equation and with their first differences in the levels equation; in panel 2, they are always instrumented with their own second lags in the differenced equation and with lag 1 of their first differences in the levels equation. In panels 1 and 2, the conflict variables are always instrumented with their own second lags in the differenced equation and with the first lags of their first differences in the levels equation; a physical integrity index and a measure of political globalization are always used as additional instruments for internal and external conflict, respectively; the polity2 and log of GDP per capita variables are always considered exogenous. Columns 1, 3, 4 and 6 are restricted to a period beginning in 1981 because the physical integrity index used as an additional instrument for internal conflict shows data from that year on. All regressions include year dummies. P-values are reported for the AR(2) test and the Hansen J test. Constant not reported. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Conflict intensity descriptive statistics

	Obs.	Number of Countries	Mean	Standard deviation	Minimum	Maximum	Percentile 25	50	75	Years Covered
Internal Conflict										
Minor Armed Conflict	5,068	171	0.05	0.21	0.00	1.00	0.00	0.00	0.00	1975-2004
Intermediate Armed Conflict	5,068	171	0.06	0.23	0.00	1.00	0.00	0.00	0.00	1975-2004
War	5,068	171	0.05	0.22	0.00	1.00	0.00	0.00	0.00	1975-2004
Exernal Conflict										
Minor Armed Conflict	5,068	171	0.01	0.09	0.00	1.00	0.00	0.00	0.00	1975-2004
Intermediate Armed Conflict	5,068	171	0.01	0.09	0.00	1.00	0.00	0.00	0.00	1975-2004
War	5,068	171	0.02	0.12	0.00	1.00	0.00	0.00	0.00	1975-2004

Table 5- State capacitiy and conflict intensity

Panel 1: OLS estimations

Dependent Variable	Total Tax Revenue as a % of GDP (t)			Income Tax Revenue as a % of GDP (t)			ICRG Indicator of Quality of Government (t)		
	Internal Conflict	External Conflict	Internal and External Conflict	Internal Conflict	External Conflict	Internal and External Conflict	Internal Conflict	External Conflict	Internal and External Conflict
	(1975-2004)	(1975-2004)	(1975-2004)	(1975-2000)	(1975-2000)	(1975-2000)	(1984-2004)	(1984-2004)	(1984-2004)
	1	2	3	4	5	6	7	8	9
Minor-level Internal Conflict (t)	-2.961***		-2.914***	-1.031***		-0.997**	-0.008		-0.009
	(0.528)		(0.532)	(0.390)		(0.391)	(0.013)		(0.013)
Intermediate-level Internal Conflict (t)	-4.776***		-4.669***	-2.120***		-2.040***	-0.077***		-0.079***
	(0.501)		(0.501)	(0.400)		(0.401)	(0.011)		(0.011)
War-level Internal Conflict (t)	-5.835***		-5.731***	-3.273***		-3.223***	-0.112***		-0.113***
	(0.521)		(0.516)	(0.478)		(0.477)	(0.013)		(0.012)
Minor-level External Conflict (t)		-3.111**			-0.665			0.062	
		(1.361)			(1.109)			(0.048)	
Intermediate-level External Conflict (t)		-4.396***			-3.193***			0.049***	
		(0.991)			(0.684)			(0.017)	
War-level External Conflict (t)		-2.142*			-0.882			0.001	
		(1.253)			(0.945)			(0.025)	
External Conflict (Intermediate + War) (t)			-1.908**			-1.237**			0.037**
			(0.756)			(0.616)			(0.016)
Dummy of average polity2>3 in previous five years	5.158***	5.080***	5.172***	4.629***	4.594***	4.639***	0.096***	0.090***	0.096***
	(0.425)	(0.432)	(0.425)	(0.324)	(0.328)	(0.324)	(0.007)	(0.007)	(0.007)
log of GDP per capita (t)	4.182***	4.466***	4.183***	4.046***	4.171***	4.045***	0.114***	0.121***	0.114***
	(0.189)	(0.189)	(0.188)	(0.142)	(0.144)	(0.142)	(0.003)	(0.003)	(0.003)
Observations	2882	2882	2882	2515	2515	2515	2706	2706	2706
R-squared	0.429	0.408	0.430	0.516	0.508	0.516	0.570	0.552	0.570

Panel 2: GMM estimations

Dependent Variable	Total Tax Revenue as a % of GDP (t)			Income Tax Revenue as a % of GDP (t)			ICRG Indicator of Quality of Government (t)		
	Internal Conflict	External Conflict	Internal and External Conflict	Internal Conflict	External Conflict	Internal and External Conflict	Internal Conflict	External Conflict	Internal and External Conflict
	(1981-2004)	(1976-2004)	(1981-2004)	(1981-2000)	(1976-2000)	(1981-2000)	(1985-2004)	(1985-2004)	(1985-2004)
	1	2	3	4	5	6	7	8	9
Minor-level Internal Conflict (t)	-0.821**		-0.414*	-0.355		-0.116	-0.007		-0.009
	(0.329)		(0.235)	(0.220)		(0.152)	(0.007)		(0.007)
Intermediate-level Internal Conflict (t)	-0.837***		-0.602**	-0.473**		-0.271*	-0.013		-0.014**
	(0.320)		(0.237)	(0.191)		(0.160)	(0.008)		(0.007)
War-level Internal Conflict (t)	-1.054**		-0.629*	-0.870**		-0.464	-0.041***		-0.034***
	(0.497)		(0.367)	(0.383)		(0.287)	(0.014)		(0.012)
Minor-level External Conflict (t)		0.849			0.167			0.014	
		(0.572)			(0.368)			(0.024)	
Intermediate-level External Conflict (t)		-0.563***			-0.160			0.004	
		(0.154)			(0.109)			(0.004)	
War-level External Conflict (t)		0.111			0.257			-0.004	
		(0.432)			(0.197)			(0.010)	
External Conflict (Intermediate + War) (t)			0.016			0.194			0.011
			(0.259)			(0.144)			(0.009)
Dependent variable (t-1)	0.917***	0.944***	0.957***	0.915***	0.975***	0.963***	0.937***	0.927***	0.930***
	(0.025)	(0.022)	(0.015)	(0.028)	(0.018)	(0.018)	(0.023)	(0.021)	(0.020)
Dummy of average polity2>3 in previous five years	0.528***	0.458***	0.362***	0.351**	0.193**	0.183*	-0.002	-0.001	-0.002
	(0.174)	(0.123)	(0.103)	(0.139)	(0.093)	(0.098)	(0.003)	(0.003)	(0.003)
log of GDP per capita (t)	0.307**	0.222*	0.138*	0.346***	0.106	0.158**	0.007***	0.010***	0.008***
	(0.125)	(0.114)	(0.075)	(0.127)	(0.080)	(0.076)	(0.003)	(0.003)	(0.003)
AR(2) test	[0.217]	[0.170]	[0.215]	[0.279]	[0.172]	[0.282]	[0.000]	[0.000]	[0.000]
Hansen J test	[1.000]	[0.998]	[1.000]	[1.000]	[0.992]	[1.000]	[0.955]	[0.196]	[1.000]
Observations	2329	2777	2329	1964	2412	1964	2487	2481	2410

Note: All equations are estimated using the Arellano & Bover (1995)/Blundell & Bond (1998) one step system GMM estimator. The lagged state capacity variables are always instrumented with their own first lags in the differenced equation and with their first differences in the levels equation. The conflict variables are always instrumented with their own second lags in the differenced equation and with the first lags of their first differences in the levels equation. A physical integrity index and a measure of political globalization are always used as additional instruments for internal and external conflict, respectively. The polity2 and log of GDP per capita variables are always considered exogenous. Columns 1, 3, 4 and 6 of panel 2 are restricted to a period beginning in 1981 because the physical integrity index used as an additional instrument for internal conflict shows data from that year on. All regressions include year dummies. P-values are reported for the AR(2) test and the Hansen J test. Constant not reported. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Municipal Panel Descriptive Statistics

	Obs.	Number of municipalities	Mean	Standard deviation	Minimum	Maximum	Percentile 25	Percentile 50	Percentile 75	Years Covered
State Capacity										
Tax revenue as a % of total revenue	15,937	1,074	12.02	11.88	0.00	100	3.87	8.09	16.20	1984-2002
Expenditure in roads as a % of total expenditure	8,147	1,069	10.17	7.48	0.00	82.21	4.95	8.47	13.42	1990-2002
Conflict										
Attacks by guerrilla and paramilitaries	17,627	1,104	1.13	3.60	0.00	76.00	0.00	0.00	1.00	1993-2008
Massacres	17,627	1,104	0.03	0.24	0.00	8.00	0.00	0.00	0.00	1993-2008
Confrontations	17,627	1,104	0.07	0.55	0.00	20.00	0.00	0.00	0.00	1993-2008
Kidnappings per 100,000 inhabitants	9,986	1,071	1.39	5.54	0.00	139.86	0.00	0.00	0.00	1993-2002
Deaths per 100,000 inhabitants	9,986	1,071	1.25	4.53	0.00	62.46	0.00	0.00	0.00	1993-2002
Forced displacement (expulsion) per 1,000 inhabitants	5,552	1,030	8.61	34.52	0.00	896.20	0.00	0.69	4.04	1997-2009
Controls and Instruments										
GDP per capita in constant 1994 pesos (department)*	23,623	1,101	1,506,641	632,529	445,014	7,482,864	1,079,564	1,443,580	1,864,334	1984-2005
Population	19,350	1,071	35,362	201,288	576.00	6,712,247	7,201	12,696	23,753	1984-2002
Royalties in constant 1998 thousand pesos	6,639	511	611,910	2,465,434	0.00	39,454,256	0.00	332.23	82,880	1994-2006
Cash transfers in constant 1998 thousand pesos	12,938	1,098	3,407,081	20,594,548	4,817	806,538,752	1,067,259	1,596,347	2,447,068	1994-2005
Presence of military bases	28,921	1,126	0.02	0.15	0.00	1.00	0.00	0.00	0.00	1984-2009

*Common to all municipalities in the same department

Table 7- State capacity and conflict : municipality panel estimations, annual data 1994-2002

	Panel 1: OLS		Panel 2: GMM	
	1	2	3	4
Dependent Variable	log(Tax revenue as a % of total revenue)	log(Expenditure in roads as a % of total expenditure)	log(Tax revenue as a % of total revenue)	log(Expenditure in roads as a % of total expenditure)
Total attacks (guerrilla and paramilitary)	-0.020***	-0.003	0.006	-0.037***
	(0.006)	(0.005)	(0.006)	(0.014)
Dependent variable (t-1)	-	-	0.329***	0.150***
			(0.088)	(0.044)
AR(2) test	-	-	[0.612]	[0.620]
Hansen J test	-	-	[0.792]	[0.711]
Observations	3072	2709	2579	2013
R-squared	0.306	0.045	-	-
Massacres	-0.090**	-0.075	-0.131	-0.249
	(0.042)	(0.062)	(0.116)	(0.202)
Dependent variable (t-1)	-	-	0.329***	0.154***
			(0.086)	(0.044)
AR(2) test	-	-	[0.675]	[0.622]
Hansen J test	-	-	[0.856]	[0.460]
Observations	3072	2709	2579	2013
R-squared	0.303	0.045	-	-
Military confrontations	-0.054**	-0.042	-0.007	-0.171
	(0.022)	(0.032)	(0.019)	(0.114)
Dependent variable (t-1)	-	-	0.323***	0.154***
			(0.087)	(0.044)
AR(2) test	-	-	[0.642]	[0.684]
Hansen J test	-	-	[0.555]	[0.596]
Observations	3072	2709	2579	2013
R-squared	0.304	0.043	-	-
Kidnappings per 100,000 inhabitants	-0.012***	-0.010**	-0.026**	-0.018
	(0.004)	(0.005)	(0.013)	(0.015)
Dependent variable (t-1)	-	-	0.315***	0.148***
			(0.088)	(0.044)
AR(2) test	-	-	[0.571]	[0.561]
Hansen J test	-	-	[0.421]	[0.641]
Observations	3072	2709	2579	2013
R-squared	0.305	0.048	-	-
Deaths per 100,000 inhabitants	-0.024***	-0.001	-0.016	-0.037
	(0.004)	(0.003)	(0.011)	(0.027)
Dependent variable (t-1)	-	-	0.342***	0.146***
			(0.086)	(0.045)
AR(2) test	-	-	[0.570]	[0.566]
Hansen J test	-	-	[0.365]	[0.085]
Observations	3072	2709	2579	2013
R-squared	0.312	0.045	-	-
Expulsion per 1,000 inhabitants [1]	-0.003***	-0.001**	-0.008***	-0.003
	(0.001)	(0.000)	(0.003)	(0.004)
Dependent variable (t-1)	-	-	0.227	0.122**
			(0.147)	(0.056)
AR(2) test	-	-	[0.142]	[0.951]
Hansen J test	-	-	[0.675]	[0.466]
Observations	1777	1644	1355	1190
R-squared	0.341	0.044	-	-

Note: This table reports regressions of state capacity on individual measures of conflict, the log of the department's real GDP per capita to which the corresponding municipality belongs, the log of population, the level of royalties, the level of cash transfers and time dummies. Equations in panel 2 are estimated using the Arellano & Bover (1995)/Blundell & Bond (1998) one step system GMM estimator. The lagged regressors are always instrumented with their own first lags in the differenced equation and with their first differences in the levels equation, whereas the conflict variables are always instrumented with their own second lags in the differenced equation and with the first lags of their first differences in the levels equation. The log of GDP per capita, the log of population, the level of royalties and the level of cash transfers are always considered exogenous. P-values are reported for the AR(2) test and the Hansen J test. Constant not reported. Robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1. [1] 1997-2002

Dependent Variable	Panel 1: GMM plus military bases as an additional instrument		Panel 2: GMM instrumenting with second and third lags	
	1	2	3	4
	log(Tax revenue as a % of total revenue)	log(Expenditure in roads as a % of total expenditure)	log(Tax revenue as a % of total revenue)	log(Expenditure in roads as a % of total expenditure)
Total attacks (guerrilla and paramilitary)	0.008	-0.037***	0.003	-0.036***
	(0.006)	(0.014)	(0.006)	(0.014)
Dependent variable (t-1)	0.382***	0.150***	0.355***	0.144***
	(0.087)	(0.044)	(0.083)	(0.042)
AR(2) test	[0.520]	[0.618]	[0.581]	[0.616]
Hansen J test	[0.625]	[0.746]	[0.368]	[0.530]
Observations	2579	2013	2579	2013
Massacres	-0.213*	-0.251	-0.124	-0.262
	(0.120)	(0.199)	(0.116)	(0.203)
Dependent variable (t-1)	0.378***	0.154***	0.351***	0.146***
	(0.085)	(0.044)	(0.083)	(0.042)
AR(2) test	[0.611]	[0.621]	[0.638]	[0.607]
Hansen J test	[0.742]	[0.500]	[0.483]	[0.640]
Observations	2579	2013	2579	2013
Military confrontations	-0.008	-0.171	-0.011	-0.158
	(0.018)	(0.114)	(0.018)	(0.109)
Dependent variable (t-1)	0.376***	0.154***	0.344***	0.149***
	(0.086)	(0.044)	(0.083)	(0.042)
AR(2) test	[0.549]	[0.683]	[0.618]	[0.672]
Hansen J test	[0.363]	[0.652]	[0.394]	[0.689]
Observations	2579	2013	2579	2013
Kidnappings per 100,000 inhabitants	-0.027**	-0.018	-0.019*	-0.015
	(0.013)	(0.015)	(0.011)	(0.016)
Dependent variable (t-1)	0.365***	0.147***	0.345***	0.139***
	(0.088)	(0.044)	(0.082)	(0.041)
AR(2) test	[0.483]	[0.560]	[0.552]	[0.554]
Hansen J test	[0.244]	[0.681]	[0.437]	[0.719]
Observations	2579	2013	2579	2013
Deaths per 100,000 inhabitants	-0.020*	-0.037	-0.016	-0.038
	(0.011)	(0.027)	(0.011)	(0.026)
Dependent variable (t-1)	0.389***	0.146***	0.367***	0.136***
	(0.085)	(0.045)	(0.083)	(0.042)
AR(2) test	[0.493]	[0.566]	[0.541]	[0.549]
Hansen J test	[0.284]	[0.105]	[0.448]	[0.135]
Observations	2579	2013	2579	2013
Expulsion per 1,000 inhabitants [1/]	-0.008***	-0.003	-0.006**	-0.005
	(0.003)	(0.004)	(0.003)	(0.004)
Dependent variable (t-1)	0.267*	0.122**	0.226*	0.148**
	(0.147)	(0.056)	(0.128)	(0.060)
AR(2) test	[0.150]	[0.951]	[0.115]	[0.830]
Hansen J test	[0.620]	[0.525]	[0.243]	[0.536]
Observations	1355	1190	1355	1190

Note: This table reports regressions of state capacity on individual measures of conflict, the log of the department's real GDP per capita to which the corresponding municipality belongs, the log of population, the level of royalties, the level of cash transfers and time dummies. All equations are estimated using the Arellano & Bover (1995)/Blundell & Bond (1998) one step system GMM estimator. In panel 1, the lagged regressors of state capacity are instrumented with their own fist lags in the differenced equation and with their first differences in the levels equation, whereas the conflict variables are instrumented with their own second lags in the differenced equation and with the first lags of their first differences in the levels equation; a dummy variable indicating the presence of one or more military bases in a municipality is included as an additional instrument. In panel 2, the lagged regressors of state capacity are instrumented with their own first and second lags in the differenced equation and with their first differences in the levels equation, whereas the conflict variables are instrumented with their own second and third lags in the differenced equation and with the first lags of their first differences in the levels equation. The log of GDP per capita, the log of population, the level of royalties and the level of cash transfers are always considered exogenous. P-values are reported for the AR(2) test and the Hansen J test. Constant not reported. Robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1. [1/] 1997-2002.